Online Appendix

The Effects of Working while in School:

Evidence from Employment Lotteries

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The online appendix is divided in six sections from A to F. In Section A, we include extra figures and tables mainly testing the robustness of our results to different specifications. In Section B, we replicate the main figures and tables in the paper restricting the sample to Edition 1 of the program, which we observe up to 4 years after the program. In Section C, we provide additional institutional details of the YET work-study program and the Uruguayan education system. In Section D, we explore selection into applying to the program by comparing youth who apply to the program with the eligible population of youth in Uruguay. In Section E, we provide further empirical evidence using our survey data. Finally, in Section F, we explain in detail how we compute the share of summer jobs over total employment while in school, in the US and in Uruguay.

A   Extra Figures and Tables

Table A1: YET edition by edition

Edition	1	2	3	4	5
Application Date	May 2012	May 2013	May 2014	Sep 2015	Sep 2016
Applications	46,544	43,661	31,990	21,159	27,143
Applicants	46,008	42,643	30,969	20,537	26,137
Job Offers Made	754	981	955	722	843
Jobs Started	592	754	718	614	652
Jobs Completed	549	686	660	540	615
Sector: Civil	0.82	0.73	0.70	0.64	0.62
Sector: Industry/Trade	0.02	0.04	0.04	0.04	0.04
Sector: Banking	0.16	0.23	0.26	0.32	0.34
Localities	51	64	67	65	63

Source: YET Program Administrative Data. There is a downward trend in applications over time, probably due to the program spending more resources in advertising in the first two editions, and due to longer lottery registration time windows in the first two editions. However, we do not see any notable trend in applicants' characteristics over time (see Appendix D).

Table A2: 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)
Fstat	9,401	2,818	3,305	3,302
Observations	90,423	36,181	30,410	23,832

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 A3: Effect of YET on labor outcomes - no controls

	(1) Total earnings	(2) Months with earnings	(3) Positive earnings	(4) Wages
Program year				
Year 0	1845.36	6.82	0.56	-34.04
	(39.96)	(0.08)	(0.01)	(2.81)
	[1022.81]	[2.79]	[0.44]	[329.44]
Post-Program years				
Year 1	64.65	-0.03	0.04	2.78
	(74.60)	(0.12)	(0.01)	(7.39)
	[1997.69]	[4.41]	[0.59]	[404.96]
Year 2	222.16	0.04	0.02	25.64
	(99.51)	(0.13)	(0.01)	(8.98)
	[2985.54]	[5.40]	[0.65]	[497.64]
Observations	90423	90423	90423	59743

Source: Administrative data.
Notes: Replicates Table 2 without including control variables.

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

	(1) Total earnings	(2) Months with earnings	(3) Positive earnings	(4) Wages
Program year				
Year 0	1863.91	6.85	0.56	-23.47
	(171.53)	(0.36)	(0.04)	(7.57)
	[1004.26]	[2.76]	[0.44]	[318.87]
Post-Program years				
Year 1	86.08	-0.01	0.05	7.13
	(72.63)	(0.12)	(0.01)	(4.98)
	[1976.26]	[4.38]	[0.59]	[400.61]
Year 2	242.47	0.06	0.02	28.65
	(62.88)	(0.09)	(0.01)	(6.80)
	[2965.23]	[5.38]	[0.65]	[494.64]
Observations	90423	90423	90423	59743

Source: Administrative data.
Notes: Replicates Table 2, but clustering the standard errors at the locality level.

Table A5: Effect of YET on labor outcomes - no winsorizing

	(1) Total earnings	(2) Months with earnings	(3) Positive earnings	(4) Wages
Program year				
Year 0	1861.33	6.85	0.56	-24.27
	(38.27)	(0.08)	(0.01)	(2.94)
	[1016.66]	[2.76]	[0.44]	[320.49]
Post-Program years				
Year 1	102.79	-0.01	0.05	9.35
	(75.14)	(0.12)	(0.01)	(7.59)
	[1990.57]	[4.39]	[0.59]	[402.49]
Year 2	271.29	0.06	0.02	32.11
	(101.53)	(0.13)	(0.01)	(9.33)
	[2987.91]	[5.38]	[0.65]	[497.57]
Observations	90423	90423	90423	59743

Source: Administrative data.

Notes: Replicates Table 2, without winsorizing the dependent variables used in Column (1) and Column (4).

Table A6: Effect of YET on labor outcomes - ITT effects

	(1) Total earnings	(2) Months with earnings	(3) Positive earnings	(4) Wages
Year 0	1442.06	5.30	0.44	-20.15
	(31.72)	(0.08)	(0.01)	(2.40)
	[1143.87]	[3.10]	[0.47]	[327.55]
Year 1	66.60	-0.00	0.04	5.49
	(55.55)	(0.09)	(0.01)	(5.49)
	[2129.01]	[4.62]	[0.61]	[410.69]
Year 2	187.60	0.05	0.02	22.05
	(74.71)	(0.10)	(0.01)	(6.65)
	[3065.88]	[5.47]	[0.66]	[501.88]
Observations	90423	90423	90423	59743

Source: Administrative data.
Notes: Replicates Table 2, but presents ITT effects rather than ToT effects. Control means are presented in brackets.

Table A7: Effect of YET on enrollment in education - no controls

	(1) Any Level	(2) Secondary Programs	(3) University	(4) Tertiary Non-Univ.	(5) Out-of-school Education
Program year					
Year 0	0.131	0.100	0.026	0.007	0.002
	(0.010)	(0.012)	(0.010)	(0.004)	(0.004)
	[0.727]	[0.503]	[0.196]	[0.017]	[0.023]
Post-Program years					
Year 1	0.041	0.028	0.018	0.003	-0.005
	(0.012)	(0.012)	(0.011)	(0.004)	(0.002)
	[0.603]	[0.323]	[0.258]	[0.022]	[0.015]
Year 2	0.044	0.022	0.015	0.008	0.000
	(0.012)	(0.011)	(0.010)	(0.004)	(0.002)
	[0.448]	[0.227]	[0.199]	[0.025]	[0.007]
Observations	90423	90423	90423	90423	90423

Notes: Replicates Table 4 without including control variables.

Table A8: Effect of YET on enrollment in education - ITT effects

	(1) Any Level	(2) Secondary Programs	(3) University	(4) Tertiary Non-Univ.	(5) Out-of-school Education
Year 0	0.097	0.079	0.014	0.005	0.002
	(0.007)	(0.008)	(0.006)	(0.003)	(0.003)
	[0.694]	[0.464]	[0.206]	[0.018]	[0.020]
Year 1	0.028	0.023	0.009	0.002	-0.004
	(0.009)	(0.009)	(0.007)	(0.003)	(0.002)
	[0.573]	[0.293]	[0.258]	[0.024]	[0.013]
Year 2	0.032	0.018	0.007	0.006	0.000
	(0.009)	(0.008)	(0.007)	(0.003)	(0.002)
	[0.447]	[0.208]	[0.217]	[0.025]	[0.008]
Observations	90423	90423	90423	90423	90423

Notes: Replicates Table 4, but presents ITT effects rather than ToT effects. Control means are presented in brackets.

Table A9: Effect of YET on study effort during the program year (Year 0)
Controlling for school grades in previous year

	(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	0.032	-1.51	-2.12	-0.003
	(0.033)	(0.041)	(0.74)	(1.04)	(0.10)
CCM	0.44	0.25	26.6	6.65	7.55
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) and school grades in previous year. **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 A10: Effect of YET by baseline household vulnerability

	(1) Total Earnings	(2) Enrolled Any level	(3) Total Earnings	(4) Enrolled Any Level
	Year 0		Year 2	
Treated (T)	1791.08	0.11	206.98	0.02
	(43.62)	(0.01)	(114.91)	(0.01)
T * Vulnerable	269.17	0.06	131.57	0.07
	(80.78)	(0.02)	(209.30)	(0.03)
Vulnerable	417.39	-0.11	-120.42	-0.17
	(192.44)	(0.04)	(324.87)	(0.04)
CCM No Vulnerable	1068.84	0.74	3142.59	0.49
Observations	90,423	90,423	90,423	90,423

Source: Administrative data.

Notes: two stage least squares regressions where we instrument the YET participation dummy and the interaction with Vulnerable dummy with a job offer dummy and the corresponding interaction. 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. **Enrolled Any Level**: Enrolled in any level of public education. **Total earnings**: total labor income over 12 months, winsorized at the top 1 percent of positive values and converted into U.S. dollars. **Vulnerable**: dummy for being in a household receiving a cash transfer (26% of the sample) the month before the program. **CCM**: control complier mean of the dependent variable among those who are not vulnerable.

Table A11: Effects during the program: expected returns to education

	(1)	(2)	(3)	(4)
	Expected probability (in%) of finding a job when one finishes...			
	3 years of high school	6 years of high school	tertiary education	university
Treated	-2.15	3.09	1.11	-0.39
	(1.39)	(1.41)	(1.13)	(0.87)
CCM	42.7	70.6	84.7	93.9
Applicants	1,272	1,272	1,272	1,272

Source: Survey.
Note: IV estimates of Eq. (1). The dependent variable is the answer to the following survey question: "What is the probability of finding a job when one finishes...?" Controls for lottery design are included. Covariates include school shift dummies (either morning or afternoon shifts). Robust standard errors shown in parentheses.

Table A12: Effect of working and studying during program year

	(1) Total Earns.	(2) Pos. Earns.	(3) Wages	(4) Enrolled Any Level
	Year 2			
Work and Study	423.09	0.04	56.91	0.07
	(167.69)	(0.02)	(17.16)	(0.02)
CCM	2318.25	0.57	465.72	0.46
Observations	90,423	90,423	59,743	90,423

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 is the same among those offered and not offered a YET job.

Table A13: Main Effects selecting one application at random (treated edition)

	(1) Total Earns.	(2) Pos. Earns.	(3) Wages	(4) Enrolled Any Level
			Year 2	
Treated	212.38	0.02	26.01	0.04
	(93.30)	(0.01)	(8.28)	(0.01)
CCM	2990.25	0.66	496.84	0.45
Observations	90,423	90,423	59,708	90,423

Notes: This table replicates our main results for Year 2 using a different procedure to a select a unique application for each candidate. We select one application at random among all applications for participants in the control group, and among the applications in a treated edition for participants in the treated group.

Table A14: Main Effects using multiple applications

	(1) Total Earns.	(2) Pos. Earns.	(3) Wages	(4) Enrolled Any Level
			Year 2	
Treated	189.94	0.02	24.67	0.03
	(101.46)	(0.01)	(9.32)	(0.01)
CCM	3039.74	0.66	499.71	0.47
Observations	122,195	122,195	81,297	122,195

Notes: This table replicates our main results for Year 2 keeping all applications submitted for each individual and clustering standard errors at the applicant level.

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

	(1) Year 0	(2) Year 1	(3) Year 2
Earnings	1826.77 (40.45)	99.32 (86.10)	278.86 (117.59)
Enrolled Any Level	0.131 (0.011)	0.036 (0.014)	0.044 (0.014)
Observations	84230	84230	84230

Notes: This table presents the DREO estimator of de Chaisemartin and Behaghel (2020). The DREO accounts for potential bias due to larger shares of compliers in the offer group of randomized waiting-list designs. The Earnings results compare well to Column (1) of Table 2, the Enrollment results to Column (1) of Table 4.

Table A16: Soft skills and earnings in the control group

	(1)
	Total income (monthly dollars)
Open	30.25
	(18.64)
Conscientious	29.62
	(18.43)
Extraversion	13.90
	(12.14)
Agreeableness	2.582
	(17.67)
Neurotic	-15.94
	(10.68)
Grit	-2.609
	(18.38)
Finishes on time	-7.444
	(13.93)
Adapts fast	20.56
	(13.17)
Teamwork important	-6.122
	(15.06)
Punctual	-18.67
	(11.67)
Observations	632
R-squared	0.029
mean of depvar	122.8
sd of depvar	201.6

Source: Survey.
Note: OLS regression of monthly earnings on soft skills measures in the control group. Robust standard errors shown in parentheses.

Table A17: Effect of YET on earnings by aggregate sector

	(1) Total earnings Industry	(2) Total earnings Civil	(3) Total earnings Banking	(4) Total earnings Low Qual.
Program year				
Year 0	-511.32	1816.89	599.31	-41.33
	(31.92)	(32.00)	(27.64)	(5.23)
	[821.60]	[107.53]	[24.45]	[51.27]
Post-Program years				
Year 1	61.98	22.34	40.69	-43.09
	(65.39)	(34.24)	(18.49)	(11.66)
	[1639.07]	[196.94]	[38.50]	[93.57]
Year 2	124.37	75.17	74.38	-16.88
	(87.49)	(51.44)	(30.01)	(19.75)
	[2489.82]	[262.81]	[55.12]	[117.71]
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. In Column (1), the dependent variable is earnings in firms belonging to the Industry/Trade sector. Columns (2) to (4) are resp. for the Public Sector (excluding public employees in public industries or banks), the Banking sector, and for Low-qualification jobs (construction, domestic workers and rural workers). Controls for lottery design (lottery and quota dummies) are included. Covariates include gender, a dummy for age 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 and converted into U.S. dollars. Robust standard errors shown in parenthesis and control complier means in brackets.

Table A18: Effect of sector of program job on earnings

	(1) Total earnings Year 0	(2) Total earnings Year 2	(3) Enrolled Any level Year 0	(4) Enrolled Any level Year 2
Program job in Banking	476.92 (54.92)	315.03 (228.12)	0.02 (0.02)	0.01 (0.03)
Program job in Industry	209.02 (185.33)	-48.60 (567.27)	0.03 (0.03)	0.02 (0.06)
Control Mean (Civil Sec.)	2861.55	3138.07	0.87	0.50
Observations	2,061	2,061	2,061	2,061

Notes: OLS regressions of earnings and enrollment in education on the sector of the program job. The sample is restricted to treated participants and the omitted reference category is the civil sector, which include all state-owned companies that are not in banking or industry. Controls for lottery design (lottery and quota dummies) are included. Covariates include gender, a dummy for age 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.

Table A19: Treatment Effect Heterogeneity by Gender

	(1) Total Earnings	(2) Enrolled Any level	(3) Total Earnings	(4) Enrolled Any Level
	Year 0		Year 2	
Treated (T)	1694.18	0.14	275.26	0.05
	(62.58)	(0.02)	(175.32)	(0.02)
T * Female	277.10	-0.02	-53.53	-0.01
	(77.05)	(0.02)	(207.41)	(0.03)
Female	-262.82	0.01	-788.48	0.02
	(12.54)	(0.00)	(25.03)	(0.00)
p-value T+T*Female=0	0.00	0.00	0.05	0.02
Observations	90,423	90,423	90,423	90,423

Source: Administrative data.

Notes: two stage least squares regressions where we instrument the YET participation dummy, and 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).

Figure A1: Treatment Effect Heterogeneity by Baseline Education and by Age

(a) Academic High School

(b) Technical High School


(c) University
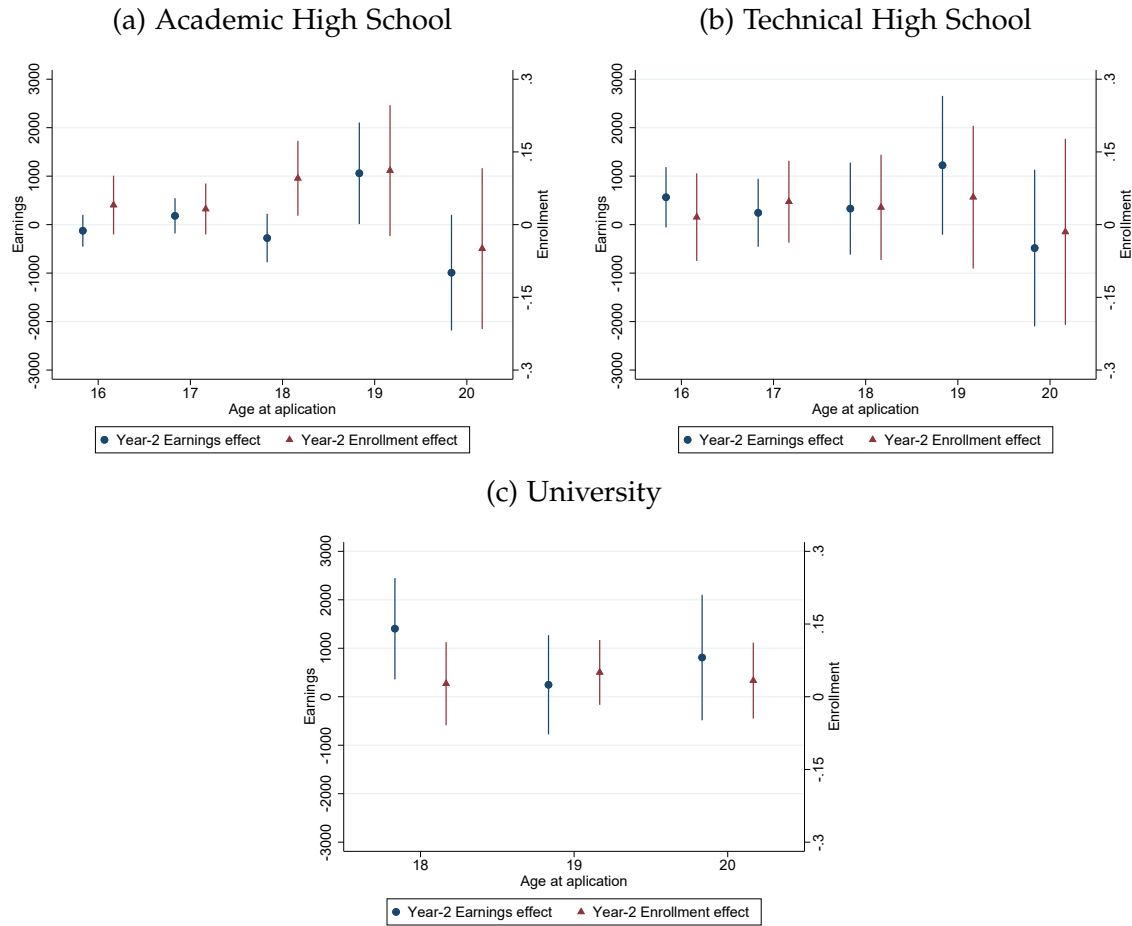
Source: Administrative data.

Note: This figure shows treatment effects of the work-study program by education level and by age, both at application date. Panel A1a restricts the estimation to students enrolled in academic high schools at the application date. Panel A1b to those enrolled in technical high schools. Panel A1c to university students. Within each education group, we estimate treatment effects on earnings two years after the program (circles, left-hand axis), and on enrollment in any education institution (triangles, right-hand axis). They are obtained by two stage least squares regressions of Equation (1), where we have further interacted the treatment dummy with age at application. Vertical lines represent the 95% confidence interval.

B   Results for Edition 1 only

Figure B1: Quarterly earnings. Edition 1
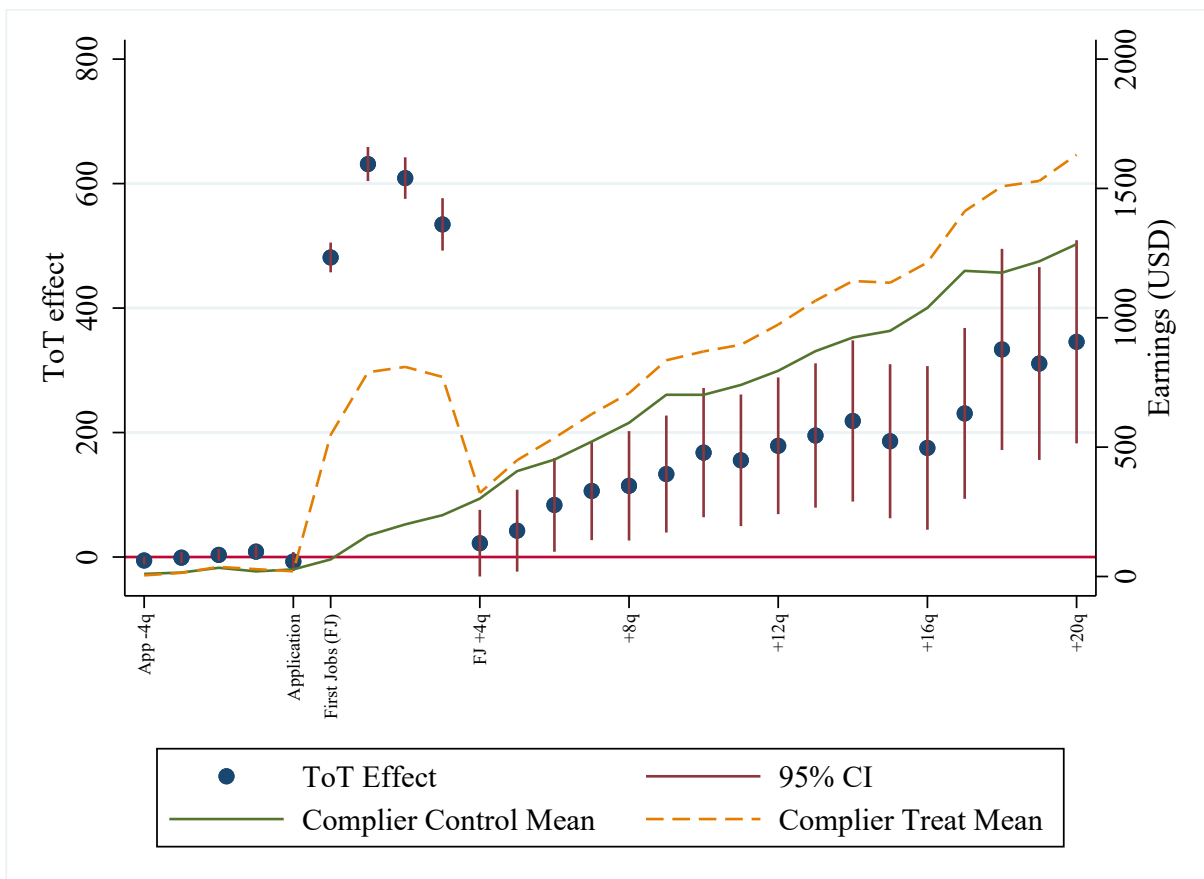
Note: This figure replicates Figure 1, but restricts the sample to the first cohort of applicants to the program.

Table B1: Balance checks - Edition 1, unique application, first application

	(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	0.26
Aged 16-18	0.70	0.46	0.71	0.45	0.44
Aged 19-20	0.30	0.46	0.29	0.45	0.44
Montevideo (Capital City)	0.52	0.50	0.58	0.49	.
Panel B. Education and Social Programs Year -1					
Enrolled in Academic Secondary Education	0.49	0.50	0.47	0.50	0.48
Enrolled in Technical Secondary Education	0.20	0.40	0.22	0.42	0.16
Enrolled in Tertiary Non-University	0.01	0.11	0.01	0.11	0.72
Enrolled in Out-of-School Programs	0.02	0.14	0.02	0.13	0.26
Highly Vulnerable HH (Food Card Recipient)	0.09	0.29	0.08	0.27	0.46
Vulnerable Household (CCT recipient)	0.25	0.43	0.25	0.43	0.69
Panel C. Labor Outcomes Year -1					
Earnings (winsorized top 1%, USD)	168.24	566.46	151.02	512.61	0.26
Positive Earnings	0.16	0.37	0.18	0.38	0.44
Months with Positive Earnings	0.75	2.19	0.67	1.93	0.14
Panel D. Aggregate orthogonality test for panels A-C					
p-value (joint F-test)					0.04
Observation	45,254		754		46,008

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 submitted. 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. **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 submitted (coefficients not included in the F-test).

Table B2: Effect of YET on labor outcomes. Edition 1

	(1) Total earnings	(2) Months with earnings	(3) Positive earnings	(4) Wages
Program year				
Year 0	1768.63	6.88	0.55	-6.79
	(57.98)	(0.13)	(0.01)	(4.30)
	[893.75]	[2.79]	[0.45]	[278.93]
Post-Program years				
Year 1	256.81	0.10	0.07	26.77
	(126.14)	(0.21)	(0.02)	(11.57)
	[1977.17]	[4.89]	[0.63]	[360.56]
Year 2	505.56	0.13	0.02	58.49
	(174.51)	(0.23)	(0.02)	(14.63)
	[2955.81]	[5.95]	[0.69]	[451.30]
Year 3	625.61	0.22	0.01	65.06
	(215.54)	(0.24)	(0.02)	(17.72)
	[3825.41]	[6.39]	[0.72]	[543.31]
Year 4	1050.59	0.49	0.05	71.01
	(264.50)	(0.23)	(0.02)	(21.63)
	[4945.20]	[6.98]	[0.75]	[657.28]
Observations	46008	46008	46008	34090

Source: Administrative data.
Notes: This table replicates Table 2, but restricts the sample to the first cohort of applicants to the program.

Table B3: Bounds for the ITT effects on wages (post-program years) Edition 1

	(1) ITT effect on wages	(2) (3) Lee bounds on wage effects		(4) Imbens and Manski 95% Confidence Interval	
		Lower	Upper	Lower	Upper
Year 1	21.32 (9.20) [379.19]	-13.84 (7.66)	46.35 (8.84)	-26.44	60.90
Year 2	45.93 (11.50) [467.29]	45.93 (11.50)	54.11 (11.42)	27.01	72.89
Year 3	51.68 (14.05) [566.58]	51.68 (14.05)	51.68 (14.05)	28.57	74.80
Year 4	56.68 (17.26) [682.32]	6.93 (14.37)	80.98 (17.15)	-16.71	109.19

Notes: This table replicates Table 3, but restricts the sample to the first cohort of applicants to the program.

Table B4: Effect of YET on enrollment in education. Edition 1

	(1) Any Level	(2) Secondary Programs	(3) University	(4) Tertiary Non-Univ.	(5) Out-of-school Education
Program year					
Year 0	0.087	0.066	0.022	0.000	-0.004
	(0.019)	(0.019)	(0.013)	(0.006)	(0.007)
	[0.724]	[0.508]	[0.181]	[0.022]	[0.028]
Post-Program years					
Year 1	0.009	0.024	-0.005	0.005	-0.013
	(0.022)	(0.021)	(0.016)	(0.008)	(0.004)
	[0.599]	[0.324]	[0.245]	[0.027]	[0.021]
Year 2	0.012	0.023	-0.007	-0.002	-0.006
	(0.022)	(0.019)	(0.017)	(0.007)	(0.004)
	[0.511]	[0.219]	[0.266]	[0.030]	[0.013]
Year 3	-0.000	-0.003	-0.000	-0.005	0.007
	(0.022)	(0.017)	(0.018)	(0.007)	(0.005)
	[0.482]	[0.185]	[0.274]	[0.031]	[0.005]
Year 4	-0.005	0.002	-0.006	-0.007	0.008
	(0.019)	(0.016)	(0.009)	(0.007)	(0.005)
	[0.226]	[0.152]	[0.045]	[0.029]	[0.003]
Observations	46008	46008	46008	46008	46008

Notes: This table replicates Table 4, but restricts the sample to the first cohort of applicants to the program.

Table B5: Effect of YET on working and studying. Edition 1

	(1) Work and Study	(2) Work No Study	(3) No Work and Study	(4) No Work No Study
Program year				
Year 0	0.52	0.04	-0.43	-0.12
	(0.02)	(0.02)	(0.01)	(0.01)
	[0.30]	[0.15]	[0.43]	[0.12]
Post-Program years				
Year 1	0.05	0.01	-0.04	-0.02
	(0.02)	(0.02)	(0.02)	(0.01)
	[0.35]	[0.28]	[0.25]	[0.12]
Year 2	0.03	-0.01	-0.02	-0.00
	(0.02)	(0.02)	(0.02)	(0.02)
	[0.34]	[0.35]	[0.17]	[0.14]
Year 3	0.01	0.00	-0.01	-0.00
	(0.02)	(0.02)	(0.02)	(0.02)
	[0.34]	[0.38]	[0.14]	[0.14]
Year 4	-0.00	0.05	-0.00	-0.05
	(0.02)	(0.02)	(0.01)	(0.02)
	[0.17]	[0.57]	[0.05]	[0.20]
Observations	46008	46008	46008	46008

Notes: This table replicates Table 6, but restricts the sample to the first cohort of applicants to the program. For 2017 we do not have the data on taking two exams, and therefore, the mean of university registration is underestimated (this applies to year 4).

C Institutional details

C.A The Uruguayan work-study program

The work-study program *"Yo Estudio y Trabajo"* (YET) offers positions in 77 localities, which include almost all the main cities in Uruguay. According to the 2011 Census, Uruguay has a population of 3.3 million divided in 19 departments and 298 localities, with around 60 localities with more than 5,000 inhabitants classified as cities (Instituto Nacional de Estadistica Uruguay, 2011).

Program applications are completed online or using a computer at an employment center and, if selected, applicants must show proof of enrollment from an educational institution certifying a minimum level of attendance (240 hours), an official identification card and the electoral card if older than eighteen. Upon selection, the no formal employment requirement is cross-validated with social security data and proof of enrollment is required every three months. Upon enrollment, students aged 16-17 receive information about how to obtain work permits.

The program remuneration is fixed at four times the minimum tax unit used in Uruguay, which means 13,360 pesos per month for a 30-hour-per-week job in January 2016. Pregnant women and mothers of kids below the age of 4, who represent around 4% of the lottery applicants, are entitled to wages that are 50% higher.

Students are allowed to re-apply from one edition to the next according to the following rules. Those who start a program job are not allowed to participate in a later edition, while those who do not start one are allowed to apply again without receiving any priority.

C.B Educational system in Uruguay

Since 2008, general secondary education is compulsory for youth aged 12-17 years old. It encompasses six years of instruction, divided into two three-year cycles. The second cycle is aimed at youth aged 15-17 years old and has a course load from 34 to 36 weekly hours. Gross enrollment rates in 2015 were 96% for the first cycle and 82% for the second cycle, while completion rates were below 50%, with very high repetition rates (Source: "Anuarios Estadísticos de Educación del Ministerio de Educación y Cultura y Departamento de Estadística.") There are two possible

tracks: the academic track, which is in general regarded as more prestigious, and the technical track. Regarding higher education, there are no tuition fees at the State university.

D Program Applicants vs Youth Population

Table D1 describes selection into program application using public data from the 2011 Uruguayan Population Census and from the 2013 wave of the continuous household survey (Instituto Nacional de Estadistica Uruguay, 2011, 2013). The Population Census conducted in Uruguay in 2011 registered 255,338 youth aged 16 to 20 (Column 1). Only 132,968 (54%) of them were attending school (Column 2). If we consider this number as the population eligible to participate in the program, then we have an application rate of 34.6 percent in the 2012 edition of the program. Two caveats are in order with this estimate. First, candidates could register into school in 2012 in order to apply to the program, which means that we overestimate the application rate. Second, some students in Column (2) worked formally for more than 90 days, which would lead us to underestimate the application rate. The second bias is probably moderate though, as only 7 percent of youth attending school earned positive income in a formal job (contributing to social security). In Column (3), we report the characteristics of the population of applicants - as declared on their application forms - to the 2012 edition.

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

Column (4) presents the characteristics of the average applicants across the first three editions of the program, our main sample, we see a slight increase in the share of women, and younger teenagers in comparison to the first edition, but overall the composition of applicants does not vary much over time and it is not very different from that of the general population of this age.

Table D1: Characteristics of youth in Uruguay

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

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

E   Survey results for the program year of Edition 5

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

Table E1 shows that, among survey respondents, the control group and the group of youth receiving a program job offer are overall balanced on baseline characteristics.

Table E2 reports the effect of being offered a program job on employment, educational enrollment and total income. This table draws the big picture of the treatment group situation around the end of the program. Overall the estimates are in line with the evidence from administrative data at the same horizon. By the end of the program, the treatment group still experiences a significant increase in employment rates by 48 p.p out of a mean of 27 percent in the control group. The enrollment rate in education is also significantly higher in the treatment group by 9 p.p. (while 3 out of 4 youth are enrolled in education in the control group). Beyond marginal distributions, we obtain a significant increase in the share of students working and studying, the main objective of the program. Conversely, the program decreases the share of young youth who are neither in employment, education, or training (NEETs) by 12 p.p., which represents 63 percent of the mean for compliers in the control group. Column (5) reports the treatment effect on total monthly income converted in dollars at the exchange rate at the time of the survey. Treated students earn \$142 more on average, which means that the program more than doubles the monthly income of youth.

Table E3 presents treatment effects on whether students are studying in public or private institutions. Conditionally on being enrolled, there are no effects on the type of schools students are enrolled at the end of the program year.
Tables E4 to E6 describe the employment experiences of program applicants: their employers, their jobs and their tasks, respectively. The estimation samples are restricted to employed youth, so results can be affected by selection and should be interpreted as descriptive evidence. Consistent with the program description above and with its objectives, employment is almost exclusively formal in the treatment

Table E1: Balance check - respondents to the survey of the 5th edition

	(1)	(2)	(3)	(4)	(5)
	Control		Offered		
	Mean	sd	Mean	sd	p^+
Observations	632		640		
p-value F test*					0.35
Panel A. Demographics					
Female	0.64	0.48	0.62	0.49	0.35
Age	17.72	1.41	17.80	1.42	0.42
Number of kids	0.03	0.17	0.02	0.16	0.53
Father completed high school	0.28	0.45	0.31	0.46	0.30
Mother completed high school	0.41	0.49	0.41	0.49	0.76
More than 10 books at home	0.48	0.50	0.49	0.50	0.49
Panel B. Education and Social Programs					
School: hours per day	5.49	1.65	5.47	1.45	0.70
School: morning shift	0.42	0.49	0.48	0.50	0.10
School: afternoon shift	0.42	0.49	0.37	0.48	0.06
School: evening shift	0.16	0.37	0.15	0.35	0.72
School: Secondary Academic	0.61	0.49	0.54	0.50	0.04
School: Secondary Technical	0.25	0.43	0.27	0.44	0.50
School: Non-Formal Education	0.01	0.12	0.03	0.16	0.62
School: Teacher's College	0.01	0.09	0.02	0.13	0.19
School: Tertiary	0.01	0.10	0.03	0.16	0.01
School: University	0.11	0.31	0.13	0.33	0.52
Enrolled the year before the program (Sec or Tert.)	0.93	0.25	0.94	0.23	0.37
Repeated grade once in primary school	0.13	0.33	0.14	0.35	0.87
Household Receives Cash Transfer	0.19	0.39	0.16	0.36	0.33
Household Recipient of Food Card	0.12	0.33	0.11	0.31	0.32

Source: Survey and administrative data on applications.

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

Table E2: Effects during the program: employment and education status.

	(1) Employed	(2) Study	(3) Work & Study	(4) NEET	(5) Tot. income month, $
Treated	0.472	0.084	0.435	-0.121	140.5
	(0.031)	(0.028)	(0.030)	(0.023)	(13.64)
CCM	0.269	0.748	0.207	0.190	123.4
Applicants	1,272	1,272	1,272	1,272	1,272

Source: Survey.
Note: IV estimates of Eq. (1). Controls for lottery design are included. Covariates include school shift dummies (either morning or afternoon shifts). Robust standard errors shown in parentheses. CCM: Control Complier Mean.

Table E3: Effects during the program: public vs private education.

	(1) Study	(2) Public School Any Level
Treated	0.084	-0.005
	(0.028)	(0.014)
CCM	0.748	0.956
Applicants	1,272	996

Source: Survey.
Note: IV estimates 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. CCM: Control Complier Mean.

group, while almost one third of the control group is employed in informal jobs (defined as those that do not contribute to social security). Column (2) of Table E4 shows that 94% of treated teenagers report being employed in the public sector, while in the control group less than 1 out of 5 applicants are working in that sector. This is consistent with the information in the program website on the list of employers. Survey respondents in the treatment group report that their main employers are: the National Bank (22%), the state-owned electricity company (19%), the state-owned telephone company (9%) and the state-owned oil and gas company (6%). These four largest employers hire 56% of the treatment group. Similarly, treated employees are significantly more likely to work in larger firms (larger than 50 employees), in the manufacturing industry, in the financial services and public services (industry classification in the survey is more detailed than in the administrative data). In a nutshell, the program crowds out small, informal employers from the retail trade industry, which is the main employer type in the control group.

Table E4: Effects during the program: employers type

	(1) Formal	(2) Public Employer	(3) Small firm < 50	(4) Manuf.	(5) Retail Trade	(6) Fin. services	(7) Public services
Treated	0.284	0.769	-0.409	0.189	-0.406	0.360	0.077
	(0.040)	(0.037)	(0.048)	(0.034)	(0.045)	(0.032)	(0.038)
CCM	0.690	0.176	0.621	0.101	0.437	0.000	0.127
Applicants	587	587	577	587	587	587	587

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

Table E5 shows that treated youth are more satisfied with their job. We see a statistically significant increase by almost two thirds of a standard deviation in a job satisfaction index. Column (2) of Table E5 also shows that the share of part-time work (less than 29 hours per week) is significantly higher in the treatment group. This translates into a lower total monthly wage. More importantly, (log) hourly wages paid to treated students are significantly higher than those paid to control group workers.

Table E5: Effects during the program: jobs type

	(1) Job satisf. (scale 1-5)	(2) Part-time work < 29 hours	(3) Total wages month, dollars	(4) Hourly wage log, dollars
Treated	0.638	0.321	-44.46	0.173
	(0.105)	(0.051)	(18.55)	(0.057)
CCM	3.664	0.350	360.5	2.311
Control sd	1.067	0.477	213	0.672
Applicants	587	587	587	573

Source: Survey Data.
Note: IV estimates 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.

In Table E6, we describe the occupations and tasks performed by employed youth. Consistently with the industries of the program employers, treated youth are much more likely to work as clerks: 93 percent of treated youth are clerks compared to 42 percent in the control group. Consequently, treated youth are much more likely to read, write and use computers on a daily basis in the workplace (Columns 2 to 4). Treated youth are less likely to measure weights or distances during their workday (Column 5). They report that their work is less physically demanding (Column 6): we see a decrease in half a standard deviation in an index capturing how physically demanding the job is.[33] Surprisingly, treated employees declare that they have less frequent interactions with their colleagues, this could be due to the fact that they work in larger firms. Although their job is closer to office work, they might be less likely to work in teams (Column 7).

Table E8 yields unique information on how the increase in working time due to the program crowds out other activities. The program increases youth weekly working time by almost 11 hours. Hours worked in the treatment group are more than double those in the control group.[34] We do not find evidence of work crowding

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[33]Table E7 provides further details on the job tasks: treated youth read more pages and are less likely to carry heavy loads.

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

Table E6: Effects during the program: occupation & tasks

	(1) Clerical occupation	(2) Reading	(3) Writing	(4) Computers every day	(5) Measuring weights,dist.	(6) Physically demand. (scale 1-10)	(7) Freq. meetings colleagues
Treated	0.511	0.220	0.121	0.448	-0.128	-1.482	-0.171
	(0.045)	(0.048)	(0.049)	(0.048)	(0.043)	(0.274)	(0.050)
CCM	0.421	0.608	0.608	0.403	0.253	4.372	0.365
Control sd	0.487	0.499	0.495	0.486	0.450	2.789	0.489
Applicants	587	587	587	587	587	587	587

Source: Survey.
Note: IV estimates of Eq. (1). Controls for lottery design are included. Covariates include school shift dummies (either morning or afternoon shifts). Robust standard errors shown in parentheses.

Table E7: Effects during the program: more details on tasks of employed youths

	(1) Pages read	(2) Pages written	(3) Carry > 25 kg
Treated	2.459	0.552	-0.147
	(1.334)	(0.619)	(0.041)
CCM	5.922	1.521	0.236
Control sd	11.77	4.614	0.444
Applicants	587	587	587

Source: Survey.
Note: IV estimates of Eq. (1). Controls for lottery design are included. Covariates include school shift dummies (either morning or afternoon shifts). Robust standard errors shown in parentheses.

out or crowding in study time. The positive effect of the program on enrollment and the negative effect on study hours conditional on being enrolled cancel each other out. The main result in Table E8 is that wage employment crowds out both home production (Column 4) and leisure time (Column 5).[35] Leisure time decreases by 14 percent and time dedicated to household chores decreases by 50 percent.[36]

Table E8: Effects during the program: time use

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Time (hours per week)			
	Working	Studying in or out of school	Commuting	Household chores	Leisure	Sleeping	Eating
Treated	10.34	-2.046	2.002	-2.805	-4.499	-0.258	-1.530
	(1.421)	(1.571)	(0.900)	(0.665)	(1.766)	(1.293)	(0.741)
CCM	9.511	20.19	5.867	5.998	33.47	58.64	10.70
Indiv.	1,272	1,272	1,272	1,272	1,272	1,272	1,272

Source: Survey.
Note: IV estimates of Eq. (1). Controls for lottery design are included. The time-use survey questions are daily, we convert answers into weekly measures. Covariates include school shift dummies (either morning or afternoon shifts). Robust standard errors shown in parentheses.

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

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

F Summer jobs vs. employment while in school

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

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

We use aggregate statistics from the 2017 Current Population Survey (U.S. Bureau of Labor Statistics, 2017). From Table A-16 published in the website of the Bureau of Labor and Statistics,[37] we compute the employment rate of teenagers (16-19) enrolled in school, excluding summer months (June-July-August), and we obtain a share of 23%. The employment rate of enrolled teenagers remains stable over the summer months, probably because of a composition effect: the enrollment rate during the summer drops from 83% to 52%. As teenagers enrolled during the year who take summer jobs probably declare themselves as non-enrolled over the summer, we need to correct our estimates of summer employment for teenagers who regularly attend school. We then assume that the entire summer increase in jobs held by teenagers who report themselves as non-enrolled over the summer is due to teenagers enrolled in non-summer months. A priori, this yields an upper bound estimate of the employment rate of the enrolled population, which then amounts to 31%. Summer employment then contributes to 31% of yearly employment ($= 0.31/(0.31 + 3*0.23)$). This number is that reported in the introduction.

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

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[37]Not seasonally adjusted, Table A-16: Employment status of the civilian noninstitutional population 16 to 24 years of age by school enrollment, age, sex, race, Hispanic or Latino ethnicity, and educational attainment

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

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