

# THE IMPACT OF SHORT-TERM EMPLOYMENT FOR LOW-INCOME YOUTH: EXPERIMENTAL EVIDENCE FROM THE PHILIPPINES

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**Abstract**—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%) increase in employment eight to twelve months later. Although 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.

## I. 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 & Elder, 2010; Quintini & 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). 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%, more than twice the rate for the general labor force (Philippine Statistics

Authority, 2015). Among youth who are working, only 30% are in the formal sector (World Bank, 2016).

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 reenroll in secondary or postsecondary education are eligible for these primarily public-sector placements. Participants earn minimum wage salaries for 20 days to 52 days of work during school breaks.<sup>1</sup> DOLE provides a 40% 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 because 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 three 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 eight to twelve months after the program 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 noncognitive skills such as 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, such as motivation and persistence. Temporary, subsidized programs like SPES also reduce employers' cost of screening

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<sup>1</sup> After implementation of our study, a 2016 amendment (RA 10917, 2016) expanded the eligible age range to 15–30 and extended the program length to 20 days to 78 days of work.

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 & Gibbons, 1996; Altonji & Pierret, 2001; Pallais, 2014). Finally, the program itself may promote employment by exposing participants to the labor market or increasing participants' aspirations (McKenzie, 2017). For example, Beam (2016) and Abebe, Caria, et al. (2020) 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 eight to twelve months later by 4.4 percentage points (79%), and this gain is concentrated exclusively among postsecondary 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 twenty 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%), 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, p. 1). 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% of average baseline tuition. Although 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 to receive the 40% 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, because control-group enrollment rates are already high (94%).

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 & Elder, 2010; Quintini & 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 twenty 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, and 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, Fafchamps, et al., 2020). 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 (nine to twelve 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.

## II. 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% (Philippine Statistics Authority, 2018), and the tertiary enrollment rate has grown to 33% (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.<sup>2</sup>

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% 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)<sup>3</sup> 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, substantial heterogeneity is found in many aspects of program implementation.

Across the country most participants are placed in local government offices; 70% are employed at local government offices nationally, and private-sector employment makes up 8% of SPES employment (Bureau of Local Employment, 2017).<sup>4</sup> In our study 94% work in local government and 6% 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 postprogram 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 ensured students 20 days to 52 days of work, most participants in our study (73%) work only the minimum of twenty 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 twenty days remain the norm for public-sector positions in off-cycle years.<sup>5</sup>

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 such as surveying, encoding, and filing documents.<sup>6</sup> Nearly 14% report that their primary or secondary tasks are essentially make-work tasks

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

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

<sup>5</sup>Across study municipalities (and some non-experimental municipalities), 82% worked exactly twenty days in 2016 and 72% worked exactly twenty days in 2017.

<sup>6</sup>See appendix table A1 for more detail.

<sup>2</sup>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 ten-year basic education system. It developed 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.



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%).

We also document substantial payment delays, particularly for the 40% subsidy that DOLE provides. Although the law specifies that payment should be made within thirty days of the program’s end, appendix table A2 shows that 36% received their DOLE counterpart three months or more after program completion, and another 16% had not received it at all by the endline interview, eight to twelve 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 before enrollment.

Although these particular issues may be specific to SPES, implementation challenges are common 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.<sup>7</sup>

### III. 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.<sup>8</sup> We recruited PESOs that had the largest SPES programs in their region, based on 2014 enrollment.<sup>9</sup> Among those we contacted, 59% participated in the impact evaluation.<sup>10</sup> The main reasons for nonparticipation 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 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. Although 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% for participating versus 21% for nonparticipating). Additionally, participating programs are slightly more likely to report that the mayor’s office is involved in the selection of participants (32% versus 23%, 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.

#### A. 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% 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.<sup>11</sup> 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 reenroll 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 of six. These requirements are widely enforced across PESOs, though some impose additional screening criteria, such as passing a home visit, providing additional documentation, or passing a qualifying exam. 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% in

<sup>7</sup>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.

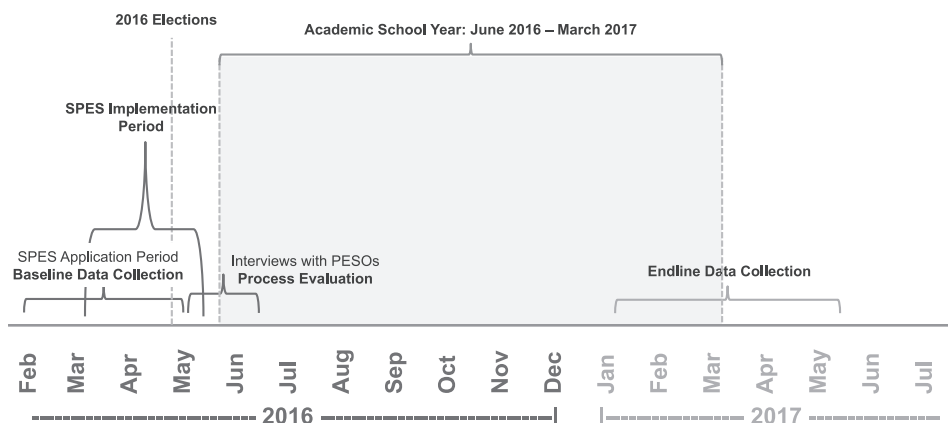
<sup>8</sup>Twenty-two offices are represented in our study, but one provincial office implemented separate recruitment and placement batches across fourteen municipalities, of which five municipalities participated. In effect, this creates 26 total participating programs, which were randomized in 31 batches.

<sup>9</sup>Specifically, we originally contacted the largest thirteen 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 one hundred participants in 2014.

<sup>10</sup>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%, or 22 out of 32 offices.

<sup>11</sup>See appendix figure A1 for the distribution of applicant ages by schooling level.

FIGURE 1.—STUDY TIMELINE



our sample versus 8% nationally), and we see that the majority of beneficiaries nationally also tend to work the minimum number of program days (73% in our sample versus 55% nationally). But we also find some differences. We cannot speak to the impact of placements in private education institutions, which make up 15% 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.<sup>12</sup> As a result, our sample overrepresents relatively younger beneficiaries, and 81% of our sample is aged 15–18, compared with 48% 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% of participants nationwide, who work in government and private-sector placements.

### B. 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 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 2016–2017 academic year in mid-June.

<sup>12</sup>While disaggregated data on program characteristics by employer type are not available nationally, data from a selection of institutions show that the median duration among students working at private education institutions was the program cap of 52 days.

We conducted an endline phone survey from January to 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% of the baseline sample. We attained a response rate of 87% 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 (6% of nonrespondents), 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.<sup>13</sup>

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% 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 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.

### C. Randomization

Among participating program offices, we randomly selected applicants from the pool of qualified, first-time

<sup>13</sup>See appendix table A6 for differential attrition by treatment assignment and appendix table A7 for covariate balance among endline respondents.

TABLE 1.—DESCRIPTIVE STATISTICS AND BALANCE TESTS

	All		Control		Treatment		<i>t</i> -test <i>p</i> -value (7)	RI <i>p</i> -value (8)
	<i>N</i> (1)	Mean/ Std. Dev. (2)	<i>N</i> (3)	Mean/ Std. Dev. (4)	<i>N</i> (5)	Mean/ Std. Dev. (6)		
Female	3,795	0.658 [0.008]	1,285	0.668 [0.013]	2,510	0.654 [0.009]	0.483	0.628
Age (mean)	3,701	17.185 [0.029]	1,229	17.076 [0.050]	2,472	17.239 [0.036]	0.601	0.571
College	3,795	0.502 [0.008]	1,285	0.394 [0.014]	2,510	0.557 [0.010]	0.549	0.775
Any past work experience	2,545	0.196 [0.008]	934	0.194 [0.013]	1,611	0.197 [0.010]	0.082*	0.064*
Formal work experience	2,545	0.072 [0.005]	934	0.070 [0.008]	1,611	0.073 [0.006]	0.536	0.602
Informal work experience	2,545	0.047 [0.004]	934	0.055 [0.007]	1,611	0.043 [0.005]	0.285	0.291
Lowest acceptable daily wage	2,155	325 [6.81]	825	310 [11.16]	1,330	335 [8.59]	0.436	0.427
Expected daily wage after graduation	2,205	530 [9.51]	842	504 [14.98]	1,363	546 [12.28]	0.620	0.612
Expected tuition next year	2,146	11,263 [403]	810	10,887 [350]	1,336	11,491 [611]	0.537	0.686
Expected educational expenses next year	2,108	9,529 [371]	811	9,186 [423]	1,297	9,744 [542]	0.193	0.136
Joint significance of all covariates							0.771	

All baseline respondents included. "College" includes 51 respondents enrolled at the vocational level. *p*-values in columns 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* < 0.10, \*\**p* < 0.05, and \*\*\**p* < 0.01.

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%) are treatment group members and 1,285 (34%) are control group members, and substantial variation was found in oversubscription rates by municipality.

#### D. 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% female) of participants nationally (Bureau of Local Employment, 2017). Although the program was open to youth ages 15 to 25, nearly all applicants (95%) 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% of participants are aged 22–25 (Bureau of Local Employment, 2017).<sup>14</sup> Our sample is fairly evenly divided between high school and postsecondary students (primarily college level, because only 1% are enrolled in vocational training at baseline), and out-of-school youth make up only 2% of our sample.<sup>15</sup>

Just under 20% of applicants have any past work experience, and few (7%) have any formal work experience. Although 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.<sup>16</sup> 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* = 0.771).

<sup>16</sup>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.

<sup>14</sup>See appendix table A4 for more details.

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

### E. Empirical Specification

Overall, 89% of treatment-group and 28% 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, 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% power is a 2.3 percentage-point increase in employment based on a measured control-group rate of 5.6% and an  $R^2$  of 0.12. Because of noncompliance, the adjusted MDE is 3.7 percentage points.

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

$$\begin{aligned} \text{spes}_{i,s} &= \alpha_0 + \alpha_1 \text{treatment}_{i,s} + f_s + X'b + v_{i,s}, \\ y_{i,s} &= \beta_0 + \beta_1 \widehat{\text{spes}}_{i,s} + f_s + X'b + e_{i,s}, \end{aligned}$$

where  $\text{treatment}_{i,s}$  is a binary indicator for whether the respondent was randomly selected to be invited to SPES and  $\text{spes}_{i,s}$  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  $\text{spes}_{i,s}$ , 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.<sup>17</sup> Because of substantial nonresponse at baseline, we recode missing values as zeros and add missing value flags.<sup>18</sup> 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 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 662. Column 2 uses administrative data on SPES enrollment, and the impact of assignment on the likelihood of enrollment is slightly larger, at 58%.

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 postsecondary). For each set of results, we include a binary indicator for being female or being at the postsecondary level, and we interact that indicator with our included covariates.<sup>19</sup>

## IV. Results

### A. Does SPES Increase Work Experience?

Only 5% of enrolled youth nationally, and 18% 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%, significant at the 1% level, and no detectable change is seen 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 P5670 (\$US 119), which is significant at the 1% level.

### B. 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 nonprofit organization approximately eight to twelve months after the program start. Column 1 of panel A shows that SPES increases the reported likelihood of working by a statistically significant 4.4 percentage points, a 79% increase relative to the control group employment rate of 5.6%. Overall we see no impact on the likelihood of having searched for work (measured since June

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

<sup>18</sup>Online appendix B shows that results are robust to excluding these covariates, although we have some loss of precision.

<sup>19</sup>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 nonenrolled students. Because the 2016–2017 academic year marked the roll-out of grades 11 and 12, very few students, if any, could progress from high school to postsecondary education during this time. However, exceptions could occur if students were in a K–12 pilot school or exited high school before graduation 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.



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, incl. SPES
Invited to SPES	0.508*** [0.020]	0.576*** [0.018]			
Enrolled in SPES			0.797*** [0.023]	−312.666 [245.954]	5,670.090*** [330.409]
Observations	3,281	3,281	3,280	3,280	3,273
Mean, control group	0.282	0.177	0.435	876.903	1,770.873
F-test statistic	662	1,021			

All endline respondents included. All specifications include controls listed in table 1 along with stratification-cell fixed effects. Earnings reported in Php. 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 nonzero SPES earnings. \* $p < 0.10$ , \*\* $p < 0.05$ , and \*\*\* $p < 0.01$ .

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** [0.020]	−0.024 [0.035]	177.152 [112.213]	1.747** [0.868]
Panel B: Interacted by gender				
Enrolled in SPES	0.079** [0.039]	0.026 [0.063]	217.219 [276.737]	2.243 [1.683]
SPES × Female	−0.047 [0.045]	−0.078 [0.076]	−68.530 [296.111]	−0.642 [1.957]
$p$ -value, SPES + SPES × Female	0.172	0.220	0.161	0.109
Panel C: Interacted by education level				
Enrolled in SPES	0.008 [0.022]	−0.082* [0.042]	−22.931 [89.868]	0.203 [1.077]
SPES × College	0.088** [0.041]	0.143** [0.069]	462.368* [246.194]	3.738** [1.740]
$p$ -value, SPES + SPES × College	0.009	0.290	0.063	0.005
Observations	3,281	3,280	3,278	3,281
Mean, control group	0.056	0.216	218.532	2.114

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. \* $p < 0.10$ , \*\* $p < 0.05$ , and \*\*\* $p < 0.01$ .

2016), and we can reject at the 95% level any increase greater than 4.5%.

Across the entire sample, including the 93% 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% level, with  $p = 0.044$ . Compared to the low control-group means, these estimates at first appear large: an 81% increase in earnings and an 83% 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% lower earnings and 20% 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 (7.9 percentage points versus 3.2 percentage points), but that difference is not statistically significant ( $p = 0.304$ ).

We also test for heterogeneous treatment effects by education level because 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 (ten percentage points versus one percentage point), and this difference is statistically significant at the 5% level.

Although we observe no aggregate change in the likelihood respondents had looked for work in the time since the program ended (column 2), panel C 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% 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,  $p = 0.290$ ), whereas 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.<sup>20</sup>

<sup>20</sup>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



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 (Php)	5,136	4,569	5,368
Hours/week	38.38	40.78	37.32
Share of employed students			
Private sector	0.79	0.81	0.78
Public/nonprofit sector	0.21	0.19	0.22
On-the-job training or internship	0.15	0.06	0.19
Salaried	0.44	0.40	0.46
“Regular” (i.e., 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 or 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

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 nonrandom sample), and column 3 provides characteristics for those who did not participate in SPES.

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% levels, respectively. If college students engage in different work tasks during the program, this difference in employment outcomes could reflect differing returns based on the type of work experience, which would be consistent with students gaining additional skills in certain types of jobs.

Differences are indeed found 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 such as 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. Although 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

evidence that SPES affects these perceptions in aggregate or for high school students in particular. (See appendix table A9 for results.)

month. Nearly 80% are in the private sector, and 23% 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%) 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%) and food service (20%), and general labor such as cleaning and construction makes up 15% of employment. Office or clerical work makes up only 9% of employment.<sup>21</sup>

### C. 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 statement if they do not enroll to receive the 40% 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, whereas Leos-Urbel (2014) finds small, positive impacts on attendance conditional on enrollment. However, SPES does increase the postprogram likelihood of employment, but these U.S. programs do not.<sup>22</sup>

In line with program expectations, 68% of SPES participants report that they used at least some of their earnings for tuition and other schooling expenses. Another 44% reported using earnings to support their family, and 35% 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% increase relative to a control-group enrollment rate of 94%. We can reject at the 95% 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. No change is seen 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

<sup>21</sup> See appendix table A11 for the full distribution of position types worked at endline.

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

TABLE 5.—IMPACT OF SPES ON SELF-REPORTED EDUCATION

	(1)	(2)	(3)	(4)
	Enrolled in school	Will enroll, 2017–2018	Grade-weighted average	Academic track, SHS only
Panel A: Aggregate treatment effects				
Enrolled in SPES	0.011 [0.019]	0.021 [0.023]	0.064 [0.082]	–0.096 [0.063]
Panel B: Interacted by gender				
Enrolled in SPES	0.074** [0.037]	0.073* [0.041]	–0.066 [0.151]	–0.031 [0.132]
SPES × Female	–0.089** [0.043]	–0.078 [0.050]	0.220 [0.179]	–0.091 [0.150]
<i>p</i> -value, SPES + SPES × Female	0.511	0.845	0.114	0.087
Panel C: Interacted by education level				
Enrolled in SPES	0.041** [0.021]	0.007 [0.016]	0.016 [0.091]	
SPES × College	–0.075* [0.041]	0.032 [0.053]	0.112 [0.166]	
<i>p</i> -value, SPES + SPES × College	0.355	0.453	0.381	
Observations	3,281	3,269	3,240	
Mean, control group	0.943	0.917	0.000	

All endline respondents included, column 4 restricted to students enrolled in grades 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. \* $p < 0.10$ , \*\* $p < 0.05$ , and \*\*\* $p < 0.01$ .

group.<sup>23</sup> 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 year of postsecondary 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% 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%) or TVL track (47%). 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.<sup>24</sup> Indeed, panel B shows that the treatment effect is entirely concentrated among men (7.4 percentage points,  $p = 0.044$ ), and we can reject the equality of coefficients between men and women at the 5% level ( $p = 0.040$ ).

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%) than high school students (93%). 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% level, and we can reject equality of coefficients between high-school-level and college-level students at the 10% level.

#### D. 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; see 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% 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), and SPES mainly shifts college students from being enrolled and not working to being enrolled and working.

<sup>23</sup>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.

<sup>24</sup>At endline, 93% of men in the control group are enrolled, versus 95% of women.

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* [0.018]	−0.021 [0.026]	0.012 [0.009]	−0.023 [0.017]
Panel B: Interacted by gender				
Enrolled in SPES	0.059* [0.035]	0.012 [0.048]	0.018 [0.018]	−0.090*** [0.032]
SPES × Female	−0.038 [0.041]	−0.049 [0.057]	−0.008 [0.021]	0.095** [0.038]
<i>p</i> -value, SPES + SPES × Female	0.326	0.213	0.309	0.796
Panel C: Interacted by education level				
Enrolled in SPES	0.009 [0.019]	0.032 [0.027]	−0.001 [0.011]	−0.040** [0.018]
SPES × College	0.059 [0.037]	−0.134** [0.052]	0.028 [0.020]	0.047 [0.037]
<i>p</i> -value, SPES + SPES × College	0.040	0.027	0.106	0.836
Observations	3,281	3,281	3,281	3,281
Mean, control group	0.041	0.902	0.015	0.042

All online 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. \* $p < 0.10$ , \*\* $p < 0.05$ , and \*\*\* $p < 0.01$ .

## V. Discussion

### A. 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, Caria, et al., 2020).

We do not find evidence of general skills acquisition or motivation channels, nor any detectable changes in job search likelihoods or methods. Although some youth continue working with their SPES employer (12%), 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. Although 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 eleven 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 eleven 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 ( $p = 0.229$ ).<sup>25</sup>

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 Ibararán et al. (2014) and Acevedo et al. (2020).<sup>26</sup>

The coefficients across these three self-reported noncognitive 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 Barbançon et al. (2020) find that a longer-term work experience in Uruguay does not affect soft skills.<sup>27</sup>

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

<sup>25</sup> 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 leads to an increase only in the likelihood respondents have experience answering phones (a sixteen percentage-point increase relative to a control-group mean of 39%).

<sup>26</sup> Appendix table A14 includes the specific life skills and workplace skills question items.

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



TABLE 7.—IMPACT OF SPES ON SKILLS AND LABOR MARKET PERCEPTIONS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Work tasks index	Self- esteem index	Life skills index	Workplace skills index	Likely find job within 6 months of grad.	Lowest wage willing to accept	Expected wage after graduation	Expect to finish college or higher
Panel A: Aggregate treatment effects								
Enrolled in SPES	0.098 [0.081]	−0.001 [0.089]	0.054 [0.083]	−0.126 [0.085]	0.074* [0.042]	−100.802 [96.054]	−211.845 [191.838]	0.006 [0.019]
Panel B: Interacted by gender								
Enrolled in SPES	0.070 [0.147]	0.075 [0.152]	0.023 [0.147]	−0.053 [0.142]	0.095 [0.076]	−0.393 [64.293]	−70.993 [132.844]	−0.025 [0.036]
SPES × Female	0.060 [0.176]	−0.097 [0.187]	0.048 [0.178]	−0.092 [0.178]	−0.033 [0.091]	−150.083 [154.758]	−215.221 [310.379]	0.050 [0.042]
<i>p</i> -value, SPES + SPES × Female	0.180	0.837	0.479	0.174	0.230	0.285	0.307	0.247
Panel C: Interacted by education level								
Enrolled in SPES	0.102 [0.101]	−0.042 [0.104]	−0.011 [0.105]	−0.118 [0.101]	0.040 [0.051]	−78.341 [71.700]	−199.967 [142.940]	0.010 [0.028]
SPES × College	0.003 [0.157]	0.084 [0.171]	0.158 [0.159]	−0.044 [0.165]	0.092 [0.083]	−22.306 [86.349]	34.382 [171.503]	−0.009 [0.032]
<i>p</i> -value, SPES + SPES × College	0.410	0.774	0.248	0.244	0.059	0.424	0.510	0.928
Observations	3,280	3,280	3,280	3,280	3,101	3,097	3,097	3,281
Mean, control group	0.000	−0.000	−0.000	0.000	0.653	344.889	585.949	0.945

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 eleven 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. \* $p < 0.10$ , \*\* $p < 0.05$ , and \*\*\* $p < 0.01$ .

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.<sup>28</sup>

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% 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 *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 seven percentage points more likely to say they will likely find a job after graduation (significant at the 10% level), an 11% 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% of

<sup>28</sup>See control group means in appendix table A13. Among the control group, more than 80% have experience with Word and PowerPoint, and more than half have experience with Excel. Nearly all have experience with online searches (93%), photocopying (83%), and encoding (72%). The only tasks for which respondents were generally inexperienced were answering phones (39%) and bookkeeping (33%).

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, Caria, et al., 2020), 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%. 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%), submitting a resume or CV (75%), and visiting an employer in person (55%). 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 or 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 and that it does not provide 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.<sup>29</sup> Evidence is found 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, because it is those in areas with lower oversubscription rates that see larger employment effects.

### B. 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% 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 postsecondary 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% is also high. Even if the program were maximally effective, increasing enrollment to 100%, then the cost of the program would still be \$1,323 per dropout avoided.<sup>30</sup> 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% of the control group did any work over the summer aside from SPES).

## VI. 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%), 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 (twenty 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% 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. Although 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 because the experimental design does not enable us to account for poten-

<sup>29</sup>The median oversubscription rate is 27%, with a range of 3% to 600%. See panel E of appendix table A15 for results.

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

tial 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, no detectable impacts are seen 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, because 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, although perhaps easier said than done, is crucial to developing and implementing programs to maximize their effectiveness.

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