I Z A Institute
of Labor Economics
Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 14661

The Impact of Short-Term Employment for Low-Income Youth: Experimental Evidence from the Philippines

Emily A. Beam
Stella Quimbo

AUGUST 2021

I Z A Institute
of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 14661

The Impact of Short-Term Employment for Low-Income Youth: Experimental Evidence from the Philippines

Emily A. Beam
University of Vermont and IZA

Stella Quimbo
University of the Philippines

AUGUST 2021

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

The Impact of Short-Term Employment for Low-Income Youth: Experimental Evidence from the Philippines*

We use a randomized field experiment to test the causal impact of short-term work experience on employment and school enrollment among disadvantaged, in-school youth in the Philippines. This experience leads to a 4.4 percentage point (79-percent) increase in employment 8 to 12 months later. While we find no aggregate increase in enrollment, we also do not find that the employment gains push youth out of school. Our results are most consistent with work experience serving as a signal of unobservable applicant quality, and these findings highlight the role of temporary work as a stepping- stone to employment for low-income youth.

JEL Classification: J24, J08, O15

Keywords: short-term employment, work experience, ALMP, experiment

Corresponding author:
Emily A. Beam
Department of Economics
University of Vermont
233 Old Mill
Burlington, VT 05405
USA
E-mail: emily.beam@uvm.edu

* We thank the Institute of Labor Studies and Department of Labor and Employment staff who have made this study possible. We are especially thankful to the members of the SPES Technical Working Group for their guidance and support. Funding for this project was provided by the International Initiative for Impact Evaluation (3ie) and the Australian Government's Department of Foreign Affairs under the 3ie Policy Window grant programme. The Human Subjects Committee for Innovations for Poverty Action provided oversight for this project, "Youth and Student Employment (SPES)," protocol #9935. This work has benefited tremendously from feedback from Eric Edmonds, Anne Fitzpatrick, Leigh Linden, Diego Ubfal; seminar and conference participants at the Asian Development Bank, Colby College, Dartmouth College, Northeastern Economics of EducationWorkshop, Philippine Institute of Development Studies, the University of Texas at Austin, and the 2018 Jobs and Development Conference; and two anonymous referees. We thank Heather Richmond for outstanding project coordination and assistance, Jose Marie Gonzalez and Grace Labalan for outstanding project management, and the IPA-Philippines field staff for dedicated data collection. Dina Emam, Minh Phuong La, Guido Maschhaupt, and Nick Manderlink provided excellent research assistance.

1 Introduction

Youth worldwide often face a slow and bumpy transition to employment. Labor market frictions, weak labor demand, and skill mismatch can make finding quality, stable employment after leaving school challenging, particularly in low-income countries (Matsumoto and Elder, 2010; Quintini and Martin, 2014). The stakes are high; the World Bank has identified youth unemployment as one of the key barriers to growth among developing countries (World Bank, 2012). And as for youth themselves, whether and when they can find employment, and the nature of that work, can have long-reaching implications for their future earnings trajectories and quality of life.

Although the Philippines has experienced strong economic growth over the past decade, youth in the Philippine labor market experience high rates of unemployment and underemployment, along with low rates of employment in the formal sector. Approximately one in four youth ages 15–24 are idle (not enrolled, employed, or in training) (World Bank, 2016), and the youth unemployment rate is 15 percent, more than twice the rate for the general labor force (Philippine Statistics Authority, 2015). Among youth who are working, only 30 percent are in the formal sector (World Bank, 2016).

In order to help youth improve their long-term labor market prospects, the Philippine Department of Labor and Employment (DOLE) implements the Special Program for the Employment of Students (SPES), a temporary employment program for low-income youth. First launched in 1992, and expanded dramatically beginning in 2009, SPES subsidizes employment for more than 250,000 low-income youth at the high school and college levels each year. The program targets disadvantaged in-school youth: those aged 15 to 25 from low-income families who are enrolled or intend to re-enroll in secondary or post-secondary education are eligible for these primarily public-sector placements. Participants earn minimum wage salaries for 20 to 52 days of work during school breaks.[1] DOLE provides a

[1] After implementation of our study, a 2016 amendment (RA 10917) expanded the eligible age range to

40-percent wage subsidy, and local offices facilitate the recruitment and matching process. Unlike most youth employment programs in developing countries, this program primarily targets *in-school* youth, as policymakers hope that summer income will increase education by offsetting tuition, fees, and the opportunity cost of foregone wages while in school. It also provides formal work experience to a group of youth with limited opportunities. We use exogenous variation in program participation to ask, how does short-term work experience affect youth employment and education outcomes?

We conducted an oversubscription-based randomized field experiment to estimate the impact of temporary employment on medium-run employment and education outcomes among 3,281 youth in 26 municipalities across 3 regions in the Philippines. Specifically, we partnered with DOLE to randomize invitations to enroll in SPES among programs with more eligible applicants than available slots. We collected baseline data at the time of program application, and we followed up 8 to 12 months after the program in order to measure applicants' employment status.

Gaining work experience through a temporary employment program may improve employment prospects for youth in several ways. Working could increase participants' human capital by building firm-specific or general work skills, as well as by improving non-cognitive skills like self-esteem, communication, time management, and general work-readiness (Heckman et al., 2006). Second, previous work experience may signal to future employers that a worker possesses desirable but more difficult to observe traits, like motivation and persistence. Temporary, subsidized programs like SPES also reduce employers' cost of screening applicants, which could lead to longer-term positions for productive workers. These screening and signaling channels can be particularly important when there is high uncertainty about a worker's productivity and when the fixed costs of hiring or replacing a worker are high (Farber and Gibbons, 1996; Altonji and Pierret, 2001; Pallais, 2014). Finally, the program itself may promote employment by exposing participants to the labor market or increasing participants'

15–30 and extended the program length to 20–78 days of work.

aspirations (McKenzie, 2017). For example, Beam (2016) and Abebe et al. (2020b) find that job fairs affect job search behavior and increase formal sector employment despite few direct hires, and both Galasso et al. (2004) and Levinsohn et al. (2014) find that wage subsidies lead to higher employment rates despite low subsidy take-up.

We find that summer employment increases the likelihood of being employed 8 to 12 months later by 4.4 percentage points (79 percent), and this gain is concentrated exclusively among post-secondary students. This rise in employment does not come at the expense of school enrollment, and the gains are concentrated with non-SPES employers. The gain in employment is most consistent with temporary work experience serving as a signal of difficult-to-observe worker characteristics to prospective employers, such as motivation, commitment, or persistence.

We do not observe an increase in hard or soft skills, and the potential for skill acquisition is limited by the short duration of work experience, typically 20 days, plus the fairly routine nature of program work tasks. Nearly all SPES beneficiaries employed at endline work with a different employer than their summer assignment (88 percent), indicating that firm-specific skills are unlikely to drive results and that screening is not a dominant mechanism. We do not see evidence of changes in aspirations, reservation wages, or labor market perceptions, which could affect employment through different job-offer or acceptance rates. In addition, we find suggestive evidence that the program effects are larger for those without prior work experience, who we expect would have the most to gain from the program in the presence of a signaling channel.

Policymakers in the Philippines anticipate that employment gains could be greater in the long run if the program increases school enrollment and graduation rates, reflected in the legislated program goal "to help poor but deserving students pursue their education," (RA 7323, 1992). Most temporary work programs focus on youth who have already left school, but this focus is similar to that of U.S. summer employment programs for high school students and federal work-study programs. Typical program earnings through SPES are substantial; nationally, students earn Php5,000 to Php9,620 (US$105 to US$203) for the minimum 20 days of work, and 2.5 times that at the maximum. In our study, even minimum program earnings would cover 60 to 70 percent of average baseline tuition. While beneficiaries are entitled to their earnings regardless of whether they enroll in school, they must show proof of enrollment or provide a signed statement if they do not enroll in order to receive the 40-percent wage subsidy from DOLE. Although program participation increases school enrollment rates among high-school level students, limited targeting prevents the program from maximizing its potential effectiveness through education channels, as control-group enrollment rates are already high (94 percent).

This study demonstrates that summer work experience increases medium-run labor force attachment among low-income youth, but not at the expense of school enrollment. This result is particularly important given documented high unemployment rates and slow school-to-work transitions among youth in many developing countries (Matsumoto and Elder, 2010; Quintini and Martin, 2014) and high variability in the impacts of many subsidized employment programs (McKenzie, 2017; Kluve et al., 2019). These results are comparable to a small but growing literature measuring the causal impact of summer employment programs for low-income students in the United States (Heller, 2014; Leos-Urbel, 2014; Gelber et al., 2016). These programs are similar to SPES in that they target in-school youth, do not directly provide on-the-job training for specific skills or industries, and have a fairly short program duration. The U.S. summer work programs typically offer 25 hours per week of work for six to eight weeks (125–200 total hours), while the modal SPES placement offers full-time work for 20 days (approximately 160 hours). Like our study, Gelber et al. (2016) find a modest increase in the likelihood of employment.

More broadly, this paper addresses whether short-term work experience can lead to better future employment opportunities, joining a body of literature that evaluates the impact of work experience generated through wage subsidies and temporary work and internship programs. Because SPES provides relatively short-term work experience that is unlikely to lead to building general or firm-specific skills, it is well-suited for exploring other potential mechanisms through which short-term work experience promotes employment, which most prior studies are limited in their ability to address.

Wage subsidies intend to encourage employers to hire out-of-work youth, but positive direct effects have been rare, particularly in the absence of accompanying job placements. Galasso et al. (2004) and Levinsohn et al. (2014) find that subsidies induce large and persistent increases in employment in Argentina and South Africa, respectively, though in neither case is the increase due to subsidy take-up. Conversely, Groh et al. (2016) find that a wage subsidy program for female college graduates in Jordan does lead to high take-up but no change in employment after the end of the program, suggesting that workers' marginal productivity remains below the market-clearing wage.

Several studies examine programs that combine work placement with additional training, making it difficult to separate the impact of work experience alone. Card et al. (2011) and Ibarrarán et al. (2014) find that two- to three-month internships after vocational training do not increase employment, while Attanasio et al. (2011) finds a combination of classroom and on-the-job training increased employment rates after a similar program in Colombia, but only among women.

This study is most similar to three recent studies that examine the impacts of work placements only. McKenzie et al. (2016) examine a subsidized six-month internship program in Yemen offered to college and vocational graduates, which leads to large increases in employment and earnings that persist five months after the program's end. A one-month job-shadowing management program for young professionals in Ethiopia also leads to a substantial increase in wage employment and earnings (Abebe et al., 2020a). Despite different contexts and targeted participants—we work with low-income high school and college students who have not yet completed their education—we also find increases in the likelihood of employment.

Le Barbanchon et al. (2020) do consider in-school youth through an evaluation of a longer-term (9–12 months) work placement program in Uruguay, and they investigate the mechanisms driving their results. Like this study, their program increases employment, as well as earnings and enrollment, and they attribute the increase in employment to the program-induced gains in work experience rather than the increase in education. The authors argue that the length of the program and nature of tasks indicates that the increase in earnings among internship participants likely reflects human capital development, ruling out an increase in soft skills, but they do not explicitly test for other mechanisms such as signaling or learning about one's own ability.

In comparison to Le Barbanchon et al. (2020), our program differs in duration and timing, and the impacts we estimate are more modest, likely because the work experience that we study is less intensive and the program does not impose strict conditionality on enrollment. One additional contribution of our study is that we are able to examine in detail the role of potential mechanisms driving the impact of work experience, including human capital development, signaling, screening, and job-search behavior.

2 Program Background

Increasing education enrollment and completion rates has been a key policy goal in the Philippines, particularly at the secondary level. To date, its secondary completion rate has risen to 82 percent (PSA, 2018), and the tertiary enrollment rate has grown to 33 percent (World Bank, 2018a). However, these rates mask gaps in access for poorer and marginalized students (World Bank, 2018b), and substantial work remains for the Philippines to achieve its goal of universal secondary school completion by 2030.[2]

[2]Promoting access to secondary education is also somewhat complicated by recent K–12 reforms. Until 2016, the Philippines was one of a handful of countries with a 10-year basic education system. It developed

The Philippine Department of Labor and Employment (DOLE) developed the Special Program for Employment of Youth (SPES) in 1992 to provide "poor but deserving" youth ages 15 to 25 with subsidized short-term employment opportunities during school breaks. The program has been revised several times by law, and a 2009 reform mandated a 20-percent annual increase in its budget. Program enrollment has grown tremendously since then, and SPES has become one of DOLE's flagship programs. As of 2016, it had an annual budget of Php817.96 million ($17.2 million)[3] and reached 229,674 participants per year (Bureau of Local Employment, 2017). Although implementing guidelines are set at a national level, programs are administered locally, usually at the municipal level through Public Employment Service Offices (PESOs). As a result, there is substantial heterogeneity in many aspects of program implementation.

Across the country, most participants are placed in local government offices; 70 percent are employed at local government offices nationally, while private-sector employment comprises 8 percent of SPES employment (Bureau of Local Employment, 2017).[4] In our study, 94 percent work in local government and 6 percent work for private-sector employers. Because the vast majority of positions are created for the purpose of the SPES program, we do not anticipate substantial displacement effects of SPES employment itself. Most students work jobs that would not exist in the absence of the program. However, we note that in terms of post-program employment impacts, our estimates reflect partial, rather than general equilibrium effects.

Within this broad program structure, we observe three common deviations from an "ideal" version of SPES that may influence its effectiveness. First, although the prevailing law assured students 20 to 52 days of work, most participants in our study (73 percent) work

---

and launched a two-year senior high school curriculum in 2016 to meet international standards, but these additional requirements could push secondary completion further out of reach for low-income youth.

[3] This and all subsequent conversions calculated using the average 2016 exchange rate of 1 USD = 47.483 Php (X-Rates, 2017).

[4] The remainder consists of other public employment offices (7 percent) and private educational institutions (15 percent), which run their own programs. These program types are not represented in our study.

only the minimum of 20 days, particularly in the public sector. Based on qualitative surveys conducted with municipal program administrators and conversations with regional DOLE officials, we learned that local mayors, seeking to maximize the political gains of the program subject to budget constraints, often maximize the number of participants by minimizing the program length. This phenomenon was particularly pronounced because 2016 was a municipal election year, although our analysis of 2017 administrative data reveals that 20 days remain the norm for public-sector positions in off-cycle years.[5]

In municipal offices, the short duration of the program coupled with the sudden inflow of dozens of participants in one office means that participants' SPES assignments do not necessarily provide opportunities to gain new skills. For most participants placed in municipal offices, their work consists of relatively low-skilled office tasks like surveying, encoding, and filing documents.[6] Nearly 14 percent report that their primary or secondary tasks are essentially make-work tasks to "maintain the cleanliness and orderliness of the office," such as rearranging chairs and opening and closing windows. This challenge arises less among private-sector employers, and the main tasks for their participants are customer service and sales (33 percent).

We also document substantial payment delays, particularly for the 40-percent subsidy that DOLE provides. Although the law specifies that payment should be made within 30 days of the program's end, Appendix Table A2 shows that 36 percent received their DOLE counterpart 3 months or more after program completion, and another 16 percent had not received it at all by the endline interview, 8 to 12 months after the program. Delayed payments will reduce the program's effect on education if credit-constrained students already struggle to cover tuition and fees prior to enrollment.

While these particular issues may be specific to SPES, implementation challenges are com-

[5]Across study municipalities (and some non-experimental municipalities), 82 percent worked exactly 20 days in 2016 and 72 percent worked exactly 20 days in 2017.

[6]See Appendix Table A1 for more detail.

mon across a host of similar government programs and initiatives. Banerjee et al. (2017) cite implementation problems as a key reason for why many programs and interventions that show promise when tested by an outside organization and/or at a smaller scale do not have comparable effects when implemented at scale by a government institution.[7]

3 Data and Methodology

Our study sample consists of 26 municipal and provincial Public Employment Service Offices (PESOs) located in three regions in the Philippines: National Capital Region (NCR), Region III on the island of Luzon, and Region XI in Mindanao.[8] We recruited PESOs that had the largest SPES programs in their region, based on 2014 enrollment.[9] Among those we contacted, 59 percent participated in the impact evaluation.[10] The main reasons for non-participation were because the mayors declined (municipal PESO managers are appointed by their mayors and are directly accountable to them, not to the central or regional DOLE offices), or because, despite a willingness to participate, the number of eligible applicants did not exceed the number of available program slots.

One concern about the relatively low participation rate is that we may inadvertently include only those programs that are especially well run, and therefore we might expect true program effects to be lower in a fully representative sample. To investigate this possibility, we conducted a qualitative survey with program administrators in 55 offices across the three

[7] Banerjee et al. (2017) also discuss the challenges of scaling a program to provide targeted instruction in India. Bold et al. (2018) document implementation challenges stemming from bureaucratic and political opposition to providing fixed-term teacher contracts. As a result, these contracts were effective when implemented by an NGO, but they had no effect when the Kenyan government offered them.

[8] There are 22 offices represented in our study, but one provincial office implemented separate recruitment and placement batches across 14 municipalities, of which 5 municipalities participated. In effect, this creates 26 total participating programs, which were randomized in 31 batches.

[9] Specifically, we originally contacted the largest 13 PESOs per region, and if an office refused, we contacted the next randomly selected PESO in that province from a back-up list of PESOs that had at least 100 participants in 2014.

[10] Based on 26 programs participating out of 44 contacted across the three regions. If we consider the one provincial office as a single participant, then the response rate 69 percent, or 22 out of 32 offices.

regions, which includes all but one of the 26 offices in the study.

We hypothesize that programs may be better run if they have a more transparent application process, provide more comprehensive orientations, coordinate with private employers, and have less government interference. Along these lines, it seems unlikely that we are "cherry-picking" the best-run programs, although we do see some differences between the two groups. While participating programs are more likely to have a public recruitment process, they are slightly less likely to hold information sessions before applications and equally likely to deliver orientation sessions. Programs that do and do not participate have a roughly equal likelihood of working with private employers (20 percent for participating vs. 21 percent for non-participating). Additionally, participating programs are slightly more likely to report that the mayor's office is involved in the selection of participants (32 percent vs 23 percent, respectively), defined as whether the office considers government referrals in their eligibility or selection criteria). See Appendix Table A3 for a more complete set of program-level descriptive statistics.

3.1 Sample selection

Among participating employment offices, we include all youth ages 15 to 25 who applied for SPES, passed the initial screening conducted by the PESOs, and consented to participate in the study. Despite the range of eligible ages, 95 percent of all new SPES applicants were age 20 or younger. Because students graduated high school after completing grade 10, most high school applicants were ages 15 and 16, and most college-age applicants were ages 17 to 19.[11] The initial screening consists of verifying applicants' age; that they are in school with an average passing grade in the past term or school year or are an out-of-school youth intending to re-enroll in school and certified to be of "good moral character" by their barangay; and that their family total income falls below the regional poverty line for a family

[11] See Appendix Figure A1 for the distribution of applicant ages by schooling level.

of six. These requirements are widely enforced across PESOs, though some impose additional screening criteria, such as passing a home visit, providing additional documentation, passing a qualifying exam, etc. Some eligible applicants were identified as returning participants and members of other priority groups, as determined by each PESO, and we exclude them from our study. After restricting the sample to those applicants eligible for randomization in municipalities with oversubscription, we have a baseline sample of 3,795 respondents.

Compared with the full set of participants nationwide, we have comparable coverage of private-sector employers (6 percent in our sample versus 8 percent nationally), and we see that the majority of beneficiaries nationally also tend to work the minimum number of program days (73 percent in our sample versus 55 percent nationally). But there are also some differences. We cannot speak to the impact of placements in private education institutions, which comprise 15 percent of national placements. Students who are placed with these schools are typically college-level students who work for much longer than those placed in government offices.[12] As a result, our sample overrepresents relatively younger beneficiaries, and 81 percent of our sample is aged 15–18, compared with 48 percent nationally. See Appendix Table A4 for complete details. This comparison highlights that our study is best able to speak to the impacts among first-time beneficiaries, comprising nearly 90 percent of participants nationwide, who work in government and private-sector placements.

3.2 Project timeline and data collection

Figure 1 shows the timeline of the study, which took place in 2016 and 2017. During the SPES enrollment process in February and March 2016, we collected baseline data from two sources: SPES application forms that we obtained from each employment office in hard- or soft-copy form and a self-administered supplemental form that was verified by local PESO officers prior

[12] While disaggregated data on program characteristics by employer type is not available nationally, data from a selection of institutions shows that the median duration among students working at private education institutions was the program cap of 52 days.

to submission. Upon the conclusion of each recruitment period, the local PESO submitted the number of available slots along with the full list of eligible applicants to the research team, noting any prioritized applicants to be excluded from randomization (and therefore, from the study). We randomly assigned students to the available slots and returned this list to the local offices. Each employment office contacted the chosen participants to complete the enrollment process, and applicants worked between April and the beginning of the 2016–17 academic year in mid-June.

We conducted an endline phone survey from January–May 2017. At enrollment, each applicant was asked for his or her cell number, an alternate number, and the numbers of three family members and one friend. Using multiple phone numbers, we surveyed 75 percent of the baseline sample. We attained a response rate of 87 percent through more intensive follow-up efforts with the remainder, contacting the local PESO offices for updated contact information, using Facebook, or visiting them in person. We have an endline sample of 3,281 respondents.

We encountered few direct refusals (six percent of non-respondents), and the main reason for attrition was because the provided numbers were invalid, no longer in service, or out of network coverage (see Appendix Table A5). We do not find evidence of differential attrition by treatment status ($p = 0.43$ with controls and $p = 0.33$ with stratification-cell fixed effects only), and the endline sample also remains largely balanced across covariates.[13]

We have data on SPES enrollment from two sources: administrative data collected from local PESO offices and encoded, along with self-reported enrollment data collected during the endline survey. Overall, these two measures match for 89 percent of respondents in the endline sample. We prefer the self-reported measure because administrative enrollment records were not complete for a few municipalities, though our results are robust to using either data source (See Online Appendix B). All other outcomes measures come from the

[13] See Appendix Table A6 for differential attrition by treatment assignment and Appendix Table A7 for covariate balance among endline respondents.

endline survey, so our measures of education, employment, and skill development are self-reported. The close alignment between self-reported and administrative SPES enrollment data provides some reassurance that the other self-reported measures are accurately reported.

3.3 Randomization

Among participating program offices, we randomly selected applicants from the pool of qualified, first-time applicants to be invited to enroll in SPES. We necessarily stratified at the employment-office level. Within each employment office, we stratified by gender, by school level (high school or college), and by age. Treatment group members were invited to participate in SPES. Control group members were not invited, but they were permitted to apply again for the 2017 summer SPES batch. Among our baseline sample, 2,510 (66 percent) are treatment group members and 1,285 (34 percent) are control group members, and there is substantial variation in oversubscription rates by municipality.

3.4 Descriptive statistics and balance tests

Table 1 presents descriptive characteristics of our sample. Nearly two-thirds of the sample are women, slightly more skewed than the overall gender distribution (59 percent female) of participants nationally (Bureau of Local Employment, 2017). Although the program was open to youth ages 15 to 25, nearly all applicants (95 percent) are age 20 or younger, with a mean age of 17.2. In part, this low average age reflects the exclusion of returning participants; nationally, 16 percent of participants are aged 22–25 (Bureau of Local Employment, 2017).[14] Our sample is fairly evenly divided between high school and post-secondary students (primarily college level, as only one percent are enrolled in vocational training at baseline), and out-of-school youth make up only two percent of our sample.[15]

[14] See Appendix Table A4 for more details.

[15] Our baseline records are incomplete in some municipalities; for those with missing information, we impute education level based on applicant age.

Just under 20 percent of applicants have any past work experience, and few (7 percent) have any formal work experience. While many high school students attend tuition-free public schools, a substantial share of the sample does pay tuition, and average unconditional baseline expected tuition is approximately Php11,300 (US$240) for the academic year, with a mean of Php10,000 (US$211) for high school students and Php12,000 (US$253) for college students. Overall, students expect to spend an additional Php9,500 (US$200) on other educational expenses such as fees, textbooks, uniforms, transportation, and meals.

Columns 7 and 8 of Table 1 show tests for balance by treatment assignment for these baseline characteristics within each randomization cell (PESO-by-gender-by-education level). Because the treatment-group shares vary substantially across municipalities, we use stratification-cell fixed effects in all balance tests.[16] Regardless of whether we consider p-values from t-tests (column 7) or randomization inference (column 8), nearly all covariates are balanced between treatment and control groups, although there is a modest difference in whether applicants have past work experience between the two groups (randomization-inference $p = 0.064$). We cannot reject the null hypothesis that these covariates are jointly zero ($p = 0.741$).

3.5 Empirical specification

Overall, 89 percent of treatment-group and 28 percent of control-group members report enrolling in SPES. For this reason, our preferred specification is LATE estimates, instrumenting program enrollment with treatment assignment throughout our analysis. ITT effects are included in Online Appendix B. In the presence of treatment heterogeneity, these estimates may not necessarily apply to the full population, particularly those (presumably with political connections) who would have enrolled in the program regardless of randomization. However,

[16]These fixed effects, particularly the PESO-level effects, are important because otherwise PESO-level differences in covariates will be indistinguishable from covariate imbalance between PESOs. For example, municipalities with higher oversubscription rates happen to have a higher share of high school students (as we see in the data). They will disproportionately contribute high school students to the control group, creating imbalance in aggregate, but not within municipalities.

when considering program expansion to a broader pool of students, these applicants on the margin of enrollment form the most relevant group.

The minimum detectable effect size with 80 percent power is a 2.3 percentage-point increase in employment based on a measured control-group rate of 5.6 percent and an R^2 of 0.12. Because of non-compliance, the adjusted MDE is 3.7 percentage points.

We estimate the following specification using two-stage least squares:

$$spes_{i,s} = \alpha_0 + \alpha_1 treatment_{i,s} + f_s + X'b + v_{i,s}$$

$$y_{i,s} = \beta_0 + \beta_1 \widehat{spes}_{i,s} + f_s + X'b + e_{i,s}$$

where $treatment_{i,s}$ is a binary indicator for whether the respondent was randomly selected to be invited to SPES and $spes_{i,s}$ is a binary indicator for whether the respondent reports participating in SPES during the study period. As discussed earlier, we use self-reported SPES participation as our measure of $spes_{i,s}$, although the general magnitude and significance of our results are unaffected if we use administrative reports of SPES participation. Online Appendix B includes the set of results using administrative data.

We include stratification-cell fixed effects, f_s, in all specifications, along with a vector of individual-level baseline covariates, X, which include gender, age, education level, past work experience (any, formal, and informal), reservation wages, expected wages, and expected educational expenses.[17] Due to substantial non-response at baseline, we recode missing values as zeros and add missing value flags.[18] We use heteroskedasticity-robust standard errors in all specifications.

The identifying assumptions of our specification are that treatment assignment is random and that, conditional on our included covariates and fixed effects, it affects our outcomes of

[17] We include controls for gender and education level despite stratification because in small programs with few applicants, stratification on all covariates was not possible.

[18] Online Appendix B shows that results are robust to excluding these covariates, although there is some loss of precision.

interest only through SPES participation.

Column 1 of Table 2 shows that assignment to treatment increases the likelihood of self-reported enrollment in SPES by approximately 51 percentage points, providing a very strong first stage, with an F-statistic of 655. Column 2 uses administrative data on SPES enrollment, and the impact of assignment on the likelihood of enrollment is slightly larger, at 58 percent.

In addition to reporting aggregate effects, we also disaggregate treatment effects along two key dimensions that were part of our stratification strategy: gender and education level (secondary versus post-secondary). For each set of results, we include a binary indicator for being female or being at the post-secondary level, and we interact that indicator with our included covariates.[19]

4 Results

4.1 Does SPES increase work experience?

Only 5 percent of enrolled youth nationally, and 18 percent of our control group, work for pay during summer breaks, suggesting that SPES participation is likely to increase summer work experience. Table 2 demonstrates that this is, indeed, the case. Program participation increases the likelihood of any summer work by 80 percent, significant at the 1-percent

[19] Our baseline education level variable reflects reported education level according to lists of applicants used for randomization. Because education level was not reported in all applicant lists, we estimate education level based on multiple age and grade-level fields in collected individual application forms. The use of different forms and the varying level of completeness may introduce error into this measure. When considering treatment heterogeneity by education level, we prefer to divide the sample based on reported current education level among students enrolled in school, using highest completed education level among non-enrolled students. Because the 2016–17 academic year marked the roll-out of grades 11 and 12, very few students, if any, could progress from high school to post-secondary education during this time. However, exceptions could occur if students were in a K–12 pilot school or exited high school before graduation in order to enter vocational training. For this reason, we also test the robustness of our results to either excluding or reclassifying those students in the first year of college or enrolled in vocational school. Our results, available upon request, are not affected.

level, and there is no detectable change in non-SPES summer earnings (column 4). Summing administrative SPES earnings with self-reported other summer earnings shows that participation raises total summer earnings by P5650 ($US 119), which is significant at the 1-percent level.

4.2 Impact of temporary work experience on employment outcomes

Additional work experience generated by SPES could have lasting effects if students leverage their summer work experience into additional employment. Table 3 shows that SPES has a persistent effect on whether students report working for a private company, government, or non-profit organization approximately 8 to 12 months after the program start. Column 1 of Panel B shows that SPES increases the reported likelihood of working by a statistically significant 4.4 percentage points, a 79-percent increase relative to the control group employment rate of 5.6 percent. Overall, there is no impact on the likelihood of having searched for work (measured since June 2016), and we can reject at the 95-percent level any increase greater than 4.5 percent.

Across the entire sample, including the 93 percent of respondents not working for pay, we find an imprecisely estimated increase in unconditional monthly earnings ($p = 0.114$) and an increase in hours worked that is statistically significant at the 5-percent level, with $p = 0.044$. Compared to the low control-group means, these estimates at first appear large: an 81-percent increase in earnings and an 83-percent increase in hours worked. However, they are driven entirely by the change in the likelihood of work. (See Appendix Table A8.) After conditioning among those who work, both estimates are more modest (11 percent *lower* earnings and 20 percent more hours worked).

In Panel B, we interact SPES participation with gender, noting that studies of similar types of employment and training programs have found heterogeneous employment by gender (Attanasio et al., 2011; Acevedo et al., 2020). Here, we see that the employment effect is greatest for men (7.8 percentage points versus 3.2 percentage points), but that difference is not statistically significant ($p = 0.313$)

We also test for heterogeneous treatment effects by education level, as college students could have greater work opportunities because of their age and, potentially, their skills. Panel C shows that the employment impact is entirely concentrated among college-level students (10 percentage points versus 1 percentage point), and this difference is statistically significant at the 5-percent level.

While we observe no aggregate change in the likelihood respondents had looked for work in the time since the program ended (column 2), panel B shows that among high school students there is a large and statistically significant negative effect on the likelihood of having looked for work since June, after the program concluded (8.2 percentage points, compared with a control-group rate of 20 percent among high school students). This highlights contrasting impacts between the two groups: college students are more likely to be employed and are modestly more likely to have looked for work as well (an increase of 6.1 percentage points, $p = 0.290$), while high school students see no change in employment but a reduction in the likelihood of search. This may reflect the statistically significant increase in the likelihood of enrolling for high school students (see Table 5), which could make the labor market less attractive.[20]

Consistent with the large increase in the likelihood of employment among college students, average monthly earnings rise by Php440 (US$9.25), and hours worked per week rise by 3.9, significant at the 5- and 1-percent levels, respectively. If college students engage in different work tasks during the program, this difference in employment outcomes could reflect differing

[20]We also test whether this drop in job-search likelihood reflects a change in perceptions about the barriers to finding work. However, we find no evidence that SPES affects these perceptions in aggregate or for high school students in particular. (See Appendix Table A9 for results.)

returns based on the type of work experience, which would be consistent with students gaining additional skills in certain types of jobs.

There are, indeed, differences in the type of program work tasks that high school students perform relative to college students. (See Appendix Figure A2.) High school students are relatively more likely to file and organize documents and engage in manual labor like cleaning, sweeping, or planting. College students are more likely to work as surveyors or data collectors or to encode and update records. However, Appendix Table A10 shows that the type of work tasks is not associated with the likelihood of employment at endline. While work tasks are likely endogenous with student characteristics, this evidence suggests that our results are more consistent with college students being more employable in general terms, rather than because of specific experience gained through their short-term work experience.

What types of jobs are youth working at endline? Table 4 shows that conditional on employment, youth work an average of 38 hours per week and earn Php5,100 (US$107) per month. Nearly 80 percent are in the private-sector, and 23 percent are in "regular" employment, meaning that the worker has a signed contract. Among those who enrolled in SPES in the previous year, very few (12 percent) had first worked with that employer as a SPES participant. Overall, the main position types substantially differ from the public-sector positions offered through SPES: youth primarily work in sales (21 percent) and food service (20 percent), while general labor such as cleaning, construction, etc. makes up 15 percent of employment. Office or clerical work comprises only nine percent of employment.[21]

4.3 Impact of temporary work experience on education outcomes

One justification for targeting in-school youth and one of the main policy objectives of SPES is that paid summer employment may help students afford to stay in school. Additionally, program guidelines require students to present a certificate of enrollment or a signed

[21] See Appendix Table A11 for the full distribution of position types worked at endline.

statement if they do not enroll in order to receive the 40-percent subsidy after the program concludes, which could directly incentivize school enrollment. Conversely, if the program increases employment rates, it could induce students to exit school earlier (Duncan, 1965). Heller (2014) and Gelber et al. (2016) find that summer employment programs in New York City and Chicago do not affect education outcomes, while Leos-Urbel (2014) finds small, positive impacts on attendance conditional on enrollment. However, SPES does increase the post-program likelihood of employment, while these U.S. programs do not.[22]

In line with program expectations, 68 percent of SPES participants report that they used at least some of their earnings for tuition and other schooling expenses. Another 44 percent reported using earnings to support their family, while 35 percent reported buying personal effects (respondents could select multiple uses). Appendix Table A12 shows the full distribution of reported uses.

Table 5 shows the overall impact of enrolling in SPES on self-reported enrollment and grades. In aggregate, SPES increases the likelihood of enrollment by a statistically insignificant 1.1 percentage points ($p = 0.569$), a 1.2-percent increase relative to a control-group enrollment rate of 94 percent. We can reject at the 95-percent level any increase larger than 4.9 percentage points. We also asked respondents whether they intend to enroll during the 2017–2018 school year, and the results are similarly small, positive, and statistically insignificant. There is no change in average grades conditional on enrollment (column 3). Grade-weighted averages are reported in standard deviation units, which have been normalized based on education-level and scale-specific means among the control group.[23] This finding is in line with experimental research on similar programs in the United States (Gelber et al., 2016).

We do not measure the impact of the program on graduation; few applicants were in the final

[22] Le Barbanchon et al. (2020) find that a year-long employment program in Uruguay increases both enrollment and education.

[23] Most grade scales range from 1 (high) and 5 (low) or between 0 (low) and 100 (high), and we drop the few observations with scales that could not be easily converted.

year of post-secondary education, and the 2016 launch of Senior High School (implementing grades 11 and 12 for the first time) meant that there was no graduating class in nearly all high schools. However, 30 percent of the sample transitioned from junior high school (grades 7–10) to senior high school (grades 11–12) in the 2016–2017 school year immediately following SPES. At the point of grade 11 enrollment, students select one of four tracks: academic, technical-vocational-livelihood (TVL), sports, and arts and design, which determines their curriculum for their remaining two years. Nearly all students in our sample select into the academic track (53 percent) or TVL track (47 percent). SPES does appear to reduce the likelihood of enrollment in academic (college-bound) tracks, although this difference is imprecisely estimated and not statistically significant ($p = 0.124$).

We also test for heterogeneity in program impacts by gender and education level. We hypothesize that program impacts might be larger for men, who *ex ante* face slightly higher drop-out rates and therefore are more likely to be on the margin of enrollment.[24] Indeed, Panel B shows that the treatment effect is entirely concentrated among men (7.2 percentage points, $p = 0.050$), and we can reject the equality of coefficients between men and women at the 5-percent level ($p = 0.043$).

We also hypothesize that program impacts might differ by education level, although the direction is ambiguous. On the one hand, college is more expensive than secondary school, so credit constraints may be more likely to bind. On the other hand, the program primarily recruits a population that has already chosen to enroll in school, and so college-level applicants may be more able to afford their tuition and fees by virtue of selection into enrollment. Among the control group, enrollment rates are higher for college students (95 percent) than high school students (93 percent). Consistent with the second hypothesis, we do see a 4.1 percentage-point increase in enrollment among high-school level SPES participants, significant at the 5-percent level, and we can reject equality of coefficients between high-school and college-level students at the 10-percent level.

[24] At endline, 93 percent of men in the control group are enrolled, versus 95 percent of women.

4.4 Does employment push youth out of school?

Because very few respondents had completed their studies at the time of the endline survey and because hours worked conditional on employment are high (38 per week on average, Appendix Table A8), one concern is that the increase in employment could crowd out enrollment (Duncan, 1965). Table 6, however, shows that the observed increase in employment is concentrated among students who remain enrolled. In aggregate, SPES increases the likelihood of being enrolled *and* working by 3.2 percentage points (marginally statistically significant, $p = 0.079$), but it does not increase the likelihood of working but not being enrolled (95-percent confidence interval: [–0.007, 0.030]). Panels B and C show these shifts more precisely. Interacting enrollment with gender and education level shows that for men and high school students, SPES induces large, statistically significant reductions in the share of students not enrolled and not working (9.0 and 4.0 percentage-point reductions, respectively), while SPES mainly shifts college students from being enrolled and not working to being enrolled and working.

5 Discussion

5.1 Why does temporary work experience increase employment?

We consider several explanations for the increase in medium-run employment generated by summer work experience. First, additional work experience may enhance participants' firm-specific or general skills with specific work tasks, or it may increase their noncognitive skills in areas such as self-esteem, motivation, and work readiness. Second, work experience may help applicants signal their productivity to future employers. Third, the subsidized nature of SPES may provide summer employers with a low-cost way of screening workers for longer-term positions. Fourth, exposure to the formal labor market may affect workers' search effort and, consequently, their employment outcomes if they adjust their aspirations or expectations or if they build social connections to help them find work (Beam, 2016; McKenzie, 2017; Abebe et al., 2020b).

We do not find evidence of general skills acquisition or motivation channels, nor any detectable changes in job search likelihoods or methods. While some youth continue working with their SPES employer (12 percent), most find new positions, suggesting that screening is not an important factor. Rather, we see suggestive evidence of larger employment effects among youth who have no prior work experience and among those from more disadvantaged backgrounds. While not conclusive, these results are consistent with a signaling channel as a key mechanism driving our results.

We first examine whether work experience obtained through SPES increases general work skills. We asked respondents whether they have experience with 11 office tasks, which we selected to reflect the type of work typical in SPES positions and vetted with DOLE staff. Column 1 of Table 7 reports the impact of SPES on a work tasks index generated by creating indicators for whether a respondent has "some" or "a lot" of experience with that task, summing all 11 binary task indicators and then normalizing based on the control-group distribution. Overall, we see no evidence of an increase in work skills; participation leads to a 0.10 standard deviation increase in the skills index ($p = 0.229$).[25]

Columns 2 through 4 of Table 7 then measure the impact of work experience on noncognitive skills using three other measures: self-esteem, based on five items drawn from Rosenberg (1965); life skills, a seven-question index developed by the Philippine Bureau of Local Employment; and workplace skills, an extract of five questions drawn from Brea (2011) and used in Ibarrarán et al. (2014) and Acevedo et al. (2020).[26]

[25] An alternative definition based on whether respondents report they have "a lot" of experience does not affect our results. Appendix Table A13 reports impacts on individual office tasks and shows that overall, SPES only leads to an increase in the likelihood respondents have experience answering phones (a 16 percentage-point increase relative to a control-group mean of 39 percent).

[26] Appendix Table A14 includes the specific life skills and workplace skills question items.

The coefficients across these three self-reported non-cognitive measures are small and not statistically significant. This lack of soft-skills development through temporary work experience is consistent with recent literature on similar interventions. Acevedo et al. (2020) find that short-term work experience, even coupled with soft-skills training, is not enough to increase soft skills among Dominican youth. Le Barbanchon et al. (2020) find that a longer-term work experience in Uruguay does not affect soft skills.[27]

Just as we do not see evidence that SPES improves work skills or noncognitive skills in aggregate, we find no evidence of differential impacts when disaggregating by gender or by education level. The lack of apparent skills development is consistent with the fairly routine nature of the most commonly assigned work tasks (see Appendix Table A1) and the short nature of the program, which would make it difficult to acquire new skills. In the terms of the work tasks measured, a high share of control-group students reports that they have experience in most dimensions.[28]

To explore whether summer work experience might serve as a positive signal to future employers or a low-cost way to screen applicants, we consider the work trajectories of those who participated in SPES. However, just 12 percent of employed SPES recipients had worked for that employer previously through SPES, indicating that neither screening nor the development of firm-specific skills is likely to be important drivers of the observed employment effect.

We hypothesize that if the work experience obtained through SPES serves as a signal to future employers, then those without previous work experience might stand to benefit the most. We find that the increase in employment is primarily concentrated among those

[27] One exception is Gottschalk (2005), who finds that work experience induced by a randomly assigned work subsidy increases individuals' perceived locus of control.

[28] See control group means in Appendix Table A13. Among the control group, more than 80 percent have experience with Word and PowerPoint, and more than half have experience with Excel. Nearly all have experience with online searches (93 percent), photocopying (83 percent), and encoding (72 percent). The only tasks for which respondents were generally inexperienced were answering phones (39 percent) and bookkeeping (33 percent).

without previous work experience, although the difference is imprecise and not statistically significant ($p = 0.766$). Because age is correlated with the likelihood of past work experience, we also test for the differential impact of the program based on previous work experience after restricting the sample to college-age students, and the difference is even starker, though we lack sufficient power to detect statistically significant differences. See Panels B and C of Appendix Table A15 for full results.

Additionally, we hypothesize that if SPES operates through a signaling channel, the work experience it provides may also be most helpful for students from disadvantaged backgrounds, who may have fewer connections and resources to find work. We test for heterogeneity based on family socioeconomic status (see Panel D of Appendix Table A15), and we again find that students from lower-income families do benefit the most, although that difference is not statistically significant ($p = 0.254$).

We then examine the role of general labor-market exposure as a result of summer work experience on employment, looking at impacts on aspirations and job-search behavior. In Table 7, we examine whether program participation affects participants' aspirations and improves their information about the labor market. Column 5 shows that, consistent with the increase in employment seen in Table 3, they are 7 percentage points more likely to say they will likely find a job after graduation (significant at the 10-percent level), an 11-percent increase relative to the control-group mean. This effect is largest for college students (panel C), for whom we see an increase in employment. Levinsohn and Pugatch (2014) note that wage subsidies could depress employment by raising individuals' reservation wages. However, we see no statistically significant change in either respondents' reservation wages or expected wages; the estimated coefficients are large and actually negative, but imprecisely estimated ($p = 0.293$ and $p = 0.269$, respectively). Additionally, we see no change in whether they expect to finish college or get a higher degree (column 8), although aspirations are already high: 95 percent of the control group expects to complete at least a bachelor's degree.

In contrast to other work that finds labor market exposure can increase job-search effort or affect search strategies (Beam, 2016; Abebe et al., 2020b), we find no impacts on the likelihood or nature of job search. As Table 3 shows, SPES decreases the likelihood of looking for work since June 2016, the beginning of the new school year, by 2.4 percentage points relative to a control-group rate of 22 percent. However, this is imprecisely estimated and not statistically significant ($p = 0.490$). The most common ways of searching among the control group include referrals from family or friends (83 percent), submitting a resume or CV (75 percent), and visiting an employer in person (55 percent). If SPES strengthens social networks useful for job-finding, we would expect participants to rely more heavily on referrals to search. However, we see no evidence of changes in particular search methods (see Appendix Table A16). We see the largest coefficients on the likelihood of having looked for work online and used family/friend referrals, but none are statistically significantly different from zero. These results suggest that summer work experience does not increase motivation to look for work, nor that it provides participants with stronger social networks they can leverage to find work.

One final consideration is that the estimated impacts on employment may reflect displacement among control-group members, leading us to overstate program impacts. In the event of displacement, we would expect that those in areas with higher rates of oversubscription would have greater effects because the control-group would have depressed labor market outcomes. However, we find no evidence of differential employment effects among those in programs with above versus below-median oversubscription rates.[29] There is evidence of differences in unconditional wages and hours worked, but the sign is the opposite of what we would expect if treatment-group members were reducing opportunities for members of the control, as it is those in areas with lower oversubscription rates that see larger employment effects.

[29] The median oversubscription rate is 27 percent, with a range of 3 percent to 600 percent. See Panel E of Appendix Table A15 for results.

5.2 Cost-effectiveness

We conduct a set of back-of-the-envelope calculations to estimate the program cost per additional student employed and enrolled, relying on the point estimates from Tables 3 and 5. In 2016 the program served 229,674 students with a direct program budget of Php817.96 million (US$17 million) (Bureau of Local Employment, 2017), for a cost of Php3561 per student (US$75), covering the 40 percent wage subsidy plus any administrative expenses. Under a restrictive assumption of homogeneous treatment effects, this implies a cost of Php81,000 (US$1,704) per additional job found. As effects are concentrated among post-secondary students, we also estimate a program cost of Php37,100 (US$781) per post-secondary student who finds employment.

If we take seriously the estimated SPES-induced 1.1 percentage-point increase in enrollment, we similarly estimate that the program costs DOLE Php323,700 (US$6,818) per student induced to enroll an additional year. The cost per student induced to enroll is high primarily because the control-group enrollment rate of 94 percent is also high. Even if the program were maximally effective, increasing enrollment to 100 percent, then the cost of the program would still be $1,323 per dropout avoided.[30] As a benchmark, annual public spending per student averages approximately Php12,800 (US$270) (Al-Samarrai, 2016).

As a program designed to increase education and promote employment these program impacts come at a very high cost. Not included in our calculations, however, is the benefit that students receive from their earnings, particularly given that the opportunity cost of participating in the program is likely fairly low (only 18 percent of the control group did any work over the summer aside from SPES).

[30] The standard error of control-group enrollment is 0.7 percent, which gives a best-case cost of $1,060 per student enrolled using the lower bound (92.9 percent) of the 95-percent confidence interval for enrollment.

6 Conclusion

We measure the causal impact of temporary work experience provided through a subsidized employment program in the Philippines. Using a randomized field experiment, we find that short-term employment increases employment rates by a statistically significant 4.4 percentage points (79 percent), and this gain is concentrated entirely among college-level students. This rise in employment does not accompany a change in aspirations or general work skills, and it does not come at the expense of education.

The gain in employment is most consistent with work experience providing a more reliable signal of applicant quality, as we find suggestive evidence that students without work experience may be most likely to benefit, and we find a distinct lack of evidence to support other channels. Specifically, the potential for skill acquisition is limited by the short duration of work experience (20 days) plus the fairly routine nature of work tasks. Additionally, we see no evidence of increases in hard or soft skills along multiple measures. Among SPES beneficiaries employed at endline, 88 percent are with a different employer, indicating that firm-specific skills are unlikely to drive results and that screening is unlikely to be an important mechanism. We do not see evidence of changes in aspirations, reservation wages, or job search methods, which could also lead to increased employment. While these findings most support a signaling mechanism, we cannot entirely rule out other explanations, such as if participants gained skills valued by employers that are not captured by our instruments.

These results reflect partial equilibrium effects, as the experimental design does not enable us to account for potential displacement effects induced by those who are now more likely to be employed (Crépon et al., 2013). However, the absence of larger treatment effects in areas with higher oversubscription rates suggests this may be unlikely to be a problem at the current program scale. In any case, broadening the pool of qualified applicants could raise firm productivity through improved match quality, and policymakers may desire to direct resources to specific groups, such as disadvantaged youth, who likely face the most difficult transitions to employment. Even within the pool of lower-income students that this study considers, we find suggestive evidence of more pronounced effects among students without previous work experience and those from relatively poorer backgrounds.

These results indicate that summer employment programs can lead to longer-run employment gains in developing countries and that these gains are most consistent with an employer signaling mechanism. However, as implemented, these gains are not cost-effective. We also see that, like previous studies in the United States, there are no detectable impacts on education outcomes, and any potential gains would come at a very high cost. This result demonstrates the importance of considering the inframarginality of participants, as it is hard to increase enrollment rates among a population likely to attend school regardless. Aligning program implementation and design with overall policy objectives and accounting for variations in local program implementation, while perhaps easier said than done, is crucial to developing and implementing programs to maximize their effectiveness.

References

Abebe, Girum, Marcel Fafchamps, Michael Koelle, and Simon Quinn, "Learning Management Through Matching: A Field Experiment Using Mechanism Design," Working Paper 2020.

Abebe, Girum Tefera, Stefano Caria, Marcel Fafchamps, Paolo Falco, Simon Franklin, Simon Quinn, and Forhad Shilpi, "Matching Frictions and Distorted Beliefs: Evidence from a Job Fair Experiment," Working Paper 2020.

Acevedo, Paloma, Guillermo Cruces, Paul Gertler, and Sebastian Martinez, "How Vocational Education Made Women Better Off but Left Men Behind," *Labour Economics*, 2020, *65* (101824).

Al-Samarrai, Samer, "Assessing Basic Education Service Delivery in the Philippines: Public Education Expenditure Tracking and Quantitative Service Delivery Study," Working Paper, World Bank Group 2016.

Altonji, Joseph G. and Charles R. Pierret, "Employer Learning and Statistical Discrimination," *The Quarterly Journal of Economics*, 2001, *116* (1), 313–350.

Attanasio, Orazio, Adriana Kugler, and Costas Meghir, "Subsidizing Vocational Training for Disadvantaged Youth in Colombia: Evidence from a Randomized Trial," *American Economic Journal: Applied Economics*, 2011, *3* (3), 188–220.

Banerjee, Abhijit, Rukmini Banerji, James Berry, Esther Duflo, Harini Kannan, Shobhini Mukerji, Marc Shotland, and Michael Walton, "From Proof of Concept to Scalable Policies: Challenges and Solutions, with an Application," *Journal of Economic Perspectives*, 2017, *31* (4), 73–102.

Beam, Emily A., "Do Job Fairs Matter? Experimental Evidence on the Impact of Job-Fair Attendance," *Journal of Development Economics*, 2016, *120*, 32–40.

Bold, Tessa, Mwangi Kimenyi, Germano Mwabu, Alice Ng'ang'a, and Justin Sandefur, "Experimental Evidence on Scaling up Education Reforms in Kenya," *Journal of Public Economics*, 2018, *168*, 1–20.

Brea, Mayra Z., "Report of the Interpretation of the CPS Scales, Rosenberg and Grit and Revision Proposals to Improve Reliability," Working Paper 105233, World Bank Group 2011.

Bureau of Local Employment, "Special Program for Employment of Students: 2016 Year-End Report," 2017.

Card, David, Pablo Ibarrarán, Ferdinando Regalia, David Rosas-Shady, and Yuri Soares, "The Labor Market Impacts of Youth Training in the Dominican Republic," *Journal of Labor Economics*, 2011, *29* (2), 267–300.

Crépon, Bruno, Esther Duflo, Marc Gurgand, Roland Rathelot, and Philippe Zamora, "Do Labor Market Policies Have Displacement Effects? Evidence from a Clustered Randomized Experiment," *The Quarterly Journal of Economics*, 2013, *128* (2), 531–580.

Duncan, Beverly, "Dropouts and the Unemployed," *Journal of Political Economy*, 1965, *73* (2), 121–134.

Farber, Henry S. and Robert Gibbons, "Learning and Wage Dynamics," *The Quarterly Journal of Economics*, 1996, *111* (4), 1007–1047.

Galasso, Emanuela, Martin Ravallion, and Agustin Salvia, "Assisting the Transition from Workfare to Work: A Randomized Experiment," *ILR Review*, 2004, *58* (1), 128–142.

Gelber, Alexander, Adam Isen, and Judd B. Kessler, "The Effects of Youth Employment: Evidence from New York City Lotteries," *The Quarterly Journal of Economics*, 2016, *131* (1), 423–460.

Gottschalk, Peter, "Can Work Alter Welfare Recipients' Beliefs?," *Journal of Policy Analysis and Management*, 2005, *24* (3), 485–498.

Groh, Matthew, Nandini Krishnan, David McKenzie, and Tara Vishwanath, "Do Wage Subsidies Provide a Stepping-Stone to Employment for Recent College Graduates? Evidence from a Randomized Experiment in Jordan," *Review of Economics and Statistics*, 2016, *98* (3), 488–502.

Heckman, James J., Jora Stixrud, and Sergio Urzua, "The Effects of Cognitive and Noncognitive Abilities on Labor Market Outcomes and Social Behavior," *Journal of Labor Economics*, 2006, *24* (3), 411–482.

Heller, Sara B., "Summer Jobs Reduce Violence among Disadvantaged Youth," *Science*, 2014, *346* (6214), 1219–1223.

Ibarrarán, Pablo, Laura Ripani, Bibiana Taboada, Juan Miguel Villa, and Brigida Garcia, "Life Skills, Employability and Training for Disadvantaged Youth: Evidence from a Randomized Evaluation Design," *IZA Journal of Labor & Development*, 2014, *3* (1), 10.

Kluve, Jochen, Susana Puerto, David Robalino, Jose M. Romero, Friederike Rother, Jonathan Stöterau, Felix Weidenkaff, and Marc Witte, "Do Youth Employment Programs Improve Labor Market Outcomes? A Quantitative Review," *World Development*, 2019, *114*, 237–253.

Le Barbanchon, Thomas, Diego Ubfal, and Federico Araya, "The Effects of Working while in School: Evidence from Uruguayan Lotteries," Working Paper 2020.

Leos-Urbel, Jacob, "What Is a Summer Job Worth? The Impact of Summer Youth Employment on Academic Outcomes," *Journal of Policy Analysis and Management*, 2014, *33* (4), 891–911.

Levinsohn, James and Todd Pugatch, "Prospective Analysis of a Wage Subsidy for Cape Town Youth," *Journal of Development Economics*, 2014, *108*, 169–183.

_ **, Neil Rankin, Gareth Roberts, and Volker Schöer**, "Wage Subsidies and Youth Employment in South Africa: Evidence from a Randomised Control Trial," Working Paper 02/2014, Stellenbosch University, Department of Economics 2014.

Matsumoto, Makiko and Sara Elder, "Characterizing the School-to-Work Transitions of Young Men and Women: Evidence from the ILO School-to-Work Transition Surveys," Technical Report, International Labour Organization 2010.

McKenzie, David, "How Effective Are Active Labor Market Policies in Developing Countries? A Critical Review of Recent Evidence," *The World Bank Research Observer*, 2017, *32* (2), 127–154.

_ **, Nabila Assaf, and Ana Paula Cusolito**, "The Demand for, and Impact of, Youth Internships: Evidence from a Randomized Experiment in Yemen," *IZA Journal of Labor & Development*, 2016, *5* (1), 1.

Pallais, Amanda, "Inefficient Hiring in Entry-Level Labor Markets," *American Economic Review*, 2014, *104* (11), 3565–99.

Philippine Statistics Authority, "Yearbook of Labor Statistics," 2015.

_ , "SDG Watch Philippines, Goal 4," 2018.

Quintini, Glenda and Sébastien Martin, "Same Same but Different: School-to-Work Transitions in Emerging and Advanced Economies," Social, Employment and Migration Working Papers 154, OECD iLibrary 2014.

RA 7323, "Special Program for Employment of Students (SPES)," 1992.

Rosenberg, Morris, *Society and the Adolescent Self-Image*, Princeton University Press, 1965.

World Bank, "World Development Report 2013: Jobs," 2012.

_ , "Republic of the Philippines Labor Market Review," 2016.

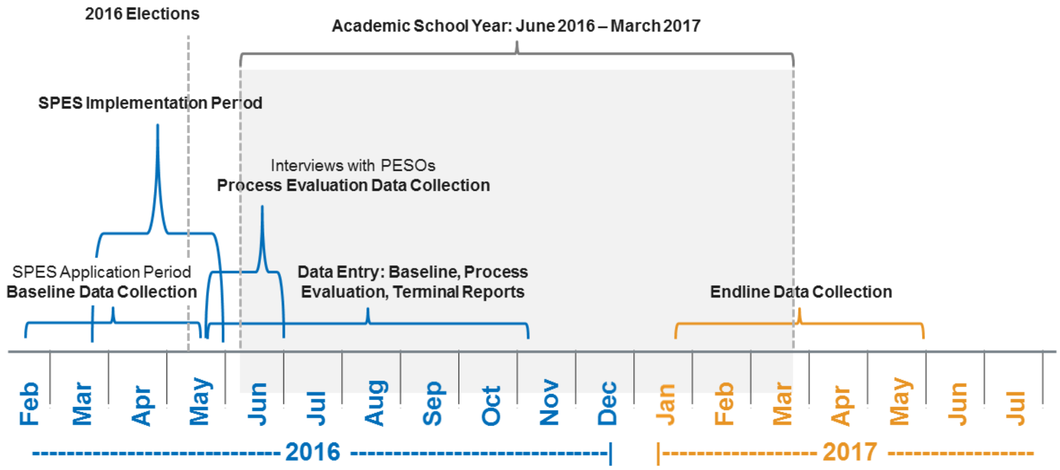
_ , "School Enrollment, Tertiary (% gross)," 2018. Last accessed: 24 March 2019. Adapted from UNESCO Institute for Statistics.

_ , "World Development Report 2018: Learning to Realize Education's Promise," 2018.

X-Rates, "Monthly Average Exchange Rates," 2017. Accessed 15 January 2018.

Figures

Figure 1: Study timeline



Tables

Table 1: Descriptive statistics and balance tests

	All		Control		Treatment			
	N	Mean/ Std. Dev	N	Mean/ Std. Dev	N	Mean/ Std. Dev	T-test p-val	RI p-val
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female	3795	0.658	1285	0.668	2510	0.654	0.483	0.628
		[0.008]		[0.013]		[0.009]		
Age (mean)	3701	17.185	1229	17.076	2472	17.239	0.601	0.571
		[0.029]		[0.050]		[0.036]		
College	3795	0.502	1285	0.394	2510	0.557	0.549	0.775
		[0.008]		[0.014]		[0.010]		
Any past work experience	2545	0.196	934	0.194	1611	0.197	0.082*	0.064*
		[0.008]		[0.013]		[0.010]		
Formal work experience	2545	0.072	934	0.070	1611	0.073	0.536	0.602
		[0.005]		[0.008]		[0.006]		
Informal work experience	2545	0.047	934	0.055	1611	0.043	0.285	0.291
		[0.004]		[0.007]		[0.005]		
Lowest acceptable daily wage	2155	325	825	310	1330	335	0.436	0.427
		[6.81]		[11.16]		[8.59]		
Expected daily wage after graduation	2205	530	842	504	1363	546	0.620	0.612
		[9.51]		[14.98]		[12.28]		
Expected tuition next year	2146	11263	810	10887	1336	11491	0.537	0.686
		[403]		[350]		[611]		
Expected educ. expenses next year	2108	9529	811	9186	1297	9744	0.193	0.136
		[371]		[423]		[542]		
Joint significance of all covariates							0.741	

Notes: All baseline respondents included. "College" includes 51 respondents enrolled at the vocational level. P-values in column 7 and 8 reflect tests for equality of means between treatment and control groups, with the randomization inference p-values in column 8 based on 1,000 replications. Covariate-specific and joint balance tests include stratification-cell fixed effects. Joint test in column 7 based on estimating a seemingly unrelated regression using covariates and missing flags (stratification cells included but not part of joint test). Although we stratify by college level and gender, we test for balance on these variables because we did not stratify on all covariates in municipalities with few applicants. **** $p<0.01$, ** $p<0.05$, * $p<0.10$.*

Table 2: Impact of SPES invitation on SPES enrollment and self-reported summer employment

	First stage		Summer employment and earnings		
	(1) Self-reported	(2) Admin	(3) Any summer work	(4) Total earnings	(5) Total earnings, inc. SPES
Invited to SPES	0.508***	0.575***			
	[0.020]	[0.018]			
Enrolled in SPES			0.797***	-321.743	5649.550***
			[0.023]	[244.127]	[328.485]
Observations	3281	3281	3280	3280	3273
Mean, control group	0.282	0.177	0.435	876.903	1770.873
F-test statistic	665	1020			

Notes: All endline respondents included. All specifications include controls listed in Table 1 along with stratification-cell fixed effects. Column 4 sums self-reported earnings with administrative earnings from SPES records. Consequently, 65 respondents who did not report enrolling in SPES are recorded as having non-zero SPES earnings. **** p<0.01, ** p<0.05, * p<0.10*

Table 3: Impact of SPES on self-reported employment

	(1) Currently working (formal)	(2) Looked for work	(3) Current monthly earnings	(4) Work hours weekly now
Panel A: Aggregate treatment effects				
Enrolled in SPES	0.044**	-0.024	177.152	1.747**
	[0.020]	[0.035]	[112.213]	[0.868]
Panel B: Interacted by gender				
Enrolled in SPES	0.078**	0.022	231.791	2.192
	[0.039]	[0.063]	[276.227]	[1.685]
SPES X Female	-0.046	-0.076	-77.399	-0.610
	[0.045]	[0.076]	[295.842]	[1.961]
p-value, SPES + SPES X Female	0.179	0.208	0.145	0.112
Panel C: Interacted by education level				
Enrolled in SPES	0.008	-0.082*	-22.931	0.203
	[0.022]	[0.042]	[89.868]	[1.077]
SPES X College	0.088**	0.143**	462.368*	3.738**
	[0.041]	[0.069]	[246.194]	[1.740]
p-value, SPES + SPES X College	0.009	0.290	0.063	0.005
Observations	3281	3280	3278	3281
Mean, control group	0.056	0.216	218.532	2.114

Notes: All endline respondents included. All specifications include controls listed in Table 1 along with stratification-cell fixed effects. Panels B and C add controls multiplied by the binary interaction term along with uninteracted stratification-cell fixed effects. *** *p<0.01, ** p<0.05, * p<0.10*

Table 4: Characteristics of jobs held at endline survey

	All (1)	Not SPES (2)	SPES (3)
Mean among employed students			
Months employed	18.29	22.24	16.54
Monthly wage	5111	4493	5368
Hours/week	38.38	40.78	37.32
Share of employed students			
Private sector	0.79	0.81	0.78
Public/non-profit sector	0.21	0.19	0.22
On-the-job training/internship	0.15	0.06	0.19
Salaried	0.44	0.40	0.46
"Regular" (ie formal)	0.23	0.20	0.24
Found through SPES	0.09	0.01	0.12
Found referral	0.55	0.54	0.55
Applied directly	0.29	0.34	0.27
Position type			
Food service	0.20	0.13	0.23
Sales	0.21	0.24	0.20
Cleaning/laborer	0.15	0.21	0.12
Office	0.09	0.10	0.08
Baseline characteristics			
Work experience	0.33	0.35	0.31
Formal work experience	0.12	0.20	0.08
College level	0.66	0.62	0.68
Female	0.54	0.56	0.53
Observations	222	68	154

Notes: Sample restricted to endline respondents who report being employed. Column 1 provides average characteristics of job positions among all employed respondents. Column 2 provides characteristics for those who participated in SPES (a non-random sample), and Column 3 provides characteristics for those who did not participate in SPES.

Table 5: Impact of SPES on self-reported education

	(1) Enrolled in school	(2) Will enroll, 2017-18	(3) Grade-Weighted Average	(4) Academic track, SHS only
Panel A: Aggregate treatment effects				
Enrolled in SPES	0.011	0.021	0.064	-0.096
	[0.019]	[0.023]	[0.082]	[0.063]
Panel B: Interacted by gender				
Enrolled in SPES	0.072*	0.073*	-0.079	-0.037
	[0.037]	[0.041]	[0.152]	[0.132]
SPES X Female	-0.087**	-0.079	0.228	-0.086
	[0.043]	[0.050]	[0.180]	[0.151]
p-value, SPES + SPES X Female	0.489	0.851	0.126	0.085
Panel C: Interacted by education level				
Enrolled in SPES	0.041**	0.007	0.016	
	[0.021]	[0.016]	[0.091]	
SPES X College	-0.075*	0.032	0.112	
	[0.041]	[0.053]	[0.166]	
p-value, SPES + SPES X College	0.355	0.453	0.381	
Observations	3281	3269	3240	
Mean, control group	0.943	0.917	0.000	

Notes: All endline respondents included, column 4 restricted to students enrolled in grade 11 and 12. Grade-weighted average normalized using education-level and scale-specific means and standard deviations of the control group. All specifications include controls listed in Table 1 along with stratification-cell fixed effects. Panels B and C add controls multiplied by the binary interaction term along with uninteracted stratification-cell fixed effects. **** p<0.01, ** p<0.05, * p<0.10*

Table 6: Impact of SPES on self-reported employment and enrollment

	(1) Enrolled and working	(2) Enrolled, not working	(3) Not enrolled, working	(4) Not enrolled, not working
Panel A: Aggregate treatment effects				
Enrolled in SPES	0.032*	-0.021	0.012	-0.023
	[0.018]	[0.026]	[0.009]	[0.017]
Panel B: Interacted by gender				
Enrolled in SPES	0.059*	0.012	0.018	-0.090***
	[0.035]	[0.048]	[0.018]	[0.032]
SPES X Female	-0.038	-0.049	-0.008	0.095**
	[0.041]	[0.057]	[0.021]	[0.038]
p-value, SPES + SPES X Female	0.326	0.213	0.309	0.796
Panel C: Interacted by education level				
Enrolled in SPES	0.009	0.032	-0.001	-0.040**
	[0.019]	[0.027]	[0.011]	[0.018]
SPES X College	0.059	-0.134**	0.028	0.047
	[0.037]	[0.052]	[0.020]	[0.037]
p-value, SPES + SPES X College	0.040	0.027	0.106	0.836
Observations	3281	3281	3281	3281
Mean, control group	0.041	0.902	0.015	0.042

Notes: All endline respondents included. All specifications include controls listed in Table 1 along with stratification-cell fixed effects. Panels B and C add controls multiplied by the binary interaction term along with uninteracted stratification-cell fixed effects. **** p<0.01, ** p<0.05, * p<0.10*

Table 7: Impact of SPES on skills and labor market perceptions

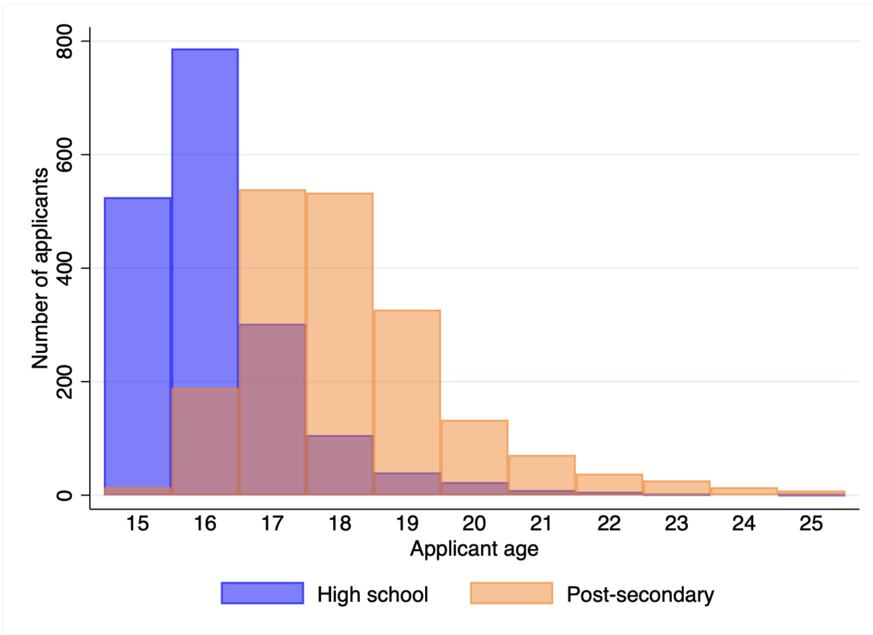
	(1) Work tasks index	(2) Self-esteem index	(3) Life skills index	(4) Workplace skills index	(5) Likely find job w/in 6 mo. of grad.	(6) Lowest wage willing to accept	(7) Expected wage after graduation	(8) Expect to finish college or higher
Panel A: Aggregate treatment effects								
Enrolled in SPES	0.098	-0.001	0.054	-0.126	0.074*	-100.802	-211.845	0.006
	[0.081]	[0.089]	[0.083]	[0.085]	[0.042]	[96.054]	[191.838]	[0.019]
Panel B: Interacted by gender								
Enrolled in SPES	0.064	0.070	0.023	-0.064	0.090	-1.049	-74.347	-0.025
	[0.148]	[0.152]	[0.147]	[0.142]	[0.076]	[63.989]	[132.137]	[0.036]
SPES X Female	0.063	-0.094	0.049	-0.085	-0.030	-149.596	-212.726	0.051
	[0.176]	[0.188]	[0.179]	[0.178]	[0.091]	[154.800]	[310.362]	[0.042]
p-value, SPES + SPES X Female	0.187	0.824	0.480	0.160	0.239	0.284	0.306	0.253
Panel C: Interacted by education level								
Enrolled in SPES	0.102	-0.042	-0.011	-0.118	0.040	-78.341	-199.967	0.010
	[0.101]	[0.104]	[0.105]	[0.101]	[0.051]	[71.700]	[142.940]	[0.028]
SPES X College	0.003	0.084	0.158	-0.044	0.092	-22.306	34.382	-0.009
	[0.157]	[0.171]	[0.159]	[0.165]	[0.083]	[86.349]	[171.503]	[0.032]
p-value, SPES + SPES X College	0.410	0.774	0.248	0.244	0.059	0.424	0.510	0.928
Observations	3280	3280	3280	3280	3101	3097	3097	3281
Mean, control group	0.000	-0.000	-0.000	0.000	0.653	344.889	585.949	0.945

Notes: All endline respondents included. All specifications include controls listed in Table 1 along with stratification-cell fixed effects. Work tasks index constructed based on reported experience with 11 office tasks: Microsoft Word, encoding, Excel, Powerpoint, photocopying, scanning, sorting, answering phones, bookkeeping, online searches, and using e-mail. Skill-specific effects reported in Appendix Table A13. Self-esteem is based on five items drawn from Rosenberg (1965), life skills is based on a seven-question index developed by the Philippine Bureau of Local Employment, and workplace skills is based on five questions drawn from Brea (2011). Each index normalized using mean and standard deviation of the control group. **** p<0.01, ** p<0.05, * p<0.10.*

Appendix

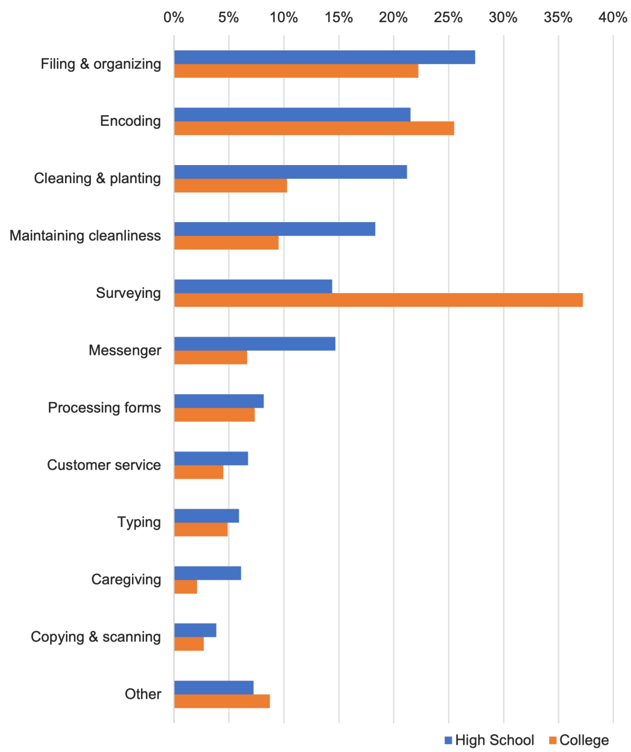
Online Appendix

Figure A1: Distribution of applicants, by age and education level



Notes: All respondents with non-missing age and education levels at baseline included.

Figure A2: Distribution of SPES tasks, by education level



Notes: Percentages reflect share of SPES participants in endline sample who reported each task as either a primary or secondary task. "Other" includes the four least-frequently reported assignments from Appendix Table A1, including teaching/tutoring, manual tasks, agricultural surveying and charity/neighborhood work.

Table A1: Distribution of SPES tasks

Rank	Assignment	Primary assignment			Primary or secondary	
		No. of students	Share	Cumul. share	No. of students	Share
		(1)	(2)	(3)	(4)	(5)
1	Surveying (enumerator/census)	802	0.258	0.258	915	0.295
2	Encoding/updating records (data)	572	0.184	0.443	778	0.251
3	Filing and organizing documents	466	0.15	0.593	730	0.235
4	Cleaning/sweeping/planting	281	0.091	0.683	513	0.165
5	Maintain cleanliness/orderliness of office	178	0.057	0.741	420	0.135
6	Messenger/errands/distributing flyers	140	0.045	0.786	318	0.102
7	Process and prepare forms	137	0.044	0.83	235	0.076
8	Customer service/sales/organizing	122	0.039	0.869	183	0.059
9	Typing letters/documents	97	0.031	0.9	174	0.056
10	Other, specify	81	0.026	0.927	128	0.041
11	Care giving/hospital assistance	68	0.022	0.948	109	0.035
12	Copying and scanning documents	60	0.019	0.968	100	0.032
13	Teaching/tutoring of children	47	0.015	0.983	76	0.024
14	Manual tasks	40	0.013	0.996	62	0.02
15	Surveying (agriculture /plants/animals)	8	0.003	0.998	8	0.003
16	Charity/neighborhood work	5	0.002	1	8	0.003
	Total	3104			3104	

Table A2: Time to receive SPES payment after program end, by payment source

	Employer		DOLE	
	N	Share	N	Share
Less than 2 weeks	680	0.30	191	0.08
2 weeks-1 month	843	0.37	458	0.20
1 - 2 months	369	0.16	419	0.19
3+ months	190	0.08	812	0.36
Not yet received	167	0.07	369	0.16
Not directly received	12	0.01	11	0.01
Observations	2261		2260	

Notes: Sample includes endline respondents who participated in SPES with non-missing responses. Participants typically received 60 percent of their wages directly from their employer, and they collected the 40-percent remainder from DOLE later.

Table A3: Characteristics of invited SPES programs, by whether participated

	Participating	Non-participating
Average program enrollment, 2014–16	268.11	486.94
Usually have oversubscription	0.36	0.07
Set aside slots for specific groups	0.24	0.24
Place any students with private employers	0.20	0.21
Share returning participants	0.34	0.26
Public recruitment process	1.00	0.90
Hold info session before application	0.17	0.20
Hold orientation session for chosen beneficiaries	0.80	0.76
Government officials influence selection	0.32	0.23
Observations	25	30

Notes: Data based on qualitative interviews with SPES program managers among municipalities invited to participate in study. Non-participating municipalities include programs that either declined to participate or accepted but did not have more eligible applicants than positions available. All questions aside from total enrollment ask about typical experiences over the past few years, excluding the current impact evaluation year. Government official influence defined as whether the local mayor takes part in the selection process and/or the office considers referrals from government officials when determining who is chosen among eligible applicants.

Table A4: Characteristics of SPES participants, nationally and among study sample

	National (1)	**Study Sample** (2)
	SPES Participants	Baseline
Share female	0.59	0.66
Share 15-18	0.48	0.81
Share 19-21	0.36	0.16
Share 22-25	0.16	0.03
Previous SPES participants	0.12	0.00
	SPES Participants	SPES Participants
SPES employer type		
Private establishment	0.08	0.06
Government	0.77	0.94
Other establishments	0.15	0.00
SPES program duration		
20 days	0.55	0.73
21-30 days	0.21	0.25
31-51 days	0.10	0.02
52 days	0.07	0.00

Notes: Column 1 includes all 2016 SPES participants Bureau of Local Employment (2017). "Other establishments" are almost exclusively placement at higher education institutions, which were not included in our study.

Table A5: Reasons for endline attrition

	Number (1)	Share (2)	Cumul. Share (3)
Respondent could not be reached	456	0.89	0.89
Partial interview	21	0.04	0.93
Refused or hung up	30	0.06	0.99
Scheduled, could not recontact	4	0.01	1.00
Total	511	1.00	

Notes: Sample includes all respondents attempted but not surveyed at endline.

Table A6: Differential endline attrition

	Overall	Control	Treatment	P-value
	(1)	(2)	(3)	(4)
Baseline respondents	3795	1285	2510	
Attempted to contact	3792	1284	2508	
Response rate	0.87	0.84	0.88	
No covariates				0.333
Covariates				0.425

Notes: Response rates conditional on attempting to contact for endline, excluding 3 baseline respondents for which we had no contact information. P-values based on tests for differential response rates by treatment status, including stratification-cell fixed effects in both specifications. The "covariates" test also includes controls listed in Table 1. **** p<0.01, ** p<0.05, * p<0.10.*

Table A7: Descriptive statistics and balance, endline sample

	All		Control		Treatment			
	N	Mean/ Std. Dev	N	Mean/ Std. Dev	N	Mean/ Std. Dev	T-test p-val	RI p-val
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female	3281	0.666	1076	0.678	2205	0.66	0.847	1.000
		[0.008]		[0.014]		[0.010]		
Age (mean)	3220	17.178	1038	17.077	2182	17.226	0.621	0.402
		[0.031]		[0.055]		[0.038]		
College	3281	0.518	1076	0.414	2205	0.569	0.411	0.741
		[0.009]		[0.015]		[0.011]		
Any past work experience	2206	0.196	779	0.2	1427	0.194	0.119	0.116
		[0.008]		[0.014]		[0.010]		
Formal work experience	2206	0.074	779	0.076	1427	0.073	0.749	0.820
		[0.006]		[0.009]		[0.007]		
Informal work experience	2206	0.048	779	0.053	1427	0.045	0.584	0.594
		[0.005]		[0.008]		[0.005]		
Lowest acceptable daily wage	1875	324	688	312	1187	330	0.397	0.384
		[7.16]		[12.47]		[8.69]		
Expected daily wage after graduation	1916	530	699	519	1217	537	0.814	0.788
		[10.04]		[17.27]		[12.31]		
Expected tuition next year	1864	11150	671	10334	1193	11609	0.25	0.136
		[451]		[348]		[677]		
Expected educ. expenses next year	1833	9263	674	9041	1159	9393	0.239	0.215
		[301]		[473]		[388]		
Joint significance of all covariates								0.741

Notes: Sample restricted to endline respondents. P-values in column 7 and 8 reflect tests for equality of means between treatment and control groups, with the randomization inference p-values in column 8 based on 1,000 replications. Covariate-specific and joint balance tests include stratification-cell fixed effects. Joint test based on estimating a seemingly unrelated regression using covariates and missing flags (stratification cells included but not part of joint test). Although we stratify by college level and gender, we test for balance on these variables because we did not stratify on all covariates in municipalities with few applicants. **** p<0.01, ** p<0.05, * p<0.10.*

Table A8: Impact of SPES on self-reported wages and work hours, conditional on employment at endline

	(1) Current monthly earnings	(2) Work hours weekly now
Panel A: Aggregate treatment effects		
Enrolled in SPES	-422.41	7.41
	[2577.92]	[10.30]
Panel B: Interacted by gender		
Enrolled in SPES	-1525.17	-6.16
	[2893.39]	[10.43]
SPES X Female	9016.46	37.58
	[6446.53]	[26.06]
p-value, SPES + SPES X Female	0.186	0.165
Panel C: Interacted by education level		
Enrolled in SPES	-1737.53	30.76*
	[1862.28]	[16.74]
SPES X College	3255.16	-24.94
	[4740.12]	[18.03]
p-value, SPES + SPES X College	0.718	0.576
Observations	219	222
Mean, control group	3919.00	37.92

Notes: Sample includes endline respondents who report positive earnings and work hours, respectively. All specifications include controls listed in Table 1 along with stratification-cell fixed effects. Panels B and C add controls multiplied by the binary interaction term along with uninteracted stratification-cell fixed effects. **** p<0.01, ** p<0.05, * p<0.10*

Table A9: Impact of SPES on perceived barriers to job search

	Reports that X will affect job search much or very much					
	(1) Insufficient education	(2) Insufficient experience	(3) Few available jobs	(4) Difficult application process	(5) Few contacts	(6) Discrimination
Panel A: Aggregate treatment effects						
Enrolled in SPES	-0.066*	-0.007	-0.004	0.010	0.049	-0.026
	[0.034]	[0.041]	[0.040]	[0.042]	[0.044]	[0.044]
Panel B: Interacted by gender						
Enrolled in SPES	-0.018	0.034	-0.104	0.009	0.024	-0.135*
	[0.065]	[0.072]	[0.072]	[0.072]	[0.076]	[0.076]
SPES X Female	-0.069	-0.065	0.153*	0.003	0.037	0.166*
	[0.076]	[0.087]	[0.087]	[0.089]	[0.093]	[0.093]
p-value, SPES + SPES X Female	0.031	0.538	0.316	0.817	0.256	0.577
Panel C: Interacted by education level						
Enrolled in SPES	-0.079*	0.001	0.003	0.051	-0.004	0.012
	[0.042]	[0.050]	[0.050]	[0.050]	[0.052]	[0.052]
SPES X College	0.031	-0.025	-0.006	-0.111	0.126	-0.105
	[0.064]	[0.077]	[0.078]	[0.080]	[0.086]	[0.086]
p-value, SPES + SPES X College	0.364	0.702	0.973	0.374	0.091	0.197
Observations	3280	3280	3280	3279	3279	3279
Mean, control group	0.825	0.703	0.706	0.650	0.482	0.516

Notes: All endline respondents included. All specifications include controls listed in Table 1 along with stratification-cell fixed effects. Panels B and C add controls multiplied by the binary interaction term along with uninteracted stratification-cell fixed effects. **** p<0.01, ** p<0.05, * p<0.10*

Table A10: Relationship between SPES program tasks and endline employment, controlling for student characteristics

	(1) Currently Working
Invited to SPES	-0.005
	[0.017]
College	-0.008
	[0.043]
Encoding	0.016
	[0.017]
Typing	-0.002
	[0.025]
Filing and organizing	0.011
	[0.015]
Copying and scanning	0.016
	[0.030]
Processing forms	0.008
	[0.023]
Cleaning and planting	0.001
	[0.019]
Surveying	0.018
	[0.018]
Surveying (agri.)	0.154
	[0.152]
Messenger	0.008
	[0.019]
Maintaining cleanliness	0.031
	[0.019]
Teaching	-0.022
	[0.034]
Caregiving	0.022
	[0.029]
Charity work	0.058
	[0.138]
Customer service	0.064*
	[0.038]
Manual tasks	0.029
	[0.057]
Other	-0.003
	[0.027]
Observations	2262
Mean, control group	0.056
p-value, joint significance	0.899

Notes: All endline respondents who participated in SPES included. All specifications include controls listed in Table 1 along with stratification-cell fixed effects. Joint-significance p-value based on a hypothesis test of whether SPES program tasks jointly predict likelihood of working for pay at endline. *** *p<0.01*, ** *p<0.05*, * *p<0.10*.

Table A11: Distribution of positions held at endline

	Count	Frequency	Cum. Frequency
	(1)	(2)	(3)
Sales	47	0.21	0.21
Food service	45	0.2	0.41
Cleaning/laborer	33	0.15	0.56
Office assistant/clerical	20	0.09	0.65
Misc./unspecified assistant	18	0.08	0.73
Production worker	14	0.06	0.79
Skilled/technical	12	0.05	0.84
Vendor/sales	10	0.05	0.89
Education/health	10	0.05	0.94
Other	13	0.06	1.00
Total		222	

Notes: Sample includes respondents who report working for pay at endline.

Table A12: How participants spent SPES earnings

	Frequency	Share
Paid tuition fee/schooling expenses	1532	68%
Helped support family	990	44%
Bought personal effects	800	35%
Paid for extra-curricular activities	271	12%
Saved for the future	199	9%
Other	35	2%
Observations	2262	

Notes: Sample includes endline respondents who participated in SPES with non-missing responses. The total of 3827 responses reflects 2262 respondents, as respondents could provide multiple ways they spent SPES earnings.

Table A13: Respondent has "some" or "a lot" of experience, by work task

	(1) Using Word	(2) Power-Point	(3) Sorting	(4) Online searches	(5) Encoding	(6) Photo-copying
Enrolled in SPES	0.036	-0.071**	0.050	0.026	0.060	-0.038
	[0.032]	[0.033]	[0.039]	[0.022]	[0.038]	[0.034]
Observations	3279	3280	3279	3280	3280	3280
Mean, control group	0.827	0.843	0.666	0.927	0.718	0.829

	(7) Answering phones	(8) E-mail	(9) Using Excel	(10) Scanning	(11) Bookkeep-ing
Enrolled in SPES	0.163***	-0.044	0.043	0.018	-0.002
	[0.043]	[0.042]	[0.043]	[0.042]	[0.041]
Observations	3280	3279	3279	3280	3278
Mean, control group	0.386	0.612	0.562	0.606	0.331

Notes: All endline respondents included. All specifications include controls listed in Table 1 along with stratification-cell fixed effects. *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table A14: LIFE SKILLS AND WORKPLACE SKILLS SURVEY ITEMS

Life skills, Bureau of Local Employment

Now, I'm going to read you a series of statements. For each statement, please tell me if it describes you all of the time, most of the time, sometimes, seldom, or never.

Item	Statement
LS1	I am on time and conscious about my deadlines and manage my timetable for work.
LS2	I communicate and express my concerns related to work with my supervisor to get his or her opinion or advice.
LS3	I listen attentively to other people and try not to interrupt them while talking.
LS4	I budget my allowance (or salary) and prioritize so I can buy things that I need rather than things that I want.
LS5	I try to save my extra money for emergencies or give it to my parents/family.
LS6	I make sure that my clothes suit the occasion that I am going to or attending.
LS7	I feel determined to finish my studies and immediately look for work.

Workplace skills (Brea, 2011)

For the last series of statements, please tell me whether you strongly agree, agree, neither agree nor disagree, disagree, or strongly disagree.

Item	Domain	Statement
WP1	Communication	Sometimes it takes me several tries to explain an idea
WP2	Conflict	In a conflict, I try to consider others ways of thinking before reaching a solution
WP3	Relating with others	I know how to get along well with different types of personalities
WP4	Organization	When I have something to do, I do it at the last minute
WP5	Leadership	I propose ideas to help my teammates achieve our goals

Table A15: Heterogeneous impacts of SPES on employment, by work experience and socioeconomic status

	(1) Currently working (formal)	(2) Currently looking	(3) Current monthly earnings	(4) Work hours weekly now
Panel A: Aggregate treatment effects				
Enrolled in SPES	0.044**	-0.024	177.152	1.747**
	[0.020]	[0.035]	[112.213]	[0.868]
Panel B: Interacted by work experience				
Enrolled in SPES	0.045**	-0.037	179.134	1.952**
	[0.020]	[0.036]	[117.425]	[0.881]
SPES X Any Experience	-0.018	0.106	-49.710	-2.253
	[0.062]	[0.085]	[343.522]	[2.381]
p-value, SPES + SPES X Any Experience	0.673	0.430	0.694	0.899
Panel C: Interacted by work experience (college only)				
Enrolled in SPES	0.094**	0.069	413.684	4.228***
	[0.038]	[0.062]	[268.702]	[1.490]
SPES X Any Experience	-0.061	0.067	-62.296	-3.440
	[0.078]	[0.107]	[458.725]	[2.797]
p-value, SPES + SPES X Any Experience	0.676	0.220	0.404	0.773
Panel D: Interacted by socio-economic status				
Enrolled in SPES	0.055**	-0.049	285.390**	2.104*
	[0.025]	[0.043]	[113.604]	[1.084]
SPES X Above-median SES	-0.034	0.039	-299.857*	-1.041
	[0.029]	[0.050]	[179.405]	[1.222]
p-value, SPES + SPES X High SES	0.389	0.819	0.934	0.301
Panel E: Interacted by oversubscription rate				
Enrolled in SPES	0.065	0.035	590.470***	3.485**
	[0.040]	[0.062]	[192.822]	[1.426]
SPES X Above-median over-subscription	-0.026	-0.083	-545.165**	-2.183
	[0.047]	[0.076]	[238.315]	[1.785]
p-value, SPES + SPES X High Oversub.	0.099	0.259	0.746	0.225
Observations	3281	3280	3278	3281
Mean, control group	0.056	0.216	218.532	2.114

Notes: All endline respondents included. All specifications include controls listed in Table 1 along with stratification-cell fixed effects. Panels B through E add controls multiplied by the binary interaction term along with uninteracted stratification-cell fixed effects. **** p<0.01, ** p<0.05, * p<0.10*

Table A16: Impact of SPES on job search behaviors

	All	Conditional on searching						
	(1) Any search	(2) Submit CV	(3) Online	(4) PESO	(5) Job fair	(6) Walk-in	(7) Fam/ friend referral	(8) Officials
Panel A: Aggregate treatment effects								
Enrolled in SPES	-0.023	0.018	0.054	0.036	-0.030	0.044	0.044	0.035
	[0.035]	[.]	[0.083]	[0.083]	[0.064]	[0.084]	[0.067]	[0.062]
Panel B: Interacted by gender								
Enrolled in SPES	0.029	-0.285**	-0.071	0.118	-0.267**	-0.046	-0.009	-0.190
	[0.062]	[0.114]	[0.139]	[0.145]	[0.121]	[0.147]	[0.131]	[0.120]
SPES X Female	-0.084	0.471***	0.168	-0.104	0.348**	0.098	0.069	0.296**
	[0.076]	[0.144]	[0.174]	[0.179]	[0.143]	[0.179]	[0.156]	[0.143]
p-value, SPES + SPES X Female	0.203	0.033	0.355	0.894	0.290	0.613	0.473	0.169
Panel C: Interacted by education level								
Enrolled in SPES	-0.080*	0.162	0.052	-0.024	-0.075	0.040	-0.038	0.115
	[0.042]	[0.103]	[0.103]	[0.112]	[0.071]	[0.106]	[0.081]	[.]
SPES X College	0.141**	-0.311***	-0.016	0.071	0.038	-0.118	0.149	-0.261
	[0.069]	[0.115]	[0.151]	[0.157]	[0.116]	[0.154]	[0.133]	[.]
p-value, SPES + SPES X College	0.292	0.024	0.759	0.702	0.714	0.522	0.324	.
Observations	3280	626	626	626	625	626	626	626
Mean, control group	0.216	0.750	0.384	0.384	0.198	0.552	0.832	0.116

Notes: All endline respondents included. All specifications include controls listed in Table 1 along with stratification-cell fixed effects, excluding expected daily wages after graduation, expected tuition, and expected educational expenses. Panels B and C add controls multiplied by the binary interaction term along with uninteracted stratification-cell fixed effects. **** p<0.01, ** p<0.05, * p<0.10*

Appendix: Robustness

B.1 Intention-to-treat estimates

Table B1: Impact of SPES on self-reported employment, ITT

	(1) Currently working (formal)	(2) Looked for work	(3) Current monthly earnings	(4) Work hours weekly now
Panel A: Aggregate treatment effects				
Invited to SPES	0.022**	-0.012	90.049	0.888*
	[0.011]	[0.019]	[58.861]	[0.456]
Panel B: Interacted by gender				
Invited to SPES	0.033*	0.015	107.725	0.913
	[0.020]	[0.033]	[137.392]	[0.878]
SPES X Female	-0.017	-0.041	-26.426	-0.037
	[0.024]	[0.040]	[148.533]	[1.031]
p-value, SPES + SPES X Female	0.179	0.250	0.147	0.099
Panel C: Interacted by education level				
Invited to SPES	0.011	-0.046**	38.147	0.636
	[0.013]	[0.023]	[53.429]	[0.566]
SPES X College	0.025	0.076**	116.370	0.565
	[0.021]	[0.032]	[116.924]	[0.806]
p-value, SPES + SPES X College	0.036	0.251	0.155	0.066
Observations	3281	3280	3278	3281
Mean, control group	0.056	0.216	218.532	2.114

Notes: All endline respondents included. All specifications include controls listed in Table 1 along with stratification-cell fixed effects. Panels B and C add controls multiplied by the binary interaction term along with uninteracted stratification-cell fixed effects. **** p<0.01, ** p<0.05, * p<0.10*

Table B2: Impact of SPES on self-reported education, ITT

	(1) Enrolled in school	(2) Will enroll, 2017-18	(3) Grade-Weighted Average	(4) Academic track, SHS only
Panel A: Aggregate treatment effects				
Invited to SPES	0.006	0.011	0.032	-0.054
	[0.010]	[0.012]	[0.043]	[0.037]
Panel B: Interacted by gender				
Invited to SPES	0.031	0.036*	-0.059	-0.024
	[0.019]	[0.021]	[0.078]	[0.065]
SPES X Female	-0.038*	-0.038	0.137	-0.046
	[0.022]	[0.026]	[0.093]	[0.079]
p-value, SPES + SPES X Female	0.570	0.902	0.128	0.120
Panel C: Interacted by education level				
Invited to SPES	0.018	0.017	0.013	
	[0.012]	[0.011]	[0.053]	
SPES X College	-0.027	-0.014	0.045	
	[0.020]	[0.025]	[0.080]	
p-value, SPES + SPES X College	0.574	0.908	0.376	
Observations	3281	3269	3240	
Mean, control group	0.943	0.917	0.000	

Notes: All endline respondents included, column 4 restricted to students enrolled in grade 11 and 12. Grade-weighted average normalized using education-level and scale-specific means and standard deviations of the control group. All specifications include controls listed in Table 1 along with stratification-cell fixed effects. Panels B and C add controls multiplied by the binary interaction term along with uninteracted stratification-cell fixed effects. **** p<0.01, ** p<0.05, * p<0.10*

Table B3: IMPACT OF SPES ON SELF-REPORTED EMPLOYMENT AND ENROLLMENT, ITT

	(1) Enrolled and working	(2) Enrolled, not working	(3) Not enrolled, working	(4) Not enrolled, not working
Panel A: Aggregate treatment effects				
Invited to SPES	0.016*	-0.011	0.006	-0.012
	[0.010]	[0.013]	[0.005]	[0.009]
Panel B: Interacted by gender				
Invited to SPES	0.025	0.006	0.008	-0.039**
	[0.018]	[0.025]	[0.010]	[0.016]
SPES X Female	-0.013	-0.025	-0.004	0.041**
	[0.021]	[0.030]	[0.012]	[0.019]
p-value, SPES + SPES X Female	0.290	0.229	0.393	0.842
Panel C: Interacted by education level				
Invited to SPES	0.004	0.014	0.007	-0.025**
	[0.012]	[0.016]	[0.005]	[0.011]
SPES X College	0.028	-0.055**	-0.002	0.030
	[0.019]	[0.026]	[0.010]	[0.018]
p-value, SPES + SPES X College	0.043	0.055	0.586	0.745
Observations	3281	3281	3281	3281
Mean, control group	0.041	0.902	0.015	0.042

Notes: All endline respondents included. All specifications include controls listed in Table 1 along with stratification-cell fixed effects. Panels B and C add controls multiplied by the binary interaction term along with uninteracted stratification-cell fixed effects. *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table B4: Impact of SPES on skills and labor market perceptions, ITT

	(1) Work tasks index	(2) Self-esteem index	(3) Life skills index	(4) Workplace skills index	(5) Likely find job w/in 6 mo. of grad.	(6) Lowest wage willing to accept	(7) Expected wage after graduation	(8) Expect to finish college or higher
Panel A: Aggregate treatment effects								
Invited to SPES	0.050	-0.001	0.027	-0.064	0.038*	-51.624	-108.493	0.003
	[0.043]	[0.047]	[0.044]	[0.044]	[0.022]	[50.688]	[101.228]	[0.010]
Panel B: Interacted by gender								
Invited to SPES	0.024	0.020	0.012	-0.040	0.045	10.878	-9.815	-0.015
	[0.077]	[0.080]	[0.076]	[0.073]	[0.039]	[36.140]	[74.097]	[0.019]
SPES X Female	0.038	-0.030	0.024	-0.035	-0.010	-92.549	-146.116	0.028
	[0.093]	[0.099]	[0.093]	[0.092]	[0.047]	[92.925]	[185.998]	[0.022]
p-value, SPES + SPES X Female	0.224	0.853	0.506	0.172	0.208	0.291	0.312	0.282
Panel C: Interacted by education level								
Invited to SPES	0.015	-0.010	-0.024	-0.061	0.023	-100.187	-211.100	-0.000
	[0.057]	[0.059]	[0.058]	[0.057]	[0.029]	[82.254]	[164.179]	[0.016]
SPES X College	0.077	0.021	0.115	-0.006	0.033	111.608	235.815	0.007
	[0.077]	[0.081]	[0.076]	[0.078]	[0.040]	[77.997]	[155.588]	[0.017]
p-value, SPES + SPES X College	0.109	0.864	0.113	0.276	0.068	0.626	0.598	0.394
Observations	3280	3280	3280	3280	3101	3097	3097	3281
Mean, control group	0.000	-0.000	-0.000	0.000	0.653	344.889	585.949	0.945

Notes: All endline respondents included. All specifications include controls listed in Table 1 along with stratification-cell fixed effects. Work tasks index constructed based on reported experience with 11 office tasks: Microsoft Word, encoding, Excel, Powerpoint, photocopying, scanning, sorting, answering phones, bookkeeping, online searches, and using e-mail. Skill-specific effects reported in Appendix Table A13. Self-esteem is based on five items drawn from Rosenberg (1965), life skills is based on a seven-question index developed by the Philippine Bureau of Local Employment, and workplace skills is based on five questions drawn from Brea (2011). Each index normalized using mean and standard deviation of the control group. **** p<0.01, ** p<0.05, * p<0.10.*

B.2 LATE estimates using administrative enrollment data

Table B5: Impact of SPES on self-reported employment, LATE administrative data

	(1) Currently working (formal)	(2) Looked for work	(3) Current monthly earnings	(4) Work hours weekly now
Panel A: Aggregate treatment effects				
Enrolled in SPES	0.039**	-0.022	156.262	1.542**
	[0.018]	[0.031]	[99.198]	[0.769]
Panel B: Interacted by gender				
Enrolled in SPES	0.070**	0.020	208.516	1.974
	[0.035]	[0.056]	[249.161]	[1.526]
SPES X Female	-0.042	-0.067	-73.341	-0.589
	[0.041]	[0.068]	[265.928]	[1.762]
p-value, SPES + SPES X Female	0.180	0.208	0.146	0.114
Panel C: Interacted by education level				
Enrolled in SPES	0.006	-0.077**	-26.055	0.151
	[0.020]	[0.039]	[84.445]	[1.013]
SPES X College	0.074**	0.132**	397.938*	3.173**
	[0.036]	[0.062]	[213.461]	[1.565]
p-value, SPES + SPES X College	0.009	0.264	0.062	0.005
Observations	3281	3280	3278	3281
Mean, control group	0.056	0.216	218.532	2.114

Notes: All endline respondents included. All specifications include controls listed in Table 1 along with stratification-cell fixed effects. Panels B and C add controls multiplied by the binary interaction term along with uninteracted stratification-cell fixed effects. *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table B6: Impact of SPES on self-reported education, LATE administrative data

	(1) Enrolled in school	(2) Will enroll, 2017-18	(3) Grade-Weighted Average	(4) Academic track, SHS only
Panel A: Aggregate treatment effects				
Enrolled in SPES	0.010	0.018	0.057	-0.089
	[0.017]	[0.021]	[0.072]	[0.057]
Panel B: Interacted by gender				
Enrolled in SPES	0.064*	0.066*	-0.071	-0.031
	[0.033]	[0.037]	[0.136]	[0.107]
SPES X Female	-0.078**	-0.071	0.202	-0.088
	[0.039]	[0.044]	[0.161]	[0.127]
p-value, SPES + SPES X Female	0.489	0.850	0.125	0.084
Panel C: Interacted by education level				
Enrolled in SPES	0.039**	0.006	0.014	
	[0.019]	[0.015]	[0.085]	
SPES X College	-0.069*	0.026	0.095	
	[0.036]	[0.046]	[0.148]	
p-value, SPES + SPES X College	0.331	0.458	0.381	
Observations	3281	3269	3240	
Mean, control group	0.943	0.917	0.000	

Notes: All endline respondents included, column 4 restricted to students enrolled in grade 11 and 12. Grade-weighted average normalized using education-level and scale-specific means and standard deviations of the control group. All specifications include controls listed in Table 1 along with stratification-cell fixed effects. Panels B and C add controls multiplied by the binary interaction term along with uninteracted stratification-cell fixed effects. **** p<0.01, ** p<0.05, * p<0.10*

Table B7: Impact of SPES on self-reported employment and education, LATE administrative data

	(1) Enrolled and working	(2) Enrolled, not working	(3) Not enrolled, working	(4) Not enrolled, not working
Panel A: Aggregate treatment effects				
Enrolled in SPES	0.029*	-0.019	0.010	-0.020
	[0.016]	[0.023]	[0.008]	[0.015]
Panel B: Interacted by gender				
Enrolled in SPES	0.053*	0.011	0.017	-0.081***
	[0.032]	[0.043]	[0.017]	[0.029]
SPES X Female	-0.035	-0.044	-0.007	0.086**
	[0.037]	[0.051]	[0.019]	[0.034]
p-value, SPES + SPES X Female	0.327	0.214	0.310	0.795
Panel C: Interacted by education level				
Enrolled in SPES	0.008	0.031	-0.001	-0.037**
	[0.018]	[0.026]	[0.010]	[0.017]
SPES X College	0.050	-0.118**	0.024	0.045
	[0.033]	[0.046]	[0.017]	[0.033]
p-value, SPES + SPES X College	0.042	0.025	0.104	0.800
Observations	3281	3281	3281	3281
Mean, control group	0.041	0.902	0.015	0.042

Notes: All endline respondents included. All specifications include controls listed in Table 1 along with stratification-cell fixed effects. Panels B and C add controls multiplied by the binary interaction term along with uninteracted stratification-cell fixed effects. **** p<0.01, ** p<0.05, * p<0.10*

Table B8: Impact of SPES on skills and labor market perceptions, LATE administrative data

	(1) Work tasks index	(2) Self-esteem index	(3) Life skills index	(4) Workplace skills index	(5) Likely find job w/in 6 mo. of grad.	(6) Lowest wage willing to accept	(7) Expected wage after graduation	(8) Expect to finish college or higher
Panel A: Aggregate treatment effects								
Enrolled in SPES	0.086	-0.001	0.048	-0.111	0.065*	-88.615	-186.233	0.005
	[0.072]	[0.078]	[0.073]	[0.075]	[0.037]	[84.396]	[168.568]	[0.017]
Panel B: Interacted by gender								
Enrolled in SPES	0.058	0.063	0.020	-0.058	0.080	-1.182	-66.165	-0.023
	[0.133]	[0.137]	[0.133]	[0.128]	[0.067]	[56.525]	[116.766]	[0.032]
SPES X Female	0.053	-0.084	0.042	-0.073	-0.027	-130.778	-185.284	0.045
	[0.158]	[0.167]	[0.159]	[0.158]	[0.081]	[135.456]	[271.673]	[0.038]
p-value, SPES + SPES X Female	0.190	0.823	0.481	0.159	0.239	0.283	0.305	0.253
Panel C: Interacted by education level								
Enrolled in SPES	0.095	-0.040	-0.012	-0.109	0.036	-73.272	-188.227	0.010
	[0.094]	[0.098]	[0.098]	[0.095]	[0.048]	[66.951]	[133.403]	[0.027]
SPES X College	-0.010	0.076	0.136	-0.024	0.071	-6.874	58.258	-0.009
	[0.142]	[0.153]	[0.144]	[0.147]	[0.074]	[69.346]	[137.591]	[0.030]
p-value, SPES + SPES X College	0.430	0.766	0.247	0.254	0.062	0.429	0.521	0.947
Observations	3280	3280	3280	3280	3101	3097	3097	3281
Mean, control group	0.000	-0.000	-0.000	0.000	0.653	344.889	585.949	0.945

Notes: All endline respondents included. All specifications include controls listed in Table 1 along with stratification-cell fixed effects. Work tasks index constructed based on reported experience with 11 office tasks: Microsoft Word, encoding, Excel, Powerpoint, photocopying, scanning, sorting, answering phones, bookkeeping, online searches, and using e-mail. Skill-specific effects reported in Appendix Table A13. Self-esteem is based on five items drawn from Rosenberg (1965), life skills is based on a seven-question index developed by the Philippine Bureau of Local Employment, and workplace skills is based on five questions drawn from Brea (2011). Each index normalized using mean and standard deviation of the control group. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

B.3 LATE estimates without baseline controls

Table B9: Impact of SPES on self-reported employment, LATE without controls

	(1) Currently working (formal)	(2) Looked for work	(3) Current monthly earnings	(4) Work hours weekly now
Panel A: Aggregate treatment effects				
Enrolled in SPES	0.036*	-0.030	138.560	1.398
	[0.021]	[0.035]	[111.920]	[0.885]
Panel B: Interacted by gender				
Enrolled in SPES	0.060	0.020	229.177	1.726
	[0.039]	[0.061]	[258.398]	[1.709]
SPES X Female	-0.037	-0.075	-134.791	-0.488
	[0.045]	[0.073]	[279.257]	[1.986]
p-value, SPES + SPES X Female	0.329	0.194	0.382	0.223
Panel C: Interacted by education level				
Enrolled in SPES	0.011	-0.079**	4.568	0.565
	[0.021]	[0.038]	[94.294]	[0.938]
SPES X College	0.068**	0.132***	364.309**	2.270*
	[0.031]	[0.048]	[174.869]	[1.189]
p-value, SPES + SPES X College	0.015	0.276	0.059	0.021
Observations	3281	3280	3278	3281
Mean, control group	0.056	0.216	218.532	2.114

Notes: All endline respondents included. All specifications include stratification-cell fixed effects only. *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table B10: Impact of SPES on self-reported education, LATE without controls

	(1) Enrolled in school	(2) Will enroll, 2017-18	(3) Grade Weighted Average	(4) Academic track, SHS only
Panel A: Aggregate treatment effects				
Enrolled in SPES	0.016	0.027	0.075	-0.092
	[0.020]	[0.024]	[0.082]	[0.062]
Panel B: Interacted by gender				
Enrolled in SPES	0.056	0.051	-0.093	-0.047
	[0.036]	[0.040]	[0.146]	[0.123]
SPES X Female	-0.059	-0.037	0.249	-0.064
	[0.043]	[0.049]	[0.172]	[0.142]
p-value, SPES + SPES X Female	0.888	0.610	0.105	0.123
Panel C: Interacted by education level				
Enrolled in SPES	0.040**	0.046**	0.034	
	[0.020]	[0.018]	[0.087]	
SPES X College	-0.065**	-0.054	0.116	
	[0.030]	[0.038]	[0.123]	
p-value, SPES + SPES X College	0.420	0.867	0.226	
Observations	3281	3269	3240	
Mean, control group	0.943	0.917	0.000	

Notes: All endline respondents included, column 4 restricted to students enrolled in grade 11 and 12. Grade-weighted average normalized using education-level and scale-specific means and standard deviations of the control group. All specifications include stratification-cell fixed effects only. **** p<0.01, ** p<0.05, * p<0.10*

Table B11: Impact of SPES on self-reported education and employment, LATE without controls

	(1) Enrolled and working	(2) Enrolled, not working	(3) Not enrolled, working	(4) Not enrolled, not working
Panel A: Aggregate treatment effects				
Enrolled in SPES	0.026	-0.010	0.009	-0.025
	[0.019]	[0.026]	[0.010]	[0.017]
Panel B: Interacted by gender				
Enrolled in SPES	0.046	0.009	0.014	-0.070**
	[0.035]	[0.047]	[0.020]	[0.031]
SPES X Female	-0.030	-0.029	-0.007	0.066*
	[0.041]	[0.056]	[0.024]	[0.037]
p-value, SPES + SPES X Female	0.450	0.514	0.530	0.853
Panel C: Interacted by education level				
Enrolled in SPES	0.005	0.035	0.005	-0.045***
	[0.019]	[0.027]	[0.009]	[0.017]
SPES X College	0.057**	-0.122***	0.011	0.054**
	[0.029]	[0.039]	[0.014]	[0.027]
p-value, SPES + SPES X College	0.033	0.029	0.289	0.753
Observations	3281	3281	3281	3281
Mean, control group	0.041	0.902	0.015	0.042

Notes: All endline respondents included. All specifications include stratification-cell fixed effects only. *** *p<0.01, ** p<0.05, * p<0.10*