

They Got Mad Skills

The Effects of Training on Youth Employability and Resilience to the Ebola Shock

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Abstract

This paper discusses a randomized control trial to measure the short-term impacts of a skills intervention among urban youth in Sierra Leone at the onset of the Ebola crisis. The intervention provided (i) technical skills training, plus on-the-job training; (ii) business skills training; and (iii) a mix of (i) and (ii). All groups received stipends and literacy and numeracy training. The findings support evidence that combining cash injections and skills training can stimulate employment and entrepreneurship. The program boosted household consumption and investments in housing and assets, thereby building resilience to the Ebola shock. The

effects on cognitive and noncognitive skills were positive and heterogeneous. Youth with greater initial ability experienced more positive labor market and entrepreneurship investment impacts. Youth with less initial ability upgraded skills more extensively, although they channeled benefits into more consumption. These findings emphasize the role of basic safety nets and show that noncognitive tests may improve the targeting of skills interventions in fragile contexts. The results also confirm the age-malleability of noncognitive ability and suggest that, in low-ability contexts, the sensitive years for skill investments may reach into early adulthood.

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They Got Mad Skills: The Effects of Training on Youth Employability and Resilience to the Ebola Shock

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1 Introduction

Youth employment is a central concern of governments and researchers across the developing and developed world. The latest global estimates indicate that youth are at substantially higher risk of unemployment, underemployment, and engagement in low-earning jobs, and many youth cannot afford not to work, leaving nearly two-thirds of youth globally trapped in working poverty (ILO 2015; United Nations 2015). In some contexts, such as Sub-Saharan Africa, youth employment challenges are exacerbated by persistently high fertility rates, which translate into a young, rapidly growing population in need of jobs (Canning, Raja, and Yazbeck 2015).

Among the working-age population in many low-income countries, micro- and small enterprises, often referred to as household enterprises, are the second most important source of income after agricultural work. In Sub-Saharan Africa, for example, 22 percent of all jobs are accounted for by household enterprises (Filmer and Fox 2014). In such contexts, efforts to create and expand household enterprises are critical to addressing youth employment challenges. This is particularly true in Sub-Saharan Africa, where the wage sector is relatively small and not expected to grow sufficiently to provide the jobs needed to accommodate this demographic youth bulge for a long time, even under optimistic scenarios (Fox and Sohnesen 2012).

Yet, household enterprises face constraints in starting up and growing. Capital and skills constraints are among those most prominently discussed in the literature (Blattman, Fiala, and Martinez 2014; Brixiová, Ncube, and Bicaba 2014). Some of these constraints are likely to be more pronounced if the enterprises are led by young people (Blattman, Fiala, and Martinez 2014). Fostering entrepreneurship and developing existing household enterprises may thus be a critical pathway to the generation of employment and earning opportunities among youth. However, the evidence on the most effective interventions in the effort to relieve the constraints and foster the creation and expansion of household enterprises is still relatively limited (McKenzie and Woodruff 2014). A recent review of evidence on employment programs finds that skills and microfinance interventions have demonstrated relatively little cost-effective results (Blattman and Ralston 2015). It also suggests that the lack of capital and skills may be acting as multiple binding constraints. The most promising strategy may therefore be to relax the constraints simultaneously through capital-centric skills interventions, but evidence of the effectiveness of such interventions is still nascent.

Recent literature on entrepreneurship has emphasized the identification of the traits of entrepreneurs with high growth potential and ways to spur this growth to generate large numbers of jobs. A key question the research has tried to tackle revolves around the profile of small-scale entrepreneurs and whether they possess the same entrepreneurial spirit as entrepreneurs operating on a larger scale. The evidence indicates that most of these entrepreneurs and wage workers have more in common, including noncognitive ability, than the owners of large firms (de Mel, McKenzie, and Woodruff 2010).¹ But less attention has been focused on how to influence entrepreneurial traits and, ultimately, increase income among lower-ability necessity entrepreneurs, that is, individuals in developing countries who start small enterprises out of necessity, for whom the plausible alternative is often salaried work (Calderón, Iacovone, and Juarez 2016; de Mel, McKenzie, and Woodruff 2010). This is highly policy relevant in low-income countries, such as Sierra Leone, where a substantial proportion of entrepreneurs are self-employed, have no employees, and are without viable alternatives, especially because there are limited wage jobs.

¹ Noncognitive ability is also referred to in the literature as personality traits or socioemotional skills. To distinguish from cognitive abilities, the terms noncognitive ability or noncognitive skills are used throughout the paper.

A focus on the link between noncognitive skills and labor market outcomes may be particularly useful in fostering household enterprise development and addressing youth employment challenges in fragile settings. This is so for three reasons. First, a large share of youth in fragile contexts missed out on early childhood interventions and failed to complete the formative years of education, which cannot easily be offset because the early years are critical in shaping cognitive and noncognitive abilities (Heckman 2008; Heckman and Masterov 2007). Nonetheless, noncognitive skills have been shown to be more malleable at later ages than cognitive skills, and enhancing the former may therefore help reduce deficits suffered earlier in life, including by increasing cognitive ability through cross-fertilization (Kautz et al. 2014).

Second, criminality among youth, an important concern in fragile settings, may be reduced by enhancing noncognitive ability, which is a strong predictor of violent and risky behaviors (Heckman, Stixrud, and Urzua 2006).² However, the collection of direct measures of impacts on these traits is still relatively rare in evaluations even of programs explicitly aiming to influence noncognitive skills (Sánchez Puerta, Valerio, and Gutiérrez Bernal 2016).

Third, the evidence on whether interventions that increase noncognitive abilities can affect employment outcomes is still limited (Blattman and Ralston 2015). Given that the vast majority of youth in these contexts are engaged in low-productivity activities, increasing noncognitive abilities could potentially lead to large economic gains to the extent that boosting these abilities improves productivity (Filmer and Fox 2014). Although most evidence on such links points to an association rather than a causal relationship, it is emerging that noncognitive skills are not only valued in the labor market, but are better than the intelligence quotient as a predictor of educational attainment and labor market success.³

Household enterprises, especially those operating at subsistence, may be more exposed to aggregate shocks than bigger and more well established businesses. External shocks can negatively affect entrepreneurial profitability if managerial ability is low (Mano et al. 2012). The vulnerability of enterprises to shocks is an acute problem in fragile contexts, where subsistence businesses make up the majority of entrepreneurial activity (Schoar 2010). However, research on shocks in low-income settings has mainly focused on the effects of negative events on consumption, expenditure, and agricultural income-generating activities; there is little evidence on the impact of shocks on nonfarm household enterprises (Hallegatte et al. 2016). Skills training associated with a small cash injection can enhance the short- and long-term ability of households to respond to shocks through at least three channels: (i) avoiding coping strategies that reduce consumption in the face of income and asset losses, (ii) allowing participants to diversify revenue, and (iii) building human capital relevant for business operations (Grimm, Knorringa, and Lay 2012; Hallegatte et al. 2017). Enabling physical and human capital investments and avoiding resource depletion may raise value added, which helps businesses adjust to shocks and graduate from subsistence (Bruton, Ahlstrom, and Si 2015; Grimm, Knorringa, and Lay 2012).

Using evidence from a randomized control trial of a skills intervention targeting youth in Sierra Leone, this paper seeks to contribute to the literature on youth employment opportunities through household enterprises in three ways. First, it seeks to contribute to a growing body of evidence aimed at identifying skills interventions that can be effective in improving household enterprise development and employment and to examine how gender may mediate the impact of these programs on labor market outcomes. Second,

² See Blattman and Ralston (2015) for a review of the literature; Blattman and Annan (2015) for evidence on Liberia.

³ See Kautz et al. (2014) for a review of this evidence; Díaz, Arias, and Tudela (2012) for evidence on Peru.

it seeks to contribute to the understanding of the process of skills formation, how skills link with household enterprise development, and the implications in targeting skills programs in low-income and fragile contexts. Third, it seeks to contribute to the understanding of the ways such interventions help build resilience to shocks.

Overall, the evaluation findings add to the recent evidence that combining skills training and cash injections, even small ones, can be effective in stimulating employment and entrepreneurship. The program increased overall employment (up by 3.1 percentage points from 12.6 in the control group), profits in the worst profitability month (25 percent), and the likelihood of first-time entrepreneurship (up by 4.1 percentage points from 15.8 in the control group). There are limited significant differences in impacts by gender, in line with most studies of this type (McKenzie 2017). The program also built household resilience to a large aggregate shock (Ebola), thereby boosting consumption by more than 50 percent and promoting investments in better housing conditions and in assets.

Based on the evaluation results, shaping noncognitive skills shows promise for youth in fragile states, but there is important heterogeneity of effects. Skills enhancements among higher-ability individuals were associated with positive labor market impacts, while lower-ability, poorer individuals experienced larger gains in skills formation, but consumed more rather than making productive investments. A similar expansion in cognitive ability was found across groups, all of which benefited from basic literacy and numeracy training, indicating that providing foundational skills may act as a second-chance parenting opportunity. This, combined with the heterogeneity of effects on skills, is indicative that, in economies with a generalized low level of skill formation, the sensitive period for investments in cognitive skill development may extend into early adulthood. The results emphasize the notion that skills interventions are not substitutes for adequate safety nets and points to important, dynamic policy trade-offs related to productivity gains and poverty reduction. The findings also confirm the literature on the age-malleability of noncognitive abilities and the synergies across various skill components.

2 The Evaluation Context and the Intervention

Youth employability and income-generating potential are key to household well-being in Sierra Leone, but a substantial share of the country's youth, especially in urban areas, are not employed. More than two Sierra Leoneans in five (41.3 percent) are youth, defined in Sierra Leone as people ages 15–35 years. According to the country's most recent labor force survey, more than half of youth are in the labor force (55.7 percent), but 26.9 percent are not in education, employment, or training.⁴ This problem is more acute in urban areas, where the latter share is 32.3 percent, while the youth employment rate is 37.7 percent (compared with 58.8 percent in rural areas). Urban youth with some secondary education exhibit one of the lowest employment rates in the country (27.2 percent). Among urban youth with some secondary education who are employed, more than half are self-employed (54.8 percent).

Skills are often mentioned as one of the key constraints on youth in accessing labor market opportunities and reaching their productive potential in Sierra Leone. Almost half of youth cannot read or write (46.5 percent), and the illiteracy rate is higher among young women than among young men (57.4 percent versus 32.9 percent). There is a large jump in earnings among people with some primary education relative to those with no schooling, and the jump at the highest levels of education is similar (Margolis et al. 2016).

⁴ Unless otherwise noted, the national labor data presented in this section are taken from Margolis et al. (2016).

However, in the middle of the education spectrum, the returns to education do not vary much, and the overall returns are low. Low returns to education typically signal that the constraint is in labor demand rather than labor supply (Hausmann, Klinger, and Wagner 2008). However, given the low literacy rates, it is likely that the low returns also reflect poor skills formation because of (i) the low quality of or inadequate learning content or (ii) the failure to teach the desired skills, such as noncognitive skills. Urban youth who have completed primary or started secondary school appear to face the largest deficit in market-relevant skills because they have lower employment rates relative to youth who have never attended school or who have incomplete primary educational attainment (23.8 percent versus 59.6 percent).⁵

Access to capital is essential for the creation and expansion of household enterprises, a key source of employment among urban youth. However, lack of access to capital is reportedly among the most important constraints to benefiting from job opportunities and starting up or expanding household enterprises. Close to half (46.7 percent) of individuals who work in nonfarm household enterprises are engaged in enterprises that are unable to borrow money to fill routine needs or cope with a negative shock. Among the unemployed, this is the main constraint to obtaining work or starting a business: almost 60 percent of the unemployed report this to be the case.

The Youth Employment Support Project (YESP), financed by the World Bank from approval in 2010 to closing in 2015, was introduced to relieve key constraints to household enterprise development and access to labor income more broadly among youth in Sierra Leone. In the absence of the project, the likelihood that the young participants would have been provided with similar benefits is slim: only 1.8 percent of young individuals report they have participated in employment support programs provided by the government, donors, or nongovernmental organizations.

The project's skills development and employment support component was designed as a series of programs targeted at youth with different levels of educational attainment to boost short-term employment opportunities and enhance the employability and incomes of youth. It involved skill development interventions that sought to (i) promote the creation or expansion of small youth-owned household enterprises in economically viable activities and (ii) improve the skills base and technical capacity of young people to make youth more employable.

One of these programs was targeted on urban youth ages 15–35 who possessed at least some secondary education and who had a business or were interested in pursuing business or employment opportunities.⁶ This program was implemented by Hands Empowering the Less Privileged–Sierra Leone (HELP-SL), a local nongovernmental organization, in Sierra Leone's five major urban centers (Bo, Freetown, Kenema, Koidu City, and Makeni). The interventions envisioned during program design included classroom-based training, on-the-job training, and facilitation of access to microfinance. The interventions lasted a total of nine months. The classroom-based intervention, which lasted six months, included the following:⁷

- Basic literacy and numeracy
- Basic financial literacy skills, including bookkeeping

⁵ An alternative argument is that these youth can more easily afford to remain unemployed and are waiting to find more well paid jobs. However, this is unlikely given the low share of workers in wage employment.

⁶ The intervention design limited program participation to those with at least some secondary school; however, in practice, the application only required participants to provide evidence of primary school completion.

⁷ The program was implemented in two rounds: the first covered 1,200 participants in 2012 and was intended as a pilot to smooth out operational issues; the second round, which included the impact evaluation, had a target of 2,400.

- Training in a trade of each participant's choice, for example, welding, catering, auto mechanics, solar engineering, catering and hotel management, electricity, electrical installation, tailoring and design, and building and construction
- Entrepreneurship and business development training with modules on (1) steps in business implementation, (2) marketing and market research, (3) accounting and cost calculation, (4) business registration and legal issues, (5) financing the business, and (6) creating a business plan.

According to the design, the participants were to be provided with opportunities to participate directly in the labor market following the classroom-based intervention. Technical trainees were entitled to receive on-the-job training consisting of placement for three months as apprentices with local master apprentices in small or large businesses related to their trades of choice, such as in a mining company or in a small automobile mechanic shop. Similarly, following their classroom training, those receiving business skills training were to be provided with facilitation to access microfinance. This included support in developing a business plan, establishing business groups or cooperatives, obtaining microfinance loans from the HELP-SL finance unit, and follow-up support in creating and maintaining a business once the loan had been received. Participants were informed that the access to microfinance was not guaranteed and was contingent upon the quality of the business proposal they submitted. However, they were informed that they would receive technical support during the microfinance application process.

Based on these intervention elements, three intervention packages were designed, as follows: (i) a technical skills package consisting of technical training in a trade and on-the-job training, plus basic financial literacy; (ii) a business skills package consisting of entrepreneurship and business development training, plus access to the microfinance facilitation experience; and (iii) a package combining (i) and (ii). Basic literacy and numeracy training was provided to participants whom trainers had identified as lacking in reading and numeracy skills, irrespective of the package.

HELP-SL selected 13 technical and vocational education centers or vocational training centers (VTCs) to deliver the technical skills training, while HELP-SL delivered the business skills training because the VTCs had no experience in delivering this training content. HELP-SL also provided training materials to the VTCs. All training sessions were conducted at the VTCs, and the same curriculum was used in all VTCs to harmonize the content. Prior to program implementation, a rapid market assessment was conducted in each of the program districts to identify the market needs in technical skills and match these with the courses offered in the VTCs.

The training was delivered at no cost to participants, and the trainees in all three groups were given a daily stipend of Le 2,500 (equivalent to U\$0.60) during the course of the training to cover food and transportation expenses, conditional on regular attendance at the training sessions. Assuming perfect attendance, this would translate into approximately Le 360,000 (U\$80) over the nine-month training period and Le 200,000 (U\$44) over the six-month classroom training period, respectively, for the technical and business skills training sessions.⁸ The intervention's influence in relieving capital constraints was reduced to the cash stipend because the Ebola crisis disrupted the microfinance facilitation element (see below).

⁸ Each week, the participants in the technical skills training sessions were expected to attend courses for five days, while the participants in the business skills training sessions were expected to attend for three days. Perfect

Daily attendance among trainees was tracked and recorded by the instructors at the program VTCs using a monitoring form designed for all types of training. This was closely monitored by HELP-SL data entry clerks and supervised by youth development officers of the National Youth Commission. To encourage training completion, follow-up phone calls were made to trainees who attended irregularly.

The YESP faced a complicated and unexpected implementation challenge between March 2014 and June 2015, when more than 14,000 cases of Ebola virus disease were reported in Sierra Leone, and the disease took the lives of more than 3,900 people (WHO 2015). Four YESP beneficiaries died during program implementation. The Ebola crisis exerted an influence on the program through at least two channels: (i) labor supply and demand and (ii) productivity, although, because of the randomized design, these are not expected to have introduced any bias into the evaluation. First, Ebola had an impact on employment rates (Glennerster et al. 2015). Urban areas were particularly affected. In Freetown, the recorded decline in employment reached 9 percentage points at the height of the outbreak in November 2014. Youth were among those hardest hit (Fu et al. 2015; Glennerster et al. 2015). Relative to the previous year, self-employed individuals in nonfarm household enterprises also reported a 54 percent drop in revenues around the time the endline survey for this evaluation was collected (Glennerster et al. 2015). Evidence suggests these employment declines resulted primarily from a reduction in labor supply among people engaged in household enterprises. The Ebola crisis also weakened labor demand, evidenced by the declines in wage employment. Labor productivity was likewise affected because the government imposed movement restrictions and market closures and prohibited large gatherings, which limited the access of workers to their workplaces, clients, and markets.

During the implementation of the YESP, the economy suffered another negative shock aggravated by the Ebola crisis: falling prices for iron ore, the country's main international export (World Bank and SSL 2016). This price decline affected the economy and the labor market by directly reducing employment in mines, cutting revenues among suppliers, and decreasing government income at a time when it was most needed to help recover from the Ebola virus disease emergency.

The Ebola crisis and the iron ore price shock influenced program implementation and thus had a potential impact on the program's effectiveness. Because of worsening economic conditions, the effect of the program on employment and entrepreneurship may have been muted. Additionally, the benefits of practical training in situ were hindered because the restrictions imposed to reduce the spread of the disease led to the closure of some of the businesses where program participants had been placed for on-the-job training. The Ebola outbreak also affected implementation by disrupting the microfinance facilitation aspect of the intervention. Because of the Ebola crisis, the default rate for prior borrowers rose dramatically, which strained HELP-SL's resources and led the organization to be more cautious in approving microfinance loans. In practice, few of the beneficiaries who were entitled to receive microfinance facilitation actually received loans (20 percent). The stipend provided during the program thus became the primary cash component of the intervention.

attendance would thus represent a monthly stipend of 36 percent and 22 percent of the median earnings at baseline for people in technical skills training and business skills training, respectively. The average exchange rate of the U.S. dollar to the Leone during implementation (December 2013–December 2014) is used here throughout.

3 Program Eligibility, Experimental Design, and Data

Eligibility to participate in the program was based on age, location, educational attainment, interest in pursuing entrepreneurial activities, and the submission of a short business plan that had to pass a basic quality screening. HELP-SL and its partners conducted an intensive communication campaign to encourage all youth to apply who were ages 15–35, had some secondary education, were engaged in or interested in undertaking an entrepreneurial activity, and resided in one of the five urban areas where the program was implemented. Prospective applicants were required to present their birth and school certificates in person during registration at the nearest VTC. Participation and eligibility were determined based on the program budget and the impact evaluation design, as follows (appendix A, figure A1):

Step 1: All applicants (5,250 in total) were invited to a two-day introductory seminar organized by HELP-SL, with the support of the World Bank. At the seminar, applicants were given brief training on how to prepare a business proposal, together with the template for the business proposal, and were required to submit a short proposal on this basis. Applicants were also informed that the final selection of beneficiaries would be determined by the quality of their proposals and that those applicants passing the minimum quality threshold would be assigned to one of the training packages (or to a control group receiving no benefit) according to the results of a public lottery.

Step 2: Business proposals produced by participants were subjected to a quality screening gauged by criteria established by the HELP-SL finance unit. The criteria aimed to ensure that the business ideas in the proposals were clear, the business location was well defined, customers were clearly identified, and the inputs required were defined. A total of 3,854 of the 5,250 applicants who submitted proposals were shortlisted. More than half of those shortlisted (50.7 percent) were women.

Step 3: The 3,854 shortlisted youth were invited to take part in a public lottery carried out by Statistics Sierra Leone at the VTCs where the applications were submitted; of these, 3,319 attended and were randomly assigned to four groups (49.5 percent were women). The evaluation design relied on a random assignment based on oversubscription. A maximum of 2,400 spaces were allotted for training. The sample was stratified by gender to achieve the target of a minimum of 40 percent of participation among women and facilitate gender-disaggregated analysis. The random assignment resulted in 2,376 youth assigned to one of three treatment groups, while the remaining 943 were assigned to the control group, as follows:⁹

- Treatment A: this group received the technical skills package (791 participants)
- Treatment B: this group received the business skills package (791 participants)
- Treatment C: this group received package A, plus package B (794 participants)
- Control: no treatment (943 remaining eligible participants)

Treatment A participants received technical classroom training five half-days a week between December 2013 and May 2014. Treatment B participants attended the business skills training sessions three half-days a week from April to September 2014. During the months in which the technical and business skills training overlapped, treatment C participants attended three full days per week; during the remainder of the program they experienced the same training intensity as treatment A participants (appendix A, figure A2).

⁹ The initial design envisioned randomly assigning 800 individuals to each treatment group and 1,200 to the control group. However, fewer youth than expected applied, and therefore the numbers assigned to the control group were reduced; 800 were indeed assigned to the treatment groups, but a few participants did not take up the training.

Following completion of the technical training, treatment A and C individuals received a certificate and then graduated from the program after conclusion of the on-the-job training sessions. Participants in groups B and C were invited to form business clubs composed of members of their own choosing from the pool of program beneficiaries. The aim of the business clubs was to raise repayment rates, although group liability was not enforced because individuals who may have participated in a club could establish their own businesses and obtain secure microloans individually. After the business club participants in treatment groups B and C had completed their training, HELP-SL was to assist them through microfinance facilitation. However, the Ebola crisis disrupted the implementation of this initiative, thereby limiting the intervention's relief of capital constraints to the cash stipend provided to participants in all three treatment groups.

Competing hypotheses posited by local and international policy makers about what interventions might be effective in relieving constraints to youth employment informed the design of the three packages, although the lack of a labor force survey since before the civil war limited knowledge about the current labor market dynamics. The first hypothesis was that youth lacked the job-specific technical skills that were necessary if they were to be absorbed in the labor market, preferably in wage work. The second was that youth lacked the combination of entrepreneurial skills and capital that would enable them to engage in a business on their own, for example, by creating and expanding a household enterprise. The third was that capital was the binding constraint, and the impact of a capital infusion should therefore be tested alone against the impact of training; however, testing this hypothesis was infeasible from a political economy and operational perspective.

The impact evaluation baseline data collection was carried out between August 26 and November 14, 2013, during program enrollment and covered the 3,319 shortlisted individuals who participated in the lottery process. The randomization yielded balance between the treatment and control groups (appendix B). Of the 23 variables tested, only three main differences were found between each of the treatment groups and the control group.¹⁰ The endline survey was conducted in June 2015, less than a year after the end of the interventions (six months after the end of the business skills training and nine months after the end of the technical skills training). The evaluation therefore provides evidence of the program's impacts in the relatively short term. Of the 3,319 people who took part in the lottery and in the baseline survey, 2,782 were interviewed at endline—50.1 percent of whom were women—and thus have been included in the analysis (see section 4a on attrition).

The evaluation instruments were translated into Krio, the language most widely spoken in the major urban areas of Sierra Leone. The baseline survey was administered to each participant at the VTCs where the applications had been submitted, while endline data were collected at the homes of beneficiaries. The endline survey relied on computer-based personal interviews, while the baseline survey was paper based. Because of the randomized design, these differences should not have introduced any bias in the results.

¹⁰ The baseline report examines the baseline status of the participants in the treatment groups and the control group.

4 Empirical Strategy and Threats to Validity

The analysis relies on the intent-to-treat (ITT) estimate through an analysis of covariance regression, as follows:¹¹

$$Y_{t,ij} = \alpha_j + \beta_1 * Treat_i + \beta_2 * (Y_{t-1}) + \beta_3 * X_{t-1,ij} + C_j + \varepsilon_{t,ij}, \quad (1)$$

where $Y_{t,ij}$ are the outcomes of interest for an individual i in center j at follow-up (t). Y_{t-1} is the outcome for individual i at baseline; C_j captures time-invariant center effects; and $\varepsilon_{t,ij}$ is the error term. The estimated coefficient β_1 captures the effect of the random assignment, that is, the offer to participate in the program or the lack of such an offer. The equation includes fixed individual characteristics, $X_{t-1,ij}$, captured at baseline to increase the precision of the estimates. An analysis of covariance regression specification was used because it is more efficient than the postestimator or the difference-in-difference estimator; in particular, it improves power, especially in cases where baseline and follow-up outcomes have low autocorrelation, which is the case with a number of the outcomes of interest in this study (McKenzie 2012).

Beyond the overall treatment effect, the study sought to test differential impacts by gender. For ease in the interpretation of the results, equation (1) was reestimated for two subsamples (women and men), instead of multiple interaction terms. To test the relative effectiveness of the various treatments, the following specification was used, and an equality of treatment hypothesis was then tested:

$$Y_{t,ij} = \alpha_j + \beta_1 * Treat1_i + \beta_2 * Treat2_i + \beta_3 * Treat3_i + \beta_4 * (Y_{t-1}) + \beta_5 * X_{t-1,ij} + C_j + \varepsilon_{t,ij}. \quad (2)$$

The ITT coefficients compare the outcomes among individuals who were originally assigned to receive the treatments with the outcomes among the individuals who were assigned to the control group, regardless of whether the individuals actually took part in the program. These estimates are therefore conservative if the impacts observed are positive; they are therefore a lower bound of the effect of the program among those who attended training sessions and participated in program activities. Thus, if the program increased employability and achieved other desired outcomes, the effect would be larger among those who actually participated in the program than among those reported in this paper.

a. Attrition

At endline, Statistics Sierra Leone was able to track 2,782 of the 3,319 individuals surveyed at baseline, putting the overall survey attrition rate at 16 percent (Appendix C).¹² Over 80 percent of individuals were successfully interviewed at both baseline and endline. To check for bias resulting from attrition, Appendix C presents t-tests of differences in means between individuals who left the sample (attritors) and individuals who stayed (nonattritors). Most of the differences between attritors and nonattritors arise from variables with zero or a low number of observations among attritors or nonattritors and thus constitute a small share of the sample.

¹¹ Conditional regressions do not control for outcomes at baseline for two main reasons: (a) the loss in sample size beyond the loss from conditioning based on employment or on engagement in a household enterprise, particularly because of a labor market weakened by Ebola, and (b) key measurement changes to the endline survey after the first national labor force survey was conducted in 2014. Regressions using consumption and business practices as outcome variables do not control for outcomes at baseline because the relevant data were not collected at baseline.

¹² McKenzie and Woodruff (2014) examine survey attrition rates in studies on business training. They find a range of between 5.3 percent and 28.0 percent; the rate in about half the studies is higher than the rate in this evaluation.

To ensure that the evaluation captures the impact of the program rather than the impact of the characteristics that differ across the groups resulting from attrition, the balance tests were reestimated across the treatment groups and the control group without the attriters (Appendix C, table C3). The groups remain similar, on average. There were no differential patterns among the nonattriters in the treatment and control groups or among the key variables tested, and the only variable with a statistically significant difference in means also differed at baseline (self-reported ability to read or write a simple text).

Although no worrying trends were observed, a set of standard individual characteristics are included as control variables in all the econometric estimations. As an additional robustness check, an inverse probability weighting approach was used to conduct attrition corrections (Wooldridge 2000). This was done by estimating a probit model to predict the probability of staying in the panel and to calculate inverse probability weights, which are used to reweight the sample, giving more weight to individuals with the highest chance of attrition. More details on the methodology and results, following Adoho et al. (2014), are presented in Appendix C, along with the main unadjusted ITT results for comparison. The results show a high degree of similarity between the original (unadjusted) and the adjusted estimates. Across all outcomes, the point estimates and standard errors vary only slightly and do not change the overall results.

b. Compliance and take-up

There are potential compliance and take-up issues at two stages of the program: (i) participation in the random assignment (selection) by shortlisted applicants and (ii) compliance with the random assignment, or program take-up for those assigned to one of the treatment groups. Take-up of the random assignment was 86 percent (a total of 3,854 applicants were shortlisted to participate in the random assignment by public lottery, but only 3,319 actually attended). This discrepancy may have been partly caused by the 3.5 month lag between application and selection. The take-up at this stage did not affect the internal validity of the study because the random assignment was carried out only among those who attended the lottery. However, there are external validity considerations. For example, if those who did not attend the lottery had better networks and were therefore able to find alternative training or work opportunities, the impacts would be underestimated.

Take-up was also high for a program of this type. Survey and administrative data indicate that around 10.0 percent of those assigned to one of the treatment groups did not take up the training, and, at most, 6.1 percent of the control individuals participated in the training sessions. To ensure robustness, estimates of the local average treatment effect were also produced to examine the impact of the treatment on the treated: those individuals whose treatment status was influenced by the random assignment, that is, the compliers (Angrist and Imbens 1995; Angrist, Imbens, and Rubin 1996). The original assignment is used as an instrumental variable for treatment, where treatment is defined as a binary variable that takes on a value of 1 if the individual attended at least 75 percent of the training and 0 otherwise. As expected based on the low levels of noncompliance with the random assignment, the results are similar to the ITT estimates. Given this outcome and the large amount of data already presented, only the ITT estimates are presented here.

Various program design features may explain the high overall take-up, the low drop-out rates, and the differential take-up across treatment groups among youth who were part of the random assignment. First, the program required applicants to prepare a short business proposal, which may have acted as a signal of commitment to the program (Blattman, Fiala, and Martinez 2014). Second, individuals assigned to the treatment who attended less frequently were contacted by phone by HELP-SL to encourage participation.

Third, implementation progress reports indicated that youth assigned to treatment group B, which received business skills training only, were more likely to drop out and exhibited lower attendance relative to individuals assigned to the treatment arms providing technical skills training. Anecdotal evidence indicates this could be explained by (i) the perception that technical training was more tangible and therefore more marketable, as participants expressed their main motivation in joining the training sessions was to obtain technical skills training; (ii) an unintended consequence of the design if the three-month lag between the start of the technical skills training and the business skills training led some individuals assigned to treatment group B to seek alternative training or economic activities; and (iii) the focus of beneficiaries on the size of the stipend because treatment B was shorter in duration than the other arms.

Analysis shows that gender is not a predictor of attendance. In estimating the probability of high attendance (measured as taking part in 75 percent or more of the days of training), after controlling for age, marital status, number of children, and city of residence, the coefficient of gender is not statistically different from zero. This means that, under similar conditions, men and women are equally likely to attend the training. Estimations by gender suggest that, while marital status is not a strong predictor of women's attendance, married men are significantly less likely to attend.

c. Spillovers

Ideally, the randomization in this study should have been conducted across the VTC centers rather than among individuals to avoid the effects of information sharing among people at the same centers. However, because the number of centers was small (13), a randomization by center was not appropriate. Instead, randomization was conducted at the individual level, and measures were put in place by HELP-SL to minimize the likelihood that people assigned to different treatments would meet at their centers. To accomplish this, separate class sessions were held at different times of day. There was no concern about spillovers from other intervention elements (such as access to on-the-job training) because these elements were offered strictly to those assigned to treatments A, B, and C and were not transferable. There was also minimal or no concern about general equilibrium effects given that the number of beneficiaries was only slightly above 0.5 percent of the total economically active population of the country and only 1.2 percent of the urban youth in the labor force in the five districts where the program was implemented.¹³

5 Results

This section provides an overview of the impact of participation in the intervention as reflected in a variety of outcomes. Impact is defined as the difference in the change in outcomes observed among the treatment group(s) between the baseline and endline surveys and the corresponding change in the control group after controlling for the value of the outcome at baseline. All estimates are ITT coefficients, which compare the outcomes among individuals who were originally assigned to receive the treatments with the outcomes among the individuals who were assigned to the control group, regardless of whether the individuals actually took part in program activities.

The analysis examines the effects of the program through the lens of ultimate and proximate outcomes (appendix A, figure A3). The former are understood as the main outcomes of interest, and the latter as the channels through which the program is expected to influence the ultimate outcomes. Unconditional

¹³ The available data do not permit disaggregated estimates on urban youth in the labor force in the program cities to assess the potential for local spillovers. However, these estimates are considered reasonable given the near perfect overlap among urban areas in these five districts and the cities where the program was located.

estimates (the full sample) were derived for most outcomes; however, for a limited number of variables for which unconditional results are difficult to interpret, the results are conditional on employment or owning a business, with the caveat that these results are not necessarily of a causal nature.¹⁴ All impacts reported are statistically significant at the 10 percent level or lower unless otherwise indicated.

a. Ultimate outcomes

Employment

The program increased employability, measured by whether individuals were currently deriving positive work incomes. Treated individuals were 3.1 percentage points more likely to be employed, a substantial impact considering only about 12.6 percent of the control group was employed at endline. The positive impact on overall employment was larger among young men, who were now 4.4 percentage points more likely to be employed, compared with 1.8 percent among young women (appendix A, table A1, panel I). However, the equality of these effects by gender cannot be rejected. The greater impacts observed among men are consistent with the fact that, at baseline, more women had been self-employed in the 12 months previous to the survey (19.2 percent versus 12.6 percent among men) and that self-employment was the main source of employment among Sierra Leonean youth and was therefore expected to be most affected by the program. The impacts hold across treatment groups; only treatment groups B and C show significant impacts, but the equality of impacts across groups cannot be rejected (appendix A, table A1, panel II).

The program also changed the sources of labor income among youth. The share of men engaged in self-employment more than doubled, from 1.6 percent to 4.0 percent. This change was accompanied by a change in men's aspirations: the gap between the proportion of men and women who would choose a job in nonfarm self-employment narrowed from 12.7 percentage points to 10.2 percentage points.¹⁵ This suggests the program helped disrupt gender norms around this type of work by changing men's perceptions of its desirability, an important change, especially given the limited labor absorption potential of the wage sector. There was no significant change among women or overall; however, the equality of effects across gender cannot be rejected.

Not only did the program expand employment opportunities, but it also improved the quality of the jobs among those working as measured by earnings. Labor earnings among paid workers rose by 32.1 percent.¹⁶ The impacts among women were larger than and significantly different from the impacts among men. The earnings among women grew an outstanding 63.9 percent, while there were no impacts among men. Because of the small sample of individuals in wage work, it was not possible to measure whether this was a result of increased wages among those people who were already in wage work prior to the program, a shift into more wage work, a change in the type of businesses being run (see below), or some combination of these. All treatments had similar effects on labor earnings; only treatment groups A and C showed significant impacts, but the equality of impacts across groups cannot be rejected.

¹⁴ The concern with using conditional analysis (earnings, hours worked, type of business, business location, recordkeeping) is that there may be differential selection effects by treatment group because of, for example, differences in occupational choice, which may introduce bias into the randomized design. Balance tests on observables using the conditional sample find no such differences (available upon request).

¹⁵ The other possible responses were agricultural self-employment and wage work. In the control group, almost 30 percent of women but only 17 percent of men would have chosen nonfarm self-employment as their preferred job.

¹⁶ Unconditional estimates of earnings show no change as a result of the project. Balance tests on observables using the conditional sample find no such differences.

Entrepreneurship

The program had modest success in promoting entrepreneurship among youth (appendix A, table A2). The likelihood that participants became first-time entrepreneurs, that is, operated their own businesses, including family businesses, for the first time, increased by 4.1 percentage points relative to nonparticipants (15.8 percent). The impacts were larger and statistically significant only among men, although the equality of effects by gender cannot be rejected. These findings are consistent with the self-employment results and the fact that men in Sierra Leone engage less in nonfarm self-employment as a primary job than women, allowing men more margin for entry into this area.

The program had a positive and significant impact on volatility, as proxied by the lowest monthly profits derived by household enterprises in the previous 12 months. Treatment increased profits in the worst month by 24.7 percent; indeed, several studies have emphasized that training programs may have the strongest influence on profits during bad months, thereby reducing profit volatility (McKenzie and Woodruff 2014). However, this result appears to be explained by a greater understanding of enterprise finances among participants because the program also raised the likelihood that financial records would be kept, and the improved recordkeeping seems to have influenced how profits were measured (see below). Following the approach used by de Mel, McKenzie, and Woodruff (2014), financial recordkeeping was included as a control in the treatment regression; this showed that financial recordkeeping was positively associated with profits and, once this control is included, the impact observed on profits loses its significance.

The program also shifted the types of businesses in which youth are engaged, resulting in a distribution that resembles more closely that of the general population. According to the latest nationally representative labor force survey, most household entrepreneurs in Sierra Leone are traders or shopkeepers (84.0 percent), compared with 75.4 percent of the youth in the control group. The program increased the probability of involvement in trading or shopkeeping businesses by 7.9 percentage points, which means that 83.2 percent of the household enterprises owned by youth were of this type (appendix A, table A2, column 5). However, this outcome is driven purely by women because no change among men was observed, and the effect is significantly different by gender. This aligns with the high concentration of women in self-employment in the program's absence, and the effect was thus expected to be seen in the profile of existing businesses. However, because it relies on conditional estimates, this result should be interpreted with caution; similar effects are not found in the unconditional estimates, possibly because of the large proportion of households not engaged in any household enterprise.¹⁷

An examination of the impacts across treatment groups suggests that the entrepreneurship results are driven by treatment group B, which is consistent with the business skills content of this intervention package. The impacts by treatment are significantly larger among group B than among group C in terms of the likelihood of first-time entrepreneurship and raising profits during bad months. This result suggests that the business skills training package was more effective than the full package at promoting entrepreneurship.

¹⁷ Based on balance tests, the conditional results show no evidence of differential selection bias. Thus, the restricted sample is balanced on observables across treatment groups.

Resilience to aggregate shocks

While the YESP was originally conceived as a series of youth-targeted social protection interventions in response to the food, fuel, and financial crisis, the skills training was not designed as a safety net, but rather as an instrument to enhance youth employability. However, the Ebola outbreak amplified the importance of the program's role in building household resilience to shocks. The evaluation therefore measured the program impacts on household consumption and investments (appendix A, table A3).¹⁸

Large positive effects on household welfare as measured by consumption were observed (appendix A, table A3, panel I, columns 1–4). These effects were significantly larger among households in which the beneficiaries were women. Overall monthly consumption per capita grew by 52.5 percent, compared with about SLL 300,000 in the control group (about US\$70 at prevailing exchange rates). This is driven by remarkably large effects in all three components of consumption measured: food items (46.4 percent), nonfood items (51.6 percent), and lumpier bimonthly household expenditures (47.1 percent). Households in which the beneficiaries were women experienced much higher increases in overall per capita consumption, in the range of 60–70 percent, while male participants also registered positive, albeit narrower results, in the range of 20–35 percent.

The program also provided a small boost to household investments in assets and living conditions. The presence of a program participant in a household raised the number of assets owned to 4.4, compared with 4.2 in the control group, which, although a small difference, was remarkable given the prevailing economic conditions.¹⁹ It also increased the likelihood of improved sanitation facilities by 3.1 percentage points, which is nonnegligible given that only 20.3 percent of households in the control group had such facilities. However, no impacts were found on other measures such as access to piped water or residence in a house with a concrete roof.

b. Potential channels of impact

In light of the program's goal of reducing the constraints on skills and capital to spur employment and entrepreneurship, the observed impacts on employment and entrepreneurship were expected to flow through two main channels: skills and capital. The impacts on skills were captured through measures of cognitive skills, measures of noncognitive skills, and measures of business practices. The causal chain between employment or entrepreneurship and skills development can evidently also run in the opposite direction if, for example, the intervention allowed participants to benefit from work experience that enhanced their noncognitive ability. However, the analysis suggests this is not likely to be the case given that the evaluation examines the impacts in the relative short term, and the analysis of heterogeneity by skill suggests that participants with higher initial ability benefited more from employment, but less from the skills training (see below). Impacts on capital are measured by business spending, business loans, and capital stock.

¹⁸ All consumption results are robust to estimations in levels rather than logarithms as well as to other specification changes. Differences across groups are not reported because the treatment arms were not designed with consumption in mind.

¹⁹ Household assets include sofa; electric, gas, or kerosene stove; radio; cassette recorder; freezer or refrigerator; generator; and electric or charcoal iron. The variable is measured as the total number of assets reported by individuals.

Cognitive skills

Overall, the program enhanced cognitive skills as measured through verbal working memory.²⁰ Verbal working memory—measured through the forward digit span recall test—was positively affected by the program.²¹ The proportion of participants who could recall six or more digits rose by 6.3 percentage points (appendix A, table A4, column 1). This result is robust to controls for the status of the variables at baseline and accounts for the fact that more than half the participants (84.5 percent) could already recall six or more digits at baseline. This suggests that the program may have influenced creativity among youth because the digit span test has been found to be positively associated with the likelihood of innovating (de Mel, McKenzie, and Woodruff 2009). The gender disparity of 19.6 percentage points in verbal memory among the treatment groups at baseline fell to 13.8 percentage points as a result of the program; however, the equality of the impacts by gender cannot be rejected.

Cognitive skills have been relatively stable in other interventions, raising the question of whether the observed results may have been influenced by an effort effect, for example, if treated individuals exert more effort in answering survey questions on cognitive ability at endline to show their gratitude for being selected as beneficiaries. While this possibility cannot be entirely refuted, the impacts on cognitive skills observed match expectations given that participants facing difficulties in reading, writing, and analyzing figures in all groups received training in basic literacy and numeracy. The fact that the equality of impacts across groups cannot be rejected also points to the relevance of building foundational skills, including through basic literacy and numeracy, among youth who may have failed to acquire these skills because of low-quality education systems (Filmer and Fox 2014). In addition, the program was designed as a long-term, full-time intervention with a duration similar to a full academic year, as opposed to most skills training programs designed as high-intensity short-term courses (McKenzie and Woodruff 2014). In a context of generally low skills, this treatment intensity could have plausibly affected cognitive skills.

Noncognitive skills

There has been great interest in noncognitive skills in the recent literature on human capital development and entrepreneurship, particularly the notion that entrepreneurial ability should take into account other attributes beyond cognitive capacities. Following de Mel, McKenzie, and Woodruff (2010), who draw on the field of psychology to measure entrepreneurial traits, the evaluation therefore captured four main aspects of noncognitive skills: (i) impulse control, measured by planning, the speed of decision making, and savings behavior; (ii) polychronicity or the ability to juggle multiple activities; (iii) internal locus of control, which measures an individual's willingness to take risks and their beliefs in their ability to influence events or outcomes; and (iv) general tolerance to risk taking in general and in financial decisions. All indicators were measured using a five-point scale in which 5 is the best and 1 the worst.²²

²⁰ Self-reported literacy also increased as a result of the program. However, these results are not presented because of an imbalance in this variable at baseline (appendix B).

²¹ The test consists of showing a card with three digits and, 10 seconds later, asking the respondent to repeat the number. This is repeated with sequentially higher numbers of digits until the respondent can no longer correctly recall.

²² To measure noncognitive skills, participants were asked three questions per category (impulse control, locus of control, and polychronicity). Following Roseth, Valerio, and Gutiérrez (2016), a simple average per category was estimated after scoring each question on a five-point scale where 5 was the most desirable and 1 the least desirable answer.

The program expanded impulse control among participating youth by 2.8 percent from 3.5 in the control group (appendix A, table A4, column 2), which is of interest given that recent evidence suggests higher impulse control is associated with a higher likelihood that a firm will exhibit growth potential (de Mel, McKenzie, and Woodruff 2010). The program also increased the ability of participants to multitask from 2.1 points to 2.2 points, equivalent to an increase of 5.7 percent, a positive result given recent evidence that entrepreneurs have the ability to juggle many tasks simultaneously (Lazear 2005). It likewise raised the internal locus of control (appendix A, table A4, column 3), another trait associated with entrepreneurship, by 2.9 percent from 3.2.²³ The program has no effect on measures of overall risk taking (appendix A, table A4, column 5).

The program effects on noncognitive skills showed variation in magnitudes and significance between women and men, but the equality of effects cannot be rejected for any of the measures used. Similarly, the analysis by treatment group suggested that the equality of the positive impacts observed on impulse control, multitasking, and the internal locus of control cannot be rejected across groups. This finding suggests that, in generalized low-skill environments, simple participation in an intensive skills intervention can induce important positive changes in noncognitive ability among participants. Unlike the literature, there is no evidence suggesting that on-the-job training had an additional effect on the development of noncognitive skills (the equality of impacts across groups cannot be rejected). However, the Ebola crisis caused substantial disruptions in the implementation of the on-the-job training element, and this should therefore be reexamined in future interventions.

Business practices

Another skill area relevant to entrepreneurship is good business practices, which include appropriate recordkeeping, marketing initiatives, inventory monitoring, setting financial targets or goals, and quality control systems (McKenzie and Woodruff 2015). However, while most of these practices are standard in medium and large enterprises, their implementation is less widespread among small and microenterprises, particularly in developing countries. Given that few household enterprises are likely to apply most of these practices, only the most basic of these, recordkeeping, was measured during the evaluation.

The program had a positive, significant impact on the share of youth who kept financial records, which rose by 4.0 percentage points, up from 9.7 in the control group (appendix A, table A5). This result is found only among men, but the equality of effects by gender cannot be rejected. The share of participants keeping formal records separately for business activities and the household increased by 3.0 percentage points, from 8.3 in the control group. The share of participants keeping financial records increased in all three groups. Changes in keeping separate financial records for the business and for the household are found only among group B participants, which indicates that the business skills training was effective in enhancing business practices. However, the equality of impacts across groups could not be rejected.

Capital

The program did not expand access to loans or boost business spending, but it did raise the amounts borrowed to start businesses among participants who own businesses (appendix A, table A5, panel I). This finding suggests the program was partially successful in relieving start-up capital constraints. This is an important result because evidence suggests that most informal activities face substantial financial barriers

²³ However, there is only limited evidence on how these abilities differ across larger-scale entrepreneurs and the self-employed who do not rely on hired workers (de Mel, McKenzie, and Woodruff 2010).

and, at low levels of capital, marginal returns to capital are high and could exceed 70 percent a month (Grimm, Knorringa, and Lay 2012). However, because this result relies on conditional estimates, it should be interpreted with caution; similar effects are not found in the unconditional estimates, possibly because of the large proportion of households not engaged in household enterprises.²⁴

The increase in the amount borrowed could be linked to the shift in the distribution across the types of businesses run by participants. The rise in start-up loan amounts among participants with businesses is consistent with the large surge observed in the amount these participants reported needing to start their current businesses. There are several plausible explanations for the increase in the amount of start-up capital required. One hypothesis is that the program induced a change in the information youth had at their disposal; for example, if they had gained more knowledge about the business environment or if the training exposed them to new business ideas, this could change the optimal investment level. The latter is consistent with the shift in types of businesses as a result of the program (see above).

An alternative explanation is that the skills or capital acquired through training affected youth expectations about how much capital they could access. If the types of businesses they conceived were constrained by the capital they expected to be able to access and the skills they possessed, then relaxing these constraints through the program could have allowed them to consider more ambitious investments. This hypothesis is supported by the fact that the program exerted an impact both on the amounts youth report needing to start businesses and on skills. This result could have also occurred if the program improved business plans and, thus, the participants' expectations of their ability to borrow.

The program drove up the capital stock of businesses, which could lead to higher future productivity and growth (appendix A, table A5, column 3).²⁵ Meanwhile, monthly business expenses did not increase. This indicates that, instead of incurring additional recurrent expenses, youth chose to invest at least a portion of the cash injection in capital stock (column 3).²⁶ This investment choice is consistent with theoretical models of financial market failures that restrict both savings and access to credit, constraints that are prevalent among these youth and more broadly in the Sierra Leonean context (Blattman et al. 2014; Fafchamps et al. 2014; Margolis et al. 2016). It also supports the above hypothesis that the program led some young participants to pursue higher-productivity businesses.

The program was designed to reduce capital constraints among entrepreneurs through microfinance facilitation. However, the Ebola crisis disrupted the microfinance facilitation aspects of the program; the capital infusion provided directly by the program thus consisted primarily of the monthly stipend tied to attendance. The effects on the optimal borrowing and capital stock therefore likely reflected the liquidity effect of the stipend, combined with any indirect effect through training. There was an increase in the amount of capital needed to start a business among groups A and C, but not group B. This may suggest that participants receiving the technical skills training may be willing to use their new skills to start businesses

²⁴ Conditional estimates were used given that only participants with businesses were asked to report their borrowing for business purposes. Based on balance tests, the conditional results show no evidence of differential selection bias: the restricted sample is balanced on observables across treatment groups.

²⁵ A long-standing premise of economic theory holds that capital accumulation and technological change account for much of the change in productivity and growth. The Solow-Swan model explains exogenous economic growth through capital accumulation, labor, and productivity (Solow 1956; Swan 1956). Romer (1990) later extended the model to endogenous economic growth theory.

²⁶ Capital stock is measured as the estimated value at sale of the current stock of tools, equipment, and machines used in the business.

and were now better able to recognize the number and cost of the tools and inputs they needed. However, group comparisons show that group C was the only group experiencing a significantly larger impact in any of the capital outcomes relative to the other treatment groups.

c. Relative effectiveness of various intervention elements

Treatment group comparison

Many studies of training programs face difficulty in determining which training elements are most effective in promoting employment and entrepreneurship because various types of content are mixed together in most experiments (McKenzie and Woodruff 2014). This study aims to inform this discussion by examining the relative effectiveness of providing youth with the following: (i) occupation-specific skills through technical training, plus on-the-job training (treatment A); (ii) business skills training and facilitation of access to finance (treatment B); and (iii) a package combining the two (treatment C). However, given that Ebola effectively nullified the capital intervention among groups B and C by rendering the microfinance element infeasible, the comparison involves a technical skills package with on-the-job-training, a business skills package, and a combination of both.

The expectation based on the program design was that larger effects would be found in treatment group C, which combined all aspects of the intervention, but this is not borne out by the data. There is no evidence that the various training elements are additive: the econometric results undermine the hypothesis that the impacts of receiving treatment C are either greater than or equal to the combined impacts of receiving treatments A and B; nor are the vast majority of the impacts statistically distinguishable across the groups (appendix A, table A7). There is thus insufficient evidence to conclude that any one package is more effective at increasing the employability of youth and promoting youth-owned household enterprises.

The absence of statistically significant differences across the groups may partly reflect the evaluation timeline and the consequences of the Ebola crisis. The endline survey was collected less than a year after the end of the intervention, thus covering a short span postintervention. As a result, the market may not have yet recognized the newly acquired skills and abilities, limiting comparisons across the groups. This drawback was intensified by the fact that program participants graduated from the training to join a labor market severely affected by the Ebola crisis. The ability of the participants to secure employment was undoubtedly affected by the generally poor economic climate, as evidenced by the steep decline in the urban employment rate from 75 percent to 67 percent, an 8 percentage point increase in the number of household enterprises out of operation (a measure of business survival), and a contraction in business revenues by 40 percent (Fu et al. 2015). However, the lack of differences also provides suggestive evidence that foundational skills, particularly basic literacy and numeracy, and participation in an intensive training intervention alone have the potential to enhance skills among youth in generalized low-skill contexts.

Cash versus skills

There is some debate as to whether skills or capital are more important at inducing changes. The study design did not allow for a robust comparison between these two elements, making it difficult to disentangle the respective effects. In addition, although the program was designed to reduce capital constraints to entrepreneurship through microfinance facilitation, the Ebola crisis disrupted this aspect. The program's influence in relieving capital constraints was thus reduced to a cash infusion in the form of the stipend provided to all groups. Nonetheless, the data allow some reasonable speculation that neither skills nor cash alone (at least in the amount provided) would have been sufficient to achieve the range of impacts found.

The data support the argument that skills alone could not have achieved the quality and magnitude of the impacts found.²⁷ First, the amount of the stipend was nonnegligible. Although the daily stipend was roughly equivalent to the typical cost of food and transport, the average monthly stipend received by participants also corresponded to 49.1 percent and 28.4 percent of the median earnings at baseline among the participants receiving technical skills and business skills training, respectively, assuming perfect attendance.²⁸ Second, excluding the stipend, increases in capital stock could have been derived only from borrowing or from the reinvestment of profits, that is, the combined increase in these two should be commensurate with the increase in the capital stock.²⁹ However, the increase in profits and in the amounts borrowed did not fully account for the large increase in capital stock and consumption, suggesting that the stipend played a role. The total combined expansion in capital, expenditures, and consumption during the 12 months prior to the survey represented almost six times the sum of the amount borrowed and the total profits generated during the same period.

Nonetheless, the stipend was not likely sufficient to spur the changes observed in skills and employment. First, the program raised the likelihood of employment, a result that is unlikely to have been found in the presence of a cash treatment only, given the relatively small size of the stipend. This probably arose because of the access to new job networks or the reduced information asymmetries in companies in which the participants were apprenticed (the latter only for groups A and C). A piece of suggestive evidence is that the quality of the training centers matters for the impacts, which would not have been expected if only the liquidity effect mattered because all participants were eligible to receive the same amount of stipend. Second, the stipend cannot fully account for the rise in capital observed because the stipend only represents half (48 percent) of the required start-up capital at baseline and 72 percent of the increase in capital stock. Finally, the returns to skills in terms of consumption and earnings are high for these youth. This suggests that, although the employment gains were greater among those participants with higher initial skill endowments, the change in skills induced by the program could potentially have led to medium- and long-term effects in future earnings and consumption.³⁰

d. Heterogeneity of effects by initial noncognitive skills

Recent studies have tried to identify which individuals and businesses benefit the most from skills development programs, but few studies have had enough power to answer this question (McKenzie and Woodruff 2014). Evidence with enough statistical power has focused on how the cognitive ability of the owner affects outcomes and suggests that impacts tend to be stronger among less well educated

²⁷ One way to disentangle the effect of training versus cash on profits is to reestimate the impact after controlling directly for capital stock. However, the underlying assumption is that the production function mapping capital stock to profits is unchanged, which is unlikely to hold in the presence of a large aggregate shock (such as the Ebola crisis) between survey rounds.

²⁸ The stipend represented 26 percent and 15 percent of monthly per capita household consumption, respectively.

²⁹ It may also be derived from savings, held primarily in *osusus* (45 percent of youth), which are informal savings groups similar to *tontines* or rotating savings and credit associations elsewhere, whereby, for example, 10 people put Le 5,000 in a pot; then one of the 10 takes the resulting Le 50,000 for his or her own use, promising to put in Le 5,000 at the next group meeting to continue the process. Data on amounts saved were not collected, but there is no reason savings would be positively impacted through channels other than profits or the stipend, especially during the Ebola crisis.

³⁰ Regression analysis shows that cognitive and noncognitive ability (impulse control, locus of control, and multitasking) are strong predictors of consumption and earnings among paid workers, even if all standard controls are included. A 1 point increase in noncognitive scores is associated with 40 percent to 80 percent rises in consumption and earnings.

participants (de Mel, McKenzie, and Woodruff 2014; Sonobe, Suzuki, and Otsuka 2011). However, concerns have been raised about the ability of low-skill participants to implement what they have been taught (McKenzie and Woodruff 2014). Given that noncognitive skills are known to be strong determinants of entrepreneurial ability and that the program had impacts on these outcomes, an analysis of the heterogeneity of effects by this dimension is important.

In terms of skills formation, the program had a greater impact on both cognitive and noncognitive skills among those participants with lower initial skills by nearly all measures of noncognitive skills (appendix A, table A6, columns 6–9). However, these skill gains translated into labor market impacts differently depending on the initial noncognitive ability, possibly because of differences in the ability of beneficiaries to mitigate shocks. Among those participants with higher noncognitive skills (that is, impulse control) at baseline, the program had larger effects on employment, earnings, and the accumulation of capital stock (appendix A, table A6, columns 1–5). The direction of the interaction is similar to the interaction of employment and self-employment, although the coefficient is not significantly different from zero. Meanwhile, those participants with lower initial ability experienced larger impacts on consumption.

At first glance, this result appears to confirm the second hypothesis above that more highly skilled individuals were better able to utilize the skills acquired through the program for productive ends in the short term. However, because of the Ebola crisis, the story is likely to be more complex. Although, because baseline data on wealth were not collected, the study has not been able to test whether initially poorer individuals achieved larger impacts, statistical analyses suggest that noncognitive ability is strongly associated with wealth: on average, youth in households with a lower proxy means test score have lower noncognitive ability. A 1 point reduction in the proxy means test score is associated with a 4.1 percent, 6.7 percent, and 9.1 percent decrease in impulse control, locus of control, and multitasking scores, respectively. The differential impact by initial skill may therefore reflect differences in wealth. Individuals with less ability—who are also the poorest—may have been less able to withstand the Ebola shock in the short term and, as a result, utilize the modest capital acquired through the program to smooth their consumption rather than making productive investments.

There is also suggestive evidence that, in the longer term, those participants with initially low skills may have benefited more once they had an opportunity to apply their newly acquired skills. The data confirm that there was a strong positive link between noncognitive skills among urban Sierra Leonean youth and their consumption, earnings, and average profits. Together with the finding that the program had a larger effect in terms of noncognitive skills among those participants who had less initial ability, this may indicate that there is a time lag between when skills are acquired and returns materialize among those who start out with low ability. Additional follow-up surveys are needed to confirm this is the case, which would support previous evidence showing that targeting productive interventions on the poorest may increase the returns (Macours, Premand, and Vakis 2013).

6 Cost-effectiveness

Analysis of cost-effectiveness indicates that the skills intervention compared favorably with other interventions aimed at increasing employability in countries at similar income, despite the relatively low amount of capital provided.³¹ The program boosted overall employment by 3.1 percentage points, which is substantial given that the employment rate at endline among the control group was unusually low (only 12.6 percent) because of the Ebola crisis. The cost per job created was slightly less than US\$17,000, while similar job creation programs in developing countries have generated new jobs at a cost ranging from US\$8,500 to US\$80,000, and many others have failed to find any statistically significant impacts (McKenzie 2015).³² The estimate was similar for the cost of creating one new household enterprise, which was slightly more than US\$19,000.

Comparisons across other measures, including capital, profits, skills, and consumption, are more difficult to find among similar programs. Less than a year after it had ended, the program had generated US\$4.90 in profits and US\$2.20 in capital stock for each US\$100.00 of stipend; no comparator estimates were found, making it difficult to assess the cost-effectiveness of this outcome. Additionally, the program generated US\$3.90 in monthly consumption per capita for each US\$100.00 spent on total program costs, including stipends and program administration (US\$1.00 for each US\$25.30 spent). Thus, it compares favorably with other recently evaluated livelihood programs in Sierra Leone that generated US\$1.00 of yearly household food consumption for every US\$55.60 spent.³³ Assuming the impacts are maintained, the cost of the program would be recovered after 25 months. Similar programs in developing countries would require more than 36 months to recoup the training costs (Adoho et al. 2014; Hirshleifer et al. 2016).³⁴

The cost-effectiveness estimates presented above should be considered lower-bound estimates for two reasons. First, the program had a wide range of positive impacts, including on employment, entrepreneurship and capital, cognitive and noncognitive skills, consumption, and investments in household assets and living conditions. However, estimating the cost-effectiveness of interventions designed to impact multiple outcomes is a challenge because analyses estimate the cost of achieving a unit increase in a single outcome at a time (Dhaliwal et al. 2013). Second, cost-effectiveness estimates do not take into account potential spillovers within households, such as shifts in labor allocation because of the program. For instance, additional jobs may have been created by crowding in the labor supply of other household members because of liquidity effects and by expanding the ability of households to take risks; there is evidence of such spillovers in related youth employment programs in Sierra Leone (Rosas and Sabarwal 2016). The evaluation was not able to capture these aspects because of budget constraints; this may be an area for future research measuring the impacts on jobs and cost-effectiveness in these types of interventions. Finally, the shorter duration of treatment B translated into a lower cost per participant in this group (about US\$400) than in treatments A and C (US\$600), which implies greater cost-effectiveness in the former across most outcomes because impacts did not differ significantly across groups.

³¹ The total average stipend was Le 310,000 (equivalent to U\$73), compared with grants of U\$120 in Ghana (Fafchamps et al. 2014), U\$382 in Uganda (Blattman, Fiala, and Martinez 2014), and U\$130 in Sri Lanka (de Mel, McKenzie, and Woodruff 2014).

³² The total project cost was Le 5.7 billion (equivalent to about US\$1.3 million), and regression results indicate the intervention in Sierra Leone created 75 jobs in the short term among the 2,400 project participants. McKenzie (2015) studies seven programs with objectives similar to HELP-SL's intervention; four of them failed to create any jobs.

³³ Estimations based on the impacts and costs reported by Rosas and Sabarwal (2016).

³⁴ Unlike this paper, the papers cited calculate cost-effectiveness in terms of monthly household income.

7 Conclusions

Overall, the findings of this evaluation add to the growing body of evidence indicating that combining skills training and cash injections, even small ones, can be effective in stimulating employment and entrepreneurship. The program increased the overall levels of employment by 3.1 percentage points, the worst profits by 25 percent, and the likelihood of first-time entrepreneurship by 4.1 percentage points. Although the prevailing circumstances because of the Ebola crisis reshaped the evaluation design, making it impossible for the evaluation to cleanly disentangle the training elements from the effect of a larger capital injection, there is suggestive evidence that both cash (through the stipend) and skills played a role in the impacts. With a few exceptions, most impacts were statistically similar across treatment groups and across genders.

The results also suggest that shaping noncognitive skills shows promise in creating jobs and in the human capital accumulation of youth in fragile states, but more evidence is needed. Improvements in the skills of higher-ability individuals were associated with positive labor market impacts, while lower-ability individuals, who tended to be poorer, experienced larger gains in skills formation, but consumed more rather than making productive investments. This emphasizes that these programs are not substitutes for adequate safety nets to protect the poorest from shocks to prevent the depletion of productive resources. Combined with evidence of high returns to noncognitive skills among Sierra Leonean youth, this also suggests that the program could eventually lead to improved labor market outcomes among the poorest. However, more evidence of such interventions in low-income contexts is needed to test, particularly, the following: (i) whether the impacts on employment and entrepreneurship are sustained in the long term and materialize among poor, low-ability individuals and (ii) the extent to which less skilled individuals are able to benefit from this type of intervention in the absence of an aggregate shock such as Ebola.

Lower-ability youth tend to come from poorer households, and targeting these interventions on low-ability youth may therefore be poverty reducing in these contexts. This points to the potential of using measures of ability, such as simple noncognitive tests, for targeting in employability programs among urban youth in fragile contexts. This may be an area for future testing and evaluation. However, the results also indicate that the impacts on employment may require time to materialize among the poor and the less well skilled. Interventions designed to seek quick wins in boosting employment may thus need to target more highly skilled individuals. Meanwhile, interventions aimed at the poor with lower skills may benefit from providing a basic transfer, in addition to capital intended for productive purposes to prevent consumption needs from limiting the productive impacts of a program. A concurrent effort may also be required to attract the poorest, who might otherwise self-select out of programs (Macours, Premand, and Vakis 2013).

The fact that similar expansions in cognitive ability were found across all groups suggests that providing foundational skills, such as basic literacy and numeracy, may be a sort of second-chance parenting opportunity, helping to fill gaps in early parental investments, missed schooling, or low-quality education systems (Filmer and Fox 2014).³⁵ Combined with the results on the heterogeneity of effects on skills, this is also indicative that, in societies with a generalized low level of skill formation through adolescence and adulthood such as Sierra Leone, a unique phenomenon may emerge: the sensitive period for cognitive skill development that are ripe for investment may effectively cover a longer period, including early adulthood. The period during which dynamic complementarity may offset static complementarity may thus extend

³⁵ The term parenting is used in the general sense of Heckman and Corbin (2016), that is, it encompasses care by extended family, schooling, and other environmental influences.

beyond the early years (Heckman and Corbin 2016). This has important policy implications because the long-run productivity trade-offs between investing in less highly skilled versus more highly skilled youth may be steep. However, more evidence of the long-term effects of such interventions in low-income contexts is needed, including on heterogeneity. The impacts observed on noncognitive skills also agree with the findings in the literature that point to both the age-malleability of these types of abilities and the synergies across various components of the skills (Heckman and Corbin 2016).

Positive impacts were observed among men and women despite the fact that the design of the program was fairly gender neutral and was characterized by no explicit gender features beyond the 40 percent quota for women's participation. There is suggestive evidence of differences by gender, but, in most cases, the equality of effects by gender cannot be rejected. For instance, there were positive impacts on earnings among women (conditional estimates), an effect that may derive from greater productivity per hour given that improvements in cognitive and noncognitive skills were observed even though the number of hours worked did not change. The likelihood that men would be self-employed was increased, and this effect did not arise at the expense of wage work. Beyond reducing key capital and skills constraints to enterprise development that tend to affect women disproportionately, the program also induced positive changes in young men's perceptions related to occupational segregation. This is promising for fragile contexts, where the trade-offs between complex designs with highly gender-specific parameters and operational feasibility tend to be substantial. However, the context should be taken into account in the extent to which interventions are tailored to gender. In the case of Sierra Leone, strong impacts on women may have been more forthcoming despite the gender-neutral design because of women's generally greater labor force participation and concentration in self-employment. At the same time, marginal improvements might enhance the impacts among women by, for example: (i) encouraging low-ability women who may self-select out at the application stage, (ii) supporting entry into trades dominated by men, and (iii) providing childcare arrangements during training, the lack of which appeared anecdotally to disrupt training activities.

Finally, these results are not independent of design features and implementation factors, which likely influenced the interactions of beneficiaries with the program. First, anecdotal evidence indicates that the stipend played a substantial role in attracting and retaining participants, although the short business proposal may have also acted as a signal of commitment to the program. Second, there is a need to match youth's perceptions of marketable skills with market realities, which requires ex ante market analysis. Anecdotal evidence also suggests that youth training choices are influenced by the size of the cash grants the program provides through the stipend. These choices are important because present bias may lead young beneficiaries, especially those with limited alternative income sources, to compromise long-run gains to relieve short-term, but more salient constraints. Also, the results suggest that the quality of training centers matters, indicating that prior screening and capacity building may enhance impacts. More research on how these program attributes affect impacts is encouraged. Despite implementation challenges, the program was cost-effective overall in achieving a range of positive benefits among the targeted households, including building household resilience to a large aggregate shock, the Ebola crisis. However, more effort is needed to enhance cost-effectiveness, possibly by focusing on the basics.

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Appendix A: Figures and Tables

Figure A1. Participant Selection and Randomization Process

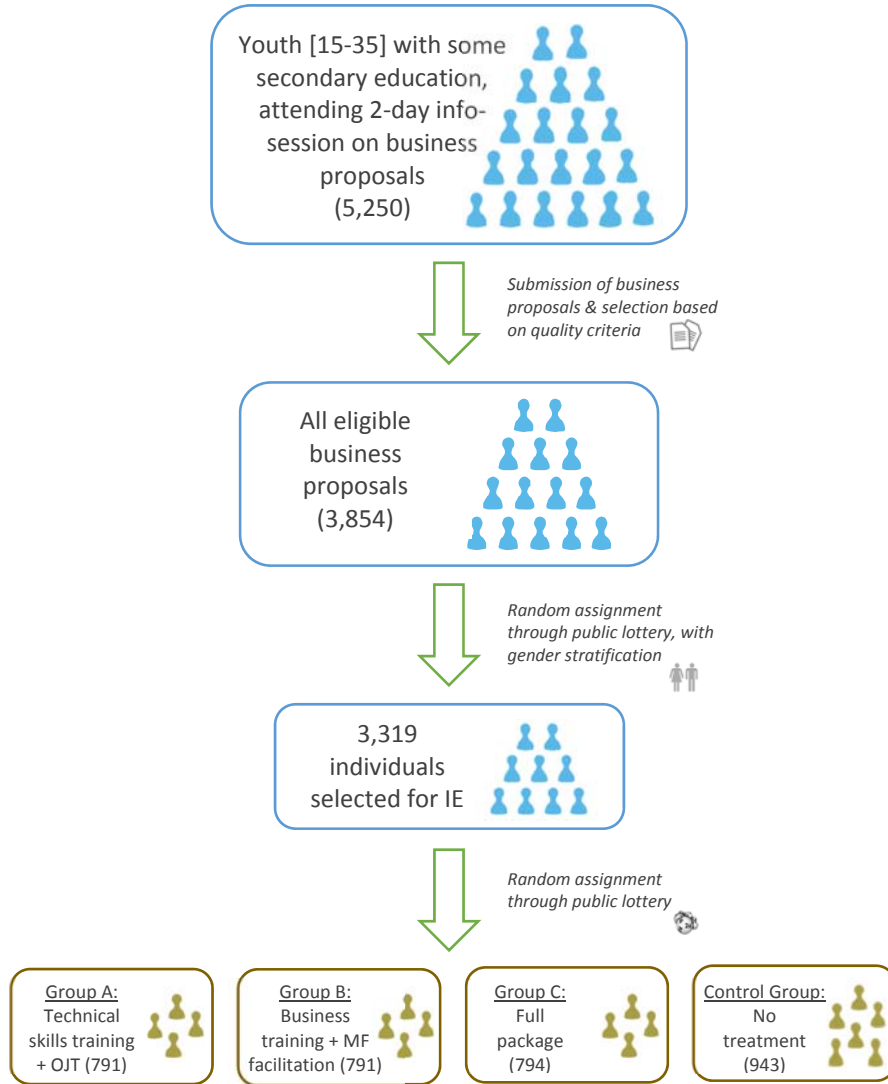


Figure A2. Program and Impact Evaluation Implementation Timeline

	2013		2014										2015		
	Aug-Nov	Dec	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct-Dec	Jun		
Group A	Baseline survey	Technical skills training						On-the-job training						Endline survey	
Group B								Business skills training							Microfinance facilitation
Group C		Technical skills training						On-the-job training							
								Business skills training							Microfinance facilitation
Control		No treatment													

Figure A3. Program Impact Flow

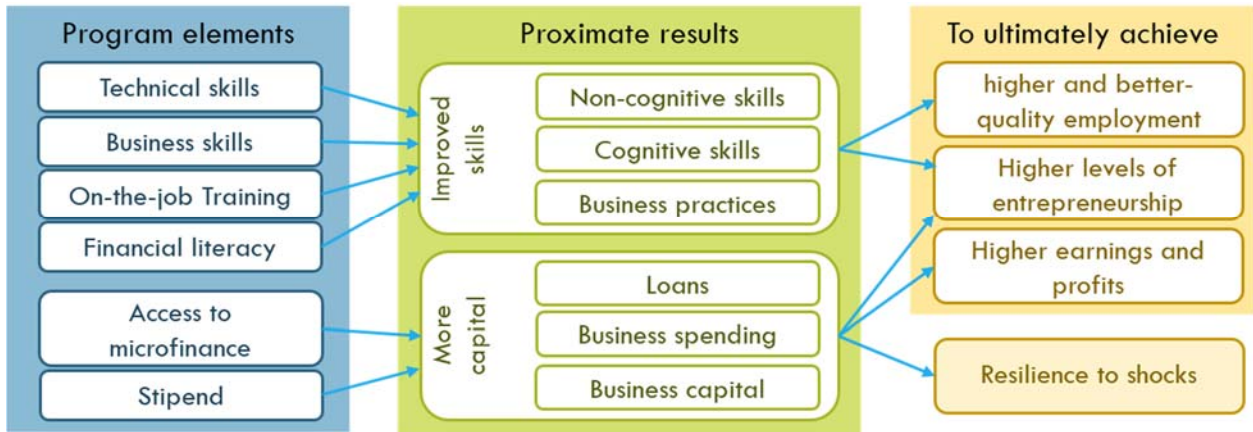


Table A1. Impact on Employment Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	<i>Overall sample</i>					<i>Conditional on employment</i>	
	Employed	Wage work	Self-employment	Unemployed	Log monthly earnings - all	Log monthly earnings	Log hours worked per day
Panel I. Impact of the overall treatment							
Treatment ITT	0.0311** (0.0144)	0.0138 (0.0117)	0.0131 (0.00922)	0.0117 (0.0210)	0.00550 (0.232)	0.321** (0.145)	0.0118 (0.0639)
Control mean	0.126	0.081	0.045	0.660	97344.860	341,684	7.79
N	2,565	2,584	2,584	2,584	2,700	822	363
Treatment ITT on women	0.0181 (0.0193)	0.0124 (0.0125)	0.000230 (0.0159)	0.0256 (0.0293)	-0.229 (0.304)	0.639** (0.278)	0.131 (0.108)
Control mean	0.118	0.046	0.072	0.692	69259.008	302,758	7.23
N	1,277	1,289	1,289	1,289	1,329	295	154
Treatment ITT on men	0.0439** (0.0215)	0.0179 (0.0200)	0.0233** (0.00912)	-0.00880 (0.0300)	0.257 (0.352)	0.113 (0.167)	-0.0716 (0.0775)
Control mean	0.134	0.117	0.016	0.628	125648.432	366,985	8.33
N	1,288	1,295	1,295	1,295	1,371	527	209
p-value effects on men vs. women	0.366	0.811	0.204	0.408	0.292	0.091	0.102
Panel II. Impact of each treatment group							
A. Technical skills package	0.0283 (0.0183)	0.00446 (0.0147)	0.0197 (0.0121)	0.0478* (0.0261)	-0.187 (0.285)	0.405** (0.172)	0.0644 (0.0782)
B. Business skills package	0.0322* (0.0189)	0.0138 (0.0154)	0.0139 (0.0121)	0.0144 (0.0266)	0.0855 (0.296)	0.261 (0.180)	0.0501 (0.0736)
C. Full package	0.0328* (0.0187)	0.0231 (0.0154)	0.00576 (0.0116)	-0.0272 (0.0267)	0.123 (0.293)	0.304* (0.176)	-0.0681 (0.0811)
Control mean	0.126	0.081	0.045	0.660	97344.860	341,684	7.79
N	2,565	2,584	2,584	2,584	2,700	822	363
p-value A=B=C	0.970	0.532	0.555	0.026	0.530	0.678	0.171
p-value A=B	0.846	0.571	0.663	0.227	0.370	0.401	0.850
p-value A=C	0.822	0.262	0.282	0.007	0.304	0.542	0.086
p-value B=C	0.977	0.589	0.527	0.138	0.904	0.812	0.122

Note:

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

All variables have a seven-day recall period, except for monthly earnings variables, which have a 12-month recall period.

Variables conditional on employment consider those working. Regression 6 limits the analysis to those with non-zero earnings.

All specifications control for labor market determinants at the baseline, including educational attainment, marital status, and age, and encompass center fixed effects. If available, baseline outcome values are included. Row 1 of panel 1 and all rows of panel II also control for the gender of the participant.

Table A2. Impact on Entrepreneurship Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Overall sample</i>				<i>Conditional on entrepreneurship</i>	
	First time entrepreneur	Log of last month's profit	Log of profit in best month	Log of profit in worst month	Business type: trader, shopkeeper	Business has a temporary location
Panel I. Impact of the overall treatment						
Treatment ITT	0.0410** (0.0168)	0.113 (0.139)	0.243 (0.149)	0.247* (0.129)	0.0786** (0.0352)	0.0288 (0.0259)
Control mean	0.158	15,914	29,869	5,591	0.754	0.084
N	2,702	2,702	2,702	2,702	754	754
Treatment ITT on women	0.0212 (0.0268)	-0.126 (0.227)	0.146 (0.233)	0.203 (0.205)	0.111*** (0.0410)	0.0274 (0.0332)
Control mean	0.262	22,538	31,252	6,697	0.785	0.083
N	1,331	1,331	1,331	1,331	495	495
Treatment ITT on men	0.0513** (0.0207)	0.284* (0.165)	0.285 (0.186)	0.255 (0.158)	-0.0511 (0.0697)	0.0509 (0.0447)
Control mean	0.121	9,238	28,475	4,477	0.678	0.085
N	1,371	1,371	1,371	1,371	259	259
p-value effects on men vs. women	0.369	0.140	0.638	0.840	0.037	0.661
Panel II. Impact of each treatment group						
A. Technical skills package	0.0269 (0.0211)	0.0848 (0.176)	0.305 (0.194)	0.279* (0.167)	0.0395 (0.0446)	0.0483 (0.0339)
B. Business skills package	0.0696*** (0.0221)	0.257 (0.185)	0.439** (0.201)	0.486*** (0.177)	0.102** (0.0427)	0.0253 (0.0314)
C. Full package	0.0276 (0.0212)	0.00221 (0.171)	-0.00949 (0.182)	-0.0182 (0.155)	0.0911** (0.0440)	0.0138 (0.0310)
Control mean	0.158	15,914	29,869	5,591	0.754	0.084
N	2,702	2,702	2,702	2,702	754	754
p-value A=B=C	0.118	0.411	0.081	0.019	0.351	0.587
p-value A=B	0.068	0.380	0.538	0.286	0.164	0.500
p-value A=C	0.975	0.656	0.123	0.088	0.270	0.304
p-value B=C	0.074	0.187	0.033	0.006	0.798	0.710

Note:

Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

First-time entrepreneur takes the value of 1 if the participants businesses represent the first the participants have had in their lives.

Variables conditional on entrepreneurship consider those who currently have a business or who had one in the past.

All specifications control for entrepreneurship determinants at the baseline, including marital status, educational attainment, and age, and encompass center fixed effects. Baseline outcome variables are also included in regressions 1 to 4 and 6. Row 1 of panel 1 and all rows of panel II also control for the gender of the participant.

Table A3. Impact on Consumption and Household Conditions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log consumption pc	Log food consumption pc	Log nonfood consumption pc	Log other household's expenses pc	Total no. of assets	Improved toilet	Concrete roof	Piped water
Panel I. Impact of the overall treatment								
Treatment ITT	0.525*** (0.0633)	0.464*** (0.0627)	0.516*** (0.0641)	0.471*** (0.0852)	0.279*** (0.0881)	0.0313* (0.0167)	0.0123 (0.00975)	0.0217 (0.0149)
Control mean	296,905	103,610	193,295	205,982	4.160	0.203	0.054	0.149
N	2,660	2,660	2,660	2,660	2,702	2,702	2,702	2,702
Treatment ITT on women	0.733*** (0.0985)	0.702*** (0.0964)	0.696*** (0.100)	0.663*** (0.119)	0.205 (0.125)	0.0276 (0.0229)	0.0146 (0.0128)	0.0412** (0.0198)
Control mean	287,849	103,906	183,943	158,496	4.049	0.185	0.046	0.126
N	1,323	1,323	1,323	1,323	1,331	1,331	1,331	1,331
Treatment ITT on men	0.284*** (0.0738)	0.192*** (0.0738)	0.306*** (0.0763)	0.250** (0.120)	0.336*** (0.125)	0.0354 (0.0244)	0.00929 (0.0147)	0.00186 (0.0222)
Control mean	306,153	103,309	202,844	254,468	4.271	0.222	0.062	0.173
N	1,337	1,337	1,337	1,337	1,371	1,371	1,371	1,371
p-value effects on men vs. women	0.000	0.000	0.002	0.014	0.452	0.813	0.783	0.181
Panel II. Impact of each treatment group								
A. Technical skills package	0.646*** (0.0791)	0.578*** (0.0761)	0.632*** (0.0804)	0.611*** (0.105)	0.292*** (0.110)	0.0440** (0.0214)	0.00238 (0.0120)	0.0375** (0.0191)
B. Business skills package	0.410*** (0.0797)	0.362*** (0.0800)	0.398*** (0.0808)	0.362*** (0.106)	0.349*** (0.112)	0.0260 (0.0213)	0.0192 (0.0130)	0.0217 (0.0192)
C. Full package	0.513*** (0.0767)	0.445*** (0.0749)	0.511*** (0.0783)	0.433*** (0.106)	0.197* (0.110)	0.0237 (0.0211)	0.0156 (0.0126)	0.00584 (0.0186)
Control mean	296,905	103,610	193,295	205,982	4.160	0.203	0.054	0.149
N	2,660	2,660	2,660	2,660	2,702	2,702	2,702	2,702
p-value A=B=C	0.015	0.020	0.019	0.059	0.418	0.625	0.411	0.297
p-value A=B	0.004	0.006	0.005	0.022	0.630	0.434	0.215	0.446
p-value A=C	0.095	0.073	0.137	0.100	0.406	0.373	0.319	0.119
p-value B=C	0.201	0.292	0.172	0.518	0.193	0.919	0.800	0.431

Note:

Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Food consumption and nonfood consumption have a one-month recall period. Nonfood items include public transport, schooling, communication (telephone), electricity, fuel or other sources of energy, and expenses in cigarettes and alcohol.

Household expenses include supplies, improvements, clothing, health care, rent for land and dwelling, money given to nonhousehold members, and weddings and funerals. This variable has a two-month recall period.

Household assets include sofa; electric, gas, or kerosene stove; radio; cassette recorder; freezer or refrigerator; generator; and electric or charcoal iron. The variable is measured as the total number of assets the individual reported.

Improved toilet is a binary variable that takes a value of 1 if the dwelling toilet corresponds to a flushing toilet in a piped sewer system or a septic tank; it is zero otherwise.

Concrete roof or walls is a binary variable that takes a value of 1 if the dwelling has a concrete roof or walls; it is zero otherwise.

Piped water is a binary variable that takes a value of 1 if the source of drinking water is piped water and zero otherwise (including public tap and protected dug well).

All specifications control for socioeconomic conditions at the baseline, including marital status, educational attainment, and age, and encompass center fixed effects. If available, baseline outcome values are included. Row 1 of panel 1 and all rows of panel II also control for the gender of the participant.

Table A4. Impact on Proximate Outcomes: Cognitive and Noncognitive Skills

	(1)	(2)	(3)	(4)	(5)
	Verbal memory 6 digits	Impulse control	Greater locus of control	Multitasking ability	Prepared to take risks
Panel I. Impact of the overall treatment					
Treatment ITT	0.0630*** (0.0203)	0.0882*** (0.0255)	0.0932*** (0.0335)	0.121*** (0.0311)	0.00958 (0.0703)
Control mean	0.651	3.53	3.24	2.13	2.35
N	2,440	2,699	2,699	2,699	2,692
Treatment ITT on women	0.0937*** (0.0316)	0.117*** (0.0363)	0.0784 (0.0482)	0.157*** (0.0463)	0.0983 (0.0983)
Control mean	0.554	3.52	3.22	2.20	2.25
N	1,167	1,328	1,328	1,328	1,326
Treatment ITT on men	0.0363 (0.0263)	0.0477 (0.0356)	0.0942** (0.0465)	0.0779* (0.0414)	-0.0726 (0.101)
Control mean	0.749	3.54	3.27	2.05	2.45
N	1,273	1,371	1,371	1,371	1,366
p-value effects on men vs. women	0.158	0.169	0.812	0.199	0.220
Panel II. Impact of each treatment group					
A. Technical skills package	0.0730*** (0.0251)	0.113*** (0.0322)	0.139*** (0.0415)	0.0825** (0.0396)	0.0787 (0.0876)
B. Business skills package	0.0319 (0.0254)	0.0663** (0.0329)	0.0833** (0.0415)	0.149*** (0.0400)	0.0220 (0.0901)
C. Full package	0.0828*** (0.0247)	0.0846*** (0.0314)	0.0562 (0.0419)	0.132*** (0.0391)	-0.0729 (0.0871)
Control mean	0.651	3.53	3.24	2.13	2.35
N	2,440	2,699	2,699	2,699	2,692
p-value A=B=C	0.110	0.397	0.144	0.269	0.243
p-value A=B	0.112	0.179	0.188	0.120	0.544
p-value A=C	0.701	0.398	0.054	0.241	0.096
p-value B=C	0.046	0.588	0.527	0.689	0.306

Note:

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Verbal memory is a binary variable that takes a value of 1 if the respondent answers a digit span test of 6 digits correctly and zero otherwise.

Impulse control, locus of control, multitasking ability, and preparedness to take risks are categorical variables taking values from 1 to 5. In regressions 2-4, a greater value represents a larger stock of the skill, and, in regression 5, greater values represent a greater willingness to take risks.

All specifications control for socioeconomic conditions at the baseline, including marital status, educational attainment, and age, and encompass center fixed effects. If available, baseline outcome values are included. Row 1 of panel 1 and all rows of panel II also control for the gender of the participant.

Table A5. Impact on Proximate Outcomes: Business Practices, Spending, and Capital

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	<i>Business practices, spending, and capital</i>						<i>Business capital - conditional on entrepreneurship</i>	
	Keeps separate financial records	Keeps financial records	Log of capital stock	Log of monthly business expenses	Took any business loan	Loan for business inputs and equipment	Log of amount needed to start business	Log of amount borrowed to start business
Panel I. Impact of the overall treatment								
Treatment ITT	0.0304** (0.0129)	0.0401*** (0.0139)	0.235* (0.137)	0.172 (0.253)	0.00588 (0.0106)	-0.00640 (0.0104)	0.732*** (0.217)	0.734** (0.333)
Control mean	0.083	0.097	58,730	127,926	0.055	0.057	828,633	515,294
N	2,702	2,702	2,702	2,702	2,702	2,702	378	174
Treatment ITT on women	0.0250 (0.0211)	0.0312 (0.0224)	0.200 (0.207)	0.0326 (0.358)	-0.00240 (0.0176)	-0.0293 (0.0184)	0.752*** (0.279)	0.554 (0.413)
Control mean	0.141	0.164	54,090	131,137	0.097	0.113	643,627	496,667
N	1,331	1,331	1,331	1,331	1,331	1,331	275	127
Treatment ITT on men	0.0325** (0.0154)	0.0447*** (0.0168)	0.298* (0.177)	0.252 (0.360)	0.0107 (0.0120)	0.0150 (0.0103)	-0.0860 (0.324)	-0.0959 (0.410)
Control mean	0.059	0.070	63,406	124,690	0.036	0.026	1,419,231	560,000
N	1,371	1,371	1,371	1,371	1,371	1,371	103	47
p-value effects on men vs. women	0.771	0.629	0.717	0.663	0.535	0.034	0.032	0.179
Panel II. Impact of each treatment group								
A. Technical skills package	0.0257 (0.0163)	0.0335* (0.0177)	0.132 (0.175)	0.270 (0.316)	-0.00185 (0.0130)	-0.00546 (0.0130)	0.517* (0.272)	0.588 (0.386)
B. Business skills package	0.0394** (0.0173)	0.0521*** (0.0185)	0.431** (0.190)	-0.0660 (0.317)	0.0147 (0.0139)	-0.00638 (0.0132)	0.436 (0.272)	0.415 (0.407)
C. Full package	0.0263 (0.0168)	0.0352* (0.0180)	0.149 (0.171)	0.305 (0.318)	0.00510 (0.0135)	-0.00736 (0.0129)	1.156*** (0.278)	1.095** (0.422)
Control mean	0.083	0.097	58,730	127,926	0.055	0.057	828,633	515,294
N	2,702	2,702	2,702	2,702	2,702	2,702	378	174
p-value A=B=C	0.714	0.599	0.273	0.464	0.509	0.990	0.027	0.253
p-value A=B	0.462	0.355	0.144	0.309	0.246	0.945	0.782	0.663
p-value A=C	0.975	0.930	0.930	0.917	0.615	0.887	0.031	0.202
p-value B=C	0.488	0.404	0.164	0.263	0.510	0.943	0.014	0.114

Note:

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Dependent variables in columns 1, 2, 5, and 6 are binary variables.

Variables conditional on entrepreneurship consider those who currently have a business or had one in the past and who needed and borrowed amounts above zero.

All unconditional specifications control for age, marital status, educational attainment, center fixed effects, and baseline outcome values, except for regressions 1, 2, and 4, which do not control for baseline values. Conditional specifications control for baseline outcome values. Row 1 of panel 1 and all rows of panel II also control for the gender of the participant.

Table A6. Heterogeneity with Respect to Baseline Noncognitive Skills

Treatment interaction with:	Control mean (SE) of interacting variable	(1) Employed	(2) Self-employment	(3) Log monthly earnings	(4) Log consumption per capita	(5) Log of capital stock	(6) Verbal memory 6 digits	(7) Impulse control	(8) Greater locus of control	(9) Multitasking ability
Panel I. Whole sample										
Impulse control	3.53 (0.019)	0.0490*** (0.0189)	0.0127 (0.0133)	0.502* (0.288)	-1.870*** (0.0750)	0.448** (0.177)	-0.115*** (0.0228)	-0.517*** (0.0321)	-0.469*** (0.0403)	-0.288*** (0.0383)
Locus of control	3.24 (0.026)	-0.00355 (0.0171)	-0.0170 (0.0107)	0.0370 (0.259)	-1.542*** (0.0802)	0.114 (0.165)	-0.0656*** (0.0213)	-0.440*** (0.0316)	-0.371*** (0.0382)	-0.256*** (0.0376)
Multitasking	2.13 (0.025)	-0.00320 (0.0176)	-0.0115 (0.0112)	0.212 (0.261)	-1.184*** (0.0816)	-0.0369 (0.153)	-0.00655 (0.0218)	-0.315*** (0.0319)	-0.300*** (0.0385)	-0.295*** (0.0352)
Panel II. Women										
Impulse control	3.52 (0.026)	0.0625** (0.0261)	0.0129 (0.0221)	0.631* (0.370)	-2.170*** (0.0996)	0.566** (0.244)	-0.105*** (0.0339)	-0.541*** (0.0454)	-0.548*** (0.0558)	-0.293*** (0.0533)
Locus of control	3.22 (0.037)	0.0208 (0.0215)	-0.0220 (0.0164)	0.0158 (0.334)	-1.730*** (0.114)	0.0672 (0.232)	-0.0782** (0.0312)	-0.455*** (0.0433)	-0.453*** (0.0518)	-0.250*** (0.0542)
Multitasking	2.20 (0.037)	0.0173 (0.0230)	-0.0190 (0.0175)	0.561* (0.338)	-1.265*** (0.127)	-0.125 (0.226)	-0.0141 (0.0322)	-0.370*** (0.0457)	-0.283*** (0.0563)	-0.241*** (0.0511)
Panel III. Men										
Impulse control	3.54 (0.028)	0.0218 (0.0281)	0.0118 (0.0129)	0.0524 (0.455)	-1.301*** (0.109)	0.384 (0.269)	-0.141*** (0.0314)	-0.450*** (0.0472)	-0.359*** (0.0589)	-0.230*** (0.0578)
Locus of control	3.27 (0.037)	-0.0358 (0.0276)	-0.00726 (0.0124)	-0.263 (0.409)	-1.121*** (0.107)	0.162 (0.231)	-0.0556* (0.0301)	-0.384*** (0.0476)	-0.252*** (0.0576)	-0.223*** (0.0529)
Multitasking	2.05 (0.034)	-0.0370 (0.0274)	-0.00290 (0.0130)	-0.352 (0.409)	-0.913*** (0.0956)	0.0621 (0.207)	-0.00179 (0.0299)	-0.198*** (0.0448)	-0.271*** (0.0538)	-0.319*** (0.0480)
Panel IV. p-value interaction effect on men vs. women										
Treatment*Impulse control		0.283	0.965	0.319	0.000	0.613	0.438	0.162	0.018	0.421
Treatment*Locus of control		0.102	0.471	0.594	0.000	0.769	0.598	0.264	0.009	0.723
Treatment*Multitasking		0.125	0.457	0.083	0.025	0.537	0.776	0.007	0.876	0.261

Note:

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Dependent variables are all unconditional.

Dependent variables in regressions 1 and 2 have a seven-day recall period; regressions 3 and 5 have a 12-month recall period; and regression 4, a one-month recall period.

Verbal memory is a binary variable that takes a value of 1 if a respondent answers with a digit span test of 6 digits correctly; it is zero otherwise.

Impulse control, locus of control, and multitasking ability are categorical variables taking values from 1 to 5. A greater value represents a larger stock of the skill.

All specifications control for age, marital status, educational attainment, and center fixed effects. Equations 1 to 5 also control for baseline outcome values if available.

Table A7. Treatment Group Impact Comparison

	Regression coefficient			t-test						
	A	B	C	A=B=C	A=B	A=C	B=C	A+B=C	A+B≥C	A+B≤C
Employment										
Employed	0.028	0.032	0.033	0.970	0.846	0.822	0.977	0.318	0.841	0.159
Wage work	0.004	0.014	0.023	0.532	0.571	0.262	0.589	0.832	0.584	0.416
Self-employment	0.020	0.014	0.006	0.555	0.663	0.282	0.527	0.116	0.942	0.058
Unemployed	0.048	0.014	-0.027	0.026	0.227	0.007	0.138	0.020	0.990	0.010
Log monthly earnings - all	-0.187	0.085	0.123	0.530	0.370	0.304	0.904	0.595	0.703	0.297
Log monthly earnings	0.405	0.261	0.304	0.678	0.401	0.542	0.812	0.144	0.928	0.072
Log hours worked per day	0.064	0.050	-0.068	0.171	0.850	0.086	0.122	0.083	0.959	0.041
Entrepreneurship										
First-time entrepreneur	0.027	0.070	0.028	0.118	0.068	0.975	0.074	0.030	0.985	0.015
Log of last month's profit	0.085	0.257	0.002	0.411	0.380	0.656	0.187	0.198	0.901	0.099
Log of profit in best month	0.305	0.439	-0.009	0.081	0.538	0.123	0.033	0.009	0.996	0.004
Log of profit in worst month	0.279	0.486	-0.018	0.019	0.286	0.088	0.006	0.002	0.999	0.001
Business type: trader, shopkeeper	0.040	0.102	0.091	0.351	0.164	0.270	0.798	0.419	0.791	0.209
Business has a temporary location	0.048	0.025	0.014	0.587	0.500	0.304	0.710	0.191	0.905	0.095
Consumption and household conditions										
Log consumption per capita	0.646	0.410	0.513	0.015	0.004	0.095	0.201	0.000	1.000	0.000
Log food consumption per capita	0.578	0.362	0.445	0.020	0.006	0.073	0.292	0.000	1.000	0.000
Log nonfood consumption per capita	0.632	0.398	0.511	0.019	0.005	0.137	0.172	0.000	1.000	0.000
Log other household expenses pc	0.611	0.362	0.433	0.059	0.022	0.100	0.518	0.000	1.000	0.000
Total no. of assets	0.292	0.349	0.197	0.418	0.630	0.406	0.193	0.006	0.997	0.003
Improved toilet	0.044	0.026	0.024	0.625	0.434	0.373	0.919	0.138	0.931	0.069
Concrete roof	0.002	0.019	0.016	0.411	0.215	0.319	0.800	0.749	0.626	0.374
Piped water	0.037	0.022	0.006	0.297	0.446	0.119	0.431	0.056	0.972	0.028
Cognitive and noncognitive skills										
Verbal memory 6 digits	0.073	0.032	0.083	0.110	0.112	0.701	0.046	0.537	0.731	0.269
Can read or write a single paragraph	0.037	0.023	0.020	0.359	0.266	0.185	0.831	0.030	0.985	0.015
Impulse control	0.113	0.066	0.085	0.397	0.179	0.398	0.588	0.044	0.978	0.022
Greater locus of control	0.139	0.083	0.056	0.144	0.188	0.054	0.527	0.005	0.997	0.003
Multitasking ability	0.083	0.149	0.132	0.269	0.120	0.241	0.689	0.087	0.956	0.044
Prepared to take risks	0.079	0.022	-0.073	0.243	0.544	0.096	0.306	0.175	0.913	0.087
Business practices, spending, and capital										
Keeps separate financial records	0.101	0.059	0.093	0.672	0.407	0.882	0.498	0.355	0.823	0.177
Keeps financial records	0.128	0.086	0.119	0.686	0.416	0.872	0.520	0.195	0.902	0.098
Log of capital stock	0.132	0.431	0.149	0.273	0.144	0.930	0.164	0.123	0.939	0.061
Log of monthly business expenses	0.270	-0.066	0.305	0.464	0.309	0.917	0.263	0.826	0.587	0.413
Took any business loan	-0.002	0.015	0.005	0.509	0.246	0.615	0.510	0.690	0.655	0.345
Loan for business inputs and equipment	-0.005	-0.006	-0.007	0.990	0.945	0.887	0.943	0.812	0.594	0.406
Log of amount needed to start business	0.517	0.436	1.156	0.027	0.782	0.031	0.014	0.609	0.696	0.304
Log of amount borrowed to start business	0.588	0.415	1.095	0.253	0.663	0.202	0.114	0.870	0.565	0.435

Appendix B: Baseline Balance Tests and Means

	Treatment A		Treatment B		Treatment C		Control		P-Values		
	N	Mean	N	Mean	N	Mean	N	Mean	T1: C	T2: C	T3: C
Freetown	791	65.36%	791	64.85%	794	65.37%	943	64.58%	0.74	0.91	0.73
Impact evaluation participant is head of HH	791	11.38%	790	12.91%	794	12.85%	942	12.85%	0.35	0.97	1.00
Head of HH is a woman	791	34.39%	791	35.02%	794	31.11%	942	33.97%	0.86	0.65	0.21
Number of people in the HH	791	4.70	791	4.77	794	4.81	942	4.79	0.31	0.79	0.87
Age	791	23.28	791	23.55	794	23.72	942	23.47	0.34	0.68	0.19
Male	790	50.13%	790	50.25%	793	50.44%	940	50.21%	0.97	0.99	0.92
Has attended school	787	98.22%	788	98.10%	792	98.36%	934	97.22%	0.17	0.23	0.11
Has been sick in the past four weeks	790	34.94%	789	31.18%	793	30.77%	931	32.33%	0.25	0.61	0.49
Can read or write a simple text	790	93.04%	790	94.94%	791	96.33%	939	92.55%	0.69	0.04	0.00
Digitspan recall test (correct answers)	791	6.36	791	6.38	794	6.47	930	6.46	0.15	0.29	0.91
Impulse control ^a	791	3.48	791	3.51	793	3.53	933	3.50	0.60	0.88	0.32
Locus of control ^a	791	3.29	791	3.25	792	3.28	933	3.31	0.51	0.07	0.36
Multitasker ^a	791	2.13	791	2.13	793	2.09	933	2.13	0.93	0.92	0.33
Risk attitudes ^a	791	2.39	791	2.48	792	2.62	933	2.58	0.02	0.23	0.62
Worked for pay in the past year	791	29.08%	791	31.48%	794	34.26%	933	29.80%	0.74	0.45	0.05
Paid employee	230	11.74%	248	12.90%	272	8.09%	277	12.27%	0.85	0.83	0.11
Never self-employed	790	78.61%	790	77.09%	792	73.48%	933	76.74%	0.35	0.86	0.12
Wholesale and retail trade	166	46.99%	177	48.02%	207	47.34%	210	44.29%	0.60	0.46	0.53
First business	166	86.75%	180	83.89%	209	87.08%	217	82.03%	0.21	0.63	0.15
Founder of the business	168	88.10%	176	84.66%	205	87.80%	210	90.00%	0.56	0.11	0.48
Years of operation	94	2.78	88	3.04	106	3.29	131	2.91	0.76	0.79	0.41
Profits made in last month	88	129,544	84	249,557	103	151,491	127	141,009	0.67	0.09	0.72
Profits made in best month	89	197,042	88	406,917	102	230,223	128	300,179	0.20	0.41	0.35
Profits made in worst month	88	129,544	84	249,557	103	151,491	127	141,009	0.67	0.09	0.72

Note: HH = household.

a. Measured on a scale from 1 (least desirable answer) to 5 (most desirable answer).

Appendix C: Detailed Attrition Analysis

At endline, Statistics Sierra Leone was able to track 2,782 of the 3,319 individuals who were surveyed at baseline. This suggests that the overall attrition rate was 16 percent (table C1). Because information on entrepreneurship, one of the main outcomes the program sought, is available for all 2,782 individuals, they are all included in the nonattritor sample. More than 80 percent of the individuals were thus successfully interviewed at both baseline and endline. To check for bias in attrition, the data are examined for compositional differences between attritors and nonattritors, and then robustness checks of the main results are conducted taking into account the predictors of attrition.

Table C1. Overall and Treatment Group Attrition Rate

	<i>A. Tech. skills + OJT</i>	<i>B. Business skills</i>	<i>C. Full Package</i>	<i>Control</i>	<i>TOTAL</i>
Baseline survey	791	791	794	943	3,319
Endline survey	669	663	673	777	2,782
Attrition	122	128	121	166	537
<i>Attrition rate</i>	<i>15%</i>	<i>16%</i>	<i>15%</i>	<i>18%</i>	<i>16%</i>

Table C2 presents the results of a set of t-tests of differences in means between the individuals who left the sample (attritors) and the individuals who stayed (nonattritors). The variables chosen for this analysis are those that may have had an impact on labor market outcomes: educational attainment, self-employment status, age, and marital status.

Table C2. P-Values of the Differences in Means, Attritors vs. Nonattritors

	<i>Treatment A</i>	<i>Treatment B</i>	<i>Treatment C</i>	<i>Control</i>	<i>Entire sample</i>
Age	0.03	0.19	0.84	0.37	0.04
Gender	0.31	0.27	0.17	0.69	0.63
Primary or less	0.96	0.31	0.33	0.63	0.62
JSS	0.22	0.49	0.19	0.60	0.50
SSS	0.15	0.53	0.31	<i>0.06</i>	0.11
Technical and vocational education and training, teaching, nursing	0.69	0.37	0.16	0.04	0.01
University	0.63	0.36	0.00	0.64	0.56
Postgraduate or Higher		0.32	0.54	0.90	0.67
Married (monogamous)	0.83	0.57	0.80	0.30	0.61
Married (polygamous)	0.16	0.32		0.32	0.05
Informal/loose union	0.03	0.32	<i>0.09</i>	0.31	0.15
Separated	0.00	0.77	0.05	0.21	0.66
Divorced	0.03	0.57	0.82	0.47	0.70
Widowed	<i>0.08</i>	0.03	0.03	0.01	0.00
Never married	<i>0.07</i>	0.33	0.68	0.83	0.94
Self-employed	0.03	0.64	0.84	0.45	0.13

Regarding marriage status, the only categories with a low p-value (indicating statistically significant differences between attritors and nonattritors) are those that have zeros in one of the categories (either attritors or nonattritors) and therefore constitute a small fraction of the sample: widows and polygamous

marriages. Those who stay are also older than those who leave, but the difference is less than a year (around 4.5 months).

Regarding education categories, there is a statistically significant difference in means between those who stayed in the sample and those who leave (attriters). In particular, the attriters are more well educated than those who stay (have degrees through technical and vocational education and training); however, this likely arises because of the small sizes in specific educational subgroups. First, the share of beneficiaries with technical and vocational education and training is small (less than 4.0 percent), as is the share of those with university education (1.3 percent). Second, because the labor outcomes are expected to be the largest among beneficiaries with the highest educational attainment, if those who are more well educated left the program and if any bias was introduced in the study's estimations, this bias would yield an underestimation of the effect of the program.

To ensure that the evaluation captured the program's impact rather than the impact of the characteristics that differ across the groups because of attrition, the balance tests were reestimated between the treatment and control groups without the attriters (table C3). The groups remain similar, on average. There were no differential patterns among the nonattriters in the treatment and control groups, and, among the key variables tested, the only variable with a difference in mean that was statistically significant also differed at baseline (can read or write a simple text).

Table C3. Baseline Balance after Attrition

<i>Indicator</i>	<i>Treatment A</i>		<i>Treatment B</i>		<i>Treatment C</i>		<i>Control</i>		<i>P-Values</i>		
	<i>N</i>	<i>Mean</i>	<i>N</i>	<i>Mean</i>	<i>N</i>	<i>Mean</i>	<i>N</i>	<i>Mean</i>	<i>T1: C</i>	<i>T2: C</i>	<i>T3: C</i>
Freetown	669	61.58%	663	62.14%	673	61.37%	777	61.52%	0.98	0.81	0.95
Impact evaluation participant is head of HH	669	11.81%	662	13.44%	673	12.63%	777	13.26%	0.41	0.92	0.72
Head of HH is a woman	669	34.23%	663	36.65%	673	31.35%	777	32.95%	0.61	0.14	0.52
Number of people in the HH	669	4.71	663	4.74	673	4.89	777	4.79	0.48	0.67	0.32
Age	669	23.40	663	23.62	673	23.71	777	23.52	0.57	0.64	0.37
Man	668	50.90%	662	49.40%	672	49.40%	776	50.52%	0.88	0.67	0.67
Has attended school	666	98.05%	660	98.03%	671	98.21%	772	97.15%	0.27	0.28	0.19
Has been sick in the past four weeks	668	35.63%	661	31.77%	672	31.25%	769	33.94%	0.50	0.38	0.28
Can read or write a simple text	669	92.83%	662	94.86%	670	96.27%	774	92.38%	0.75	0.06	0.00
Digitspan recall test (correct answers)	669	6.33	663	6.39	673	6.40	768	6.46	0.11	0.39	0.52
Impulse control ^a	669	3.48	663	3.53	672	3.52	771	3.50	0.55	0.41	0.58
Locus of control ^a	669	3.30	663	3.26	672	3.30	771	3.33	0.50	0.10	0.47
Multitasker ^a	669	2.13	663	2.12	672	2.11	771	2.13	0.99	0.76	0.62
Risk attitudes ^a	669	2.44	663	2.52	671	2.64	771	2.59	0.10	0.45	0.55
Worked for pay in the past year	669	30.34%	663	32.88%	673	34.92%	771	31.26%	0.71	0.51	0.14
Paid employee	203	9.85%	217	13.82%	235	8.09%	240	12.08%	0.46	0.58	0.15
Never self-employed	669	76.83%	662	75.83%	672	73.07%	771	74.97%	0.41	0.71	0.41
Wholesale and retail trade	152	47.37%	157	47.77%	179	46.37%	187	45.99%	0.80	0.74	0.94
First business	152	86.18%	160	83.75%	180	86.11%	193	81.35%	0.23	0.56	0.21
Founder of the business	154	88.96%	156	85.90%	179	88.83%	186	89.25%	0.93	0.35	0.90
Years of operation	87	2.77	79	3.10	93	3.05	119	2.88	0.82	0.67	0.71
Profits made in last month	81	126,542	75	239,037	90	161,394	115	138,678	0.67	0.15	0.48
Profits made in best month	82	196,057	78	406,842	89	239,445	116	277,722	0.32	0.37	0.63
Profits made in worst month	81	126,542	75	239,037	90	161,394	115	138,678	0.67	0.15	0.48

Note: HH = household.

a. Measured on a scale from 1 (least desirable answer) to 5 (most desirable answer).

Even though no worrying trends were observed in the data, these characteristics are controlled for in all the econometric estimations. Including these controls ensures that the effects of the program are estimated net of the impacts of age, educational attainment, or marriage status on the labor market. Similarly, to predict the correlates of survey attrition, an ordinary least squares equation of the determinants of participation in the surveys at both baseline and endline is estimated (table C4). Assignment to any treatment arm (A, B, or C) increases the probability of participation in the survey at both baseline and endline by 5 percentage points, and the estimate is significant at the 1 percent level. This is

Table C4. Attrition Prediction

<i>Variable</i>	<i>Nonattritor</i>	
	<i>Coeff.</i>	<i>Std. Err.</i>
Treatment	0.0519***	(0.0114)
Has children under 12	0.0247**	(0.0123)
Age	0.0226***	(0.00104)
Beneficiary is head	-0.0453***	(0.0172)
Mobile phone	0.188***	(0.0267)
Married (polygamous)	0.171***	(0.0491)
Informal/Loose union	0.0797**	(0.0360)
Separated	0.0108	(0.0653)
Divorced	0.0389	(0.0619)
Widowed	0.133***	(0.0256)
Never-married	0.182***	(0.0157)
Freetown	0.00719	(0.0122)
Kenema	-0.00759	(0.0205)
Koidu City	0.0511**	(0.0237)
Makeni	0.0740***	(0.0193)

not surprising given that 18 percent of the control group attrited (see table C1). The following variables were found to be positively correlated with staying in the sample: age, involvement in a polygamous marriage or informal union, never having been married, and access to a mobile phone.¹ Individuals in Koidu City and Makeni are more likely to stay in the sample, and this may be explained by the quality of the training centers in these cities. Besides being assigned to the control group, the other determinant of leaving the sample is status as the household head.

As a robustness check, an inverse probability weighting approach is used (Wooldridge 2000). This is accomplished by estimating a probit version of the equation to predict the probability of staying in the panel and calculating inverse probability weights, which are used to reweight the sample and reestimate the ordinary least squares models of the main dependent variables. Specifically, the probability that an individual stays in the sample is predicted through a probit function, and, later, the objective function is

weighted by 1/predicted probability. This method gives more weight to the individuals with the highest chance of attrition, giving them more influence on the estimate of the impact relative to individuals with a low probability of attrition.

The results, following Adoho et al. (2014), are presented in table C5, next to the main unadjusted ITT results for comparison. The results show a high degree of similarity between the original (unadjusted) and the adjusted estimates. Across all outcomes, the point estimates and standard errors vary only slightly. Adjusting for attrition does not change the sign or significance of any of the coefficients, showing robustness of results.

¹ Including gender in the analysis does not lead to significant changes in table C4 because the variable has no significant effect on the prediction of nonattrition and high attendance.

Table C5. Estimated Outcome Variables Adjusted for Survey Attrition

Variable	Weighted ITT estimates adjusted for attrition		ITT estimates unadjusted for attrition		N
	Coeff.	Std. Err.	Coeff.	Std. Err.	
<i>Employment</i>					
Employed	0.0286**	(0.0145)	0.0311**	(0.0144)	2,565
Wage work	0.0146	(0.0117)	0.0138	(0.0117)	2,584
Self-employment	0.0109	(0.00924)	0.0131	(0.00922)	2,584
Unemployed	0.0126	(0.0212)	0.0117	(0.0210)	2,584
Log monthly earnings - all	-0.0247	(0.234)	0.00550	(0.232)	2,700
Log monthly earnings	0.322**	(0.146)	0.321**	(0.145)	822
Log hours worked per day	0.0244	(0.0621)	0.0118	(0.0639)	363
<i>Entrepreneurship</i>					
First-time entrepreneur	0.0379**	(0.0169)	0.0410**	(0.0168)	2,702
Log of last month's profit	0.0920	(0.142)	0.113	(0.139)	2,702
Log of profit in best month	0.220	(0.150)	0.243	(0.149)	2,702
Log of profit in worst month	0.224*	(0.130)	0.247*	(0.129)	2,702
Business type: trader, shopkeeper	0.0790**	(0.0360)	0.0786**	(0.0352)	754
Business has a temporary location	0.0231	(0.0263)	0.0288	(0.0259)	754
<i>Consumption and household conditions</i>					
Log consumption per capita	0.509***	(0.0633)	0.525***	(0.0633)	2,660
Log food consumption per capita	0.451***	(0.0631)	0.464***	(0.0627)	2,660
Log nonfood consumption per capita	0.502***	(0.0642)	0.516***	(0.0641)	2,660
Log other household's expenses pc	0.427***	(0.0832)	0.471***	(0.0852)	2,660
Total No. of assets	0.283***	(0.0888)	0.279***	(0.0881)	2,702
Improved toilet	0.0318*	(0.0168)	0.0313*	(0.0167)	2,702
Concrete roof	0.0118	(0.00991)	0.0123	(0.00975)	2,702
Piped water	0.0229	(0.0152)	0.0217	(0.0149)	2,702
<i>Cognitive and socio-emotional skills</i>					
Verbal memory 6 digits	0.0632***	(0.0205)	0.0630***	(0.0203)	2,440
Impulse control	0.0847***	(0.0257)	0.0882***	(0.0255)	2,699
Greater locus of control	0.0971***	(0.0338)	0.0932***	(0.0335)	2,699
Multitasking ability	0.122***	(0.0314)	0.121***	(0.0311)	2,699
Prepared to take risks	0.0225	(0.0710)	0.00958	(0.0703)	2,692
<i>Business practices, spending and capital</i>					
Keeps separate financial records	0.0926**	(0.0413)	0.0304**	(0.0129)	2,702
Keeps financial records	0.116***	(0.0423)	0.0401***	(0.0139)	2,702
Log of capital stock	0.256*	(0.137)	0.235*	(0.137)	2,702
Log of monthly business expenses	0.230	(0.255)	0.172	(0.253)	2,702
Took any business loan	0.00915	(0.0107)	0.00588	(0.0106)	2,702
Loan for business inputs and equipment	-0.00579	(0.0105)	-0.00640	(0.0104)	2,702
Log of amount needed to start business	0.632***	(0.217)	0.732***	(0.217)	378
Log of amount borrowed to start business	0.707**	(0.349)	0.734**	(0.333)	174

Notes:

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

All specifications also include baseline outcome values when available, other control variables (that is age, marital status and educational attainment), and controlling for center fixed effects.