Economic Empowerment of Adolescent Girls (EPAG) Project

Government of Liberia, Nike Foundation and World Bank

Concept Note for an Impact Evaluation of the EPAG Project

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**Introduction**

The Government of the Republic of Liberia, together with the World Bank and the Nike Foundation, has designed a project to enhance the economic empowerment of adolescent girls and young women in Liberia. The EPAG project will (1) develop the capacity of young women to take advantage of opportunities for entrepreneurial activities in the small-scale private sector, and (2) meet the demand for skilled or semi-skilled labor from public- or private-sector firms. The EPAG project is part of a larger Bank-led Adolescent Girls Initiative (AGI), which seeks to promote the economic empowerment of adolescent girls and young women in five low-income and post-conflict countries (Afghanistan, Liberia, Nepal, Rwanda, and South Sudan). All the projects in the AGI offer training and mentorship to facilitate young women’s transition to work and share a number of common characteristics, including rigorous impact evaluations. These programs are seen as pilots in which to experiment with innovative approaches, measure the results, and provide quantitative evidence for possible scaling-up of similar interventions in the future.

The purpose of this note is to outline the concept and design for the impact evaluation of the Liberia EPAG Project. The evaluation will employ a randomized pipeline design, in which trainees are randomly selected to receive training in one of two rounds, and participants in the second round are used as a comparison, or control, group for trainees in the first round. The evaluation is expected to occur over 30 months beginning in late 2009.

**EPAG Project Design**

The EPAG project will provide skills training to adolescent girls and young women (aged 15-24) in urban and peri-urban Monrovia, to increase their employment, incomes, and well-being. The project has four components: (1) skills training for wage employment, combined with jobs placement assistance; (2) business development services and microcredit for young women entrepreneurs; (3) institutional strengthening of the government partner and implementing agencies; and (4) monitoring and impact evaluation.

Skills training will be provided both for wage employment and for entrepreneurship and business development. Training will include both technical skills as well as what are referred to as life skills, including the ability to show up for work on time, interact creatively and positively with colleagues, and communicate and negotiate, and manage personal finances. Participating adolescent girls and young women will be able to choose the training option that best suits their needs and preferences.

The institutions offering training for wage employment will be hired under performance-based contracts: providers that are more successful in placing their graduates in jobs will be given performance bonuses and will have their contract renewed and expanded; those that are less successful may have their contracts cancelled. Hence, the project will not specify *ex ante* in which sectors the skills training is to be provided. Those providing training in business development services will be similarly evaluated, and will face a similar set of incentives. Performance in this component will be measured using indicators such as: (1) the proportion of graduates who launch a business within six months of graduation, or (2) the number of these businesses surviving one year after graduation.

**Objective of the EPAG Impact Evaluation**

The EPAG project is expected to increase employment and incomes of young women in urban and peri-urban Monrovia. Experience from other regions has shown that training and entrepreneurship programs can have enormous impacts on girls’ employment prospects and earnings (Buvinic 2005; Card 2007). Since the labor market success of young women also depends on their ability to overcome significant social barriers, the training curricula will also provide information and guidance on reproductive health, child care, and endemic sexual violence and transactional sex. Conversely, the economic empowerment of young women can lead to independence and empowerment in other arenas, as well. The Liberian Ministry of Gender has specifically requested that the project examine non-labor-market outcomes, such as the incidence of victimization and violence which are a serious concern for young women in Liberia, and changes in risky behaviour which may be manifest, for example, in the incidence of sexually transmitted infections or unwanted pregnancies.

Specific questions and indicators include the following:

Are young women in Liberia prevented from obtaining productive employment due to lack of marketable skills? Will providing these skills enhance success in the labor market? Indicators include the incidence of employment and the level of income earned.

Does training in business development services lead to greater success in the development of entrepreneurial activities? Indicators include the incidence of successful launch of enterprises and the income earned from these enterprises.

What is the relative cost-effectiveness of these two alternative methods for economic empowerment, in terms of the income gains to the young women who participate? Indicators include a comparison of the relative changes in incomes and productive economic activity across the two groups receiving the interventions.

What is the impact of economic empowerment on the incidence of violence suffered or threats received by the young female participants, from partners and others? Indicators include self-reported experience of violence and other adverse events.

What is the impact of economic empowerment on the manifest outcomes of other types of behavioral risk-taking? Indicators include incidence of pregnancy and other self-reported sexually transmitted infection.

**Experimental Design of the EPAG Impact Evaluation**

This section will describe the requirements and responsibilities of the EPAG impact evaluation in relation to the rest of the project. These primarily deal with the selection of beneficiaries, the assignment of participants to experimental arms, and the steps involved in interviewing and following up participants over time.

***Identification of participants***. This project will offer training in urban and peri-urban Monrovia, in nine separate locations in Montserrado and Margibi counties. Four training providers will be contracted to offer training over these nine project sites. The total number of students was chosen as the maximum that could be provided training given the fixed budget available for training.

The evaluation firm contracted to conduct the data collection for the evaluation will also gather data on each study location in order to understand the characteristics of the local population and to calculate sample weights.

***Assignment to experimental arms***. Participants will be recruited in the initial phase of the project, at which time they will be administered a baseline survey. Following the baseline survey, participants will be assigned to one of **five** experimental arms.

First, participants will choose to apply for training in skills for wage employment (WE) or business development skills (BDS). Allocation of available places will be determined by separate lotteries for WE and BDS to be held in each of the nine project locations. The precise proportion of the sample will be 60|40, with more receiving training in BDS. Second, one-half of the participants for each type of training will be assigned to receive training in the first round, and the other half will be assigned to the control group. Assignment of participants to rounds will also be done by lottery, managed by the project coordinator with the assistance of the evaluation coordinator and the evaluation firm.

Third, those assigned to receive training in each round will be further assigned to receive training in different ways. The sample of participants in each of the BDS courses will be randomly divided in half. One half will be assigned to receive training in ordinary classes. The other half will be assigned to pairs, and will receive training in classes comprising pairs of students rather than individual students. This is to test the subsidiary hypothesis that training is more effective if girls are provided with support from a training partner.

All participants in WE classes will be trained using the paired approach. However, to ensure participation of younger women in WE classes, and to test explicitly the impact of the program on this group, women in WE classes will be stratified by age: those aged 16-18 will be recruited and selected separately than women aged 19-24. This design decision follows from the observation that previous job training programs for women have struggled to enroll and retain women from this younger age group.

Training will be conducted in two rounds. Both the treatment and control groups will be followed-up and interviewed three times – first at baseline, second at the end of the first round of training (approximately one year after baseline), and third at the end of the second round of training. In each round, the control group will be defined as those who are selected into the program, but for whom the courses do not yet have a space available. Given the structure of the control design, and as a principle of ethical programming, there is an obligation to provide training to those in the control groups who were promised training. This obligation, together with a number of other considerations discussed below, led to the experimental design described here.

Figure 1 describes the structure of the experimental design and assignment and the flow of participants across experimental groups and survey rounds. In addition, the number of participants in each experimental arm is described in Table 1.

***Survey design***. This study will consist of at least three types of questionnaires: baseline, exit poll for training participants, and follow-up surveys. In addition, the evaluation will include some key-informant-type interviews with local community and business leaders in the nine selected sites. This will provide information on the opportunities available in these communities, and the constraints to entering the labor market or establishing a business in the area.

The baseline questionnaire will consist of standard household-survey-type questions, but will not be as exhaustive or time-consuming. It will collect information on the young woman, her history and experience, her family, her (and her family’s) assets, her community, her choices, her attitudes and hopes for the future. These latter questions will most likely be self-reported beliefs, but we may include modules to measure discount rates and other characteristics more quantitatively. The follow-up questionnaires will resemble the baseline questionnaire as closely as possible. This will enable the comparison of changes in attitudes and behavior over time. In addition, the follow-up surveys will include questions on the business and employment experiences of the young women since the completion of their training.

Immediately following each round of training, the training participants will be asked to complete a short exit survey. This is intended to collect information on the participants’ views of their training, the content, pedagogy, and trainers. In addition, this survey will elicit information that might help to understand the characteristics of the participants and their environments during the training period that influence their performance during the training course. This survey will be no longer than two pages (four sides), and can be completed in half an hour or less.

The survey firm will have the primary responsibility for the design of the survey instruments, in close collaboration with the Ministry of Gender and Development and the World Bank team.

***Timing***. This pilot project will be conducted over approximately 30 months. Figure 2 shows a likely timetable for the evaluation. Within the first month, the evaluation firm will begin designing and testing the questionnaires. Recruitment of participants will begin shortly thereafter, with the baseline survey collected immediately following the selection of participants.

The large surveys (baseline and follow-up) will be scheduled at approximately one year intervals. This is partly to give the intervention some time to work. It is unlikely that the training participants will be able to find employment or establish businesses within a mere few months. The differences in the follow-up periods between the groups getting training in different rounds will provide some information to examine that question, as well as the question of whether the effects of training attenuate or (conversely) increase with time.

***Statistical power calculations*.** Given the fixed budget, the cost of the intervention will largely determine the number of participants. This in turn will determine the statistical power of the intervention’s evaluation. Table 1 shows the approximate number of participants in each survey round, including both the full surveys of participants and the short exit polls of those leaving training. At the end of the project, the evaluation team will have conducted roughly 10,000 surveys – 7500 full surveys and 2500 exit polls. These 7500 consist of 2500 at baseline, 1250 on the control group, 2250 on the entrepreneurship training, and 1500 on wage training. These numbers include 1875 surveys on those who receive training in pairs and individually.

The 7500 full surveys will form the database on which we can analysis the program’s impact; this comprises 1250 observations on the control group, and 3750 observations on 2500 young women who come through the training program. Half of the young women in the sample serve both in the control group and in the treatment group (in round 2). Repeated observations on individuals cannot be treated as truly independent. Moreover, the outcomes of the young women who receive training in pairs are by expectation correlated. This is a positive thing for the training, but it complicates the evaluation, which must take account of the possibility that the outcomes are due to the correlation of outcomes within pairs rather than the impact of the training. This problem is known as the Kish design effect of intracluster correlation. Happily, despite the clustering, the sample size provides very good statistical power to detect differences between the control and treatment groups, given that a difference really exists.

There have been few studies of similar training programs in developing countries. Card et al. (2007) found that a training program in the Dominican Republic increased the incomes of participants by 10 percent, relative to those who had not received the training; Polanco (2007) found that a different training program, also in the Dominican Republic, increased incomes of participants by 24 percent. Other studies from Peru find increases of 12-60 percent after 6 months, and 13-40 percent after one year (Card et al. 2007).

Taking the more conservative estimate, we will be able to confirm rigorously a 10 percent difference between control and treatment groups, and between the two treatment groups, with 95 percent confidence, with a power of greater than 95 percent. If the true difference between the groups is larger, the statistical power of the study quickly approaches 100 percent. Based on previous studies, we expect that the interventions will yield an improvement in incomes of between 10 and 20 percent. If we assume a Kish design effect of 1.5, that is, a 50 percent correlation between the participants in the paired training, the statistical power to answer this study’s primary questions is reduced somewhat at the lower end of the expected range of true outcomes, but is otherwise unaffected, due mainly to the relatively large sample sizes (Figure 3A and 3B).

**Figure 3. Power to detect impact of training (Left: Kish deff = 1, Right, deff = 1.5).**

***External validity and replicability*.** The identification strategy and assignment of beneficiaries to training is not perfect. Assignment to treatment is random, but assignment to wage employment versus entrepreneurship training is not. Furthermore, The identification strategy will permit the measurement of the impact of the training *on those who choose the training*. It will *not* help to understand what would happen if the training interventions were administered to to any young woman selected at random from the population. However, it *will* provide a good estimate of the impact the program would have if it were scaled up to a larger population of young women who express an interest in receiving training and assistance in either wage work or entrepreneurship.

***Ethical concerns*.** This evaluation presents a number of potentially sensitive issues for human subjects review. In the conduct of the research, the confidentiality of data must be ensured. This is especially tricky with panel surveys, where data must be matched across rounds by identifying characteristics. First, as with all repeated surveys, it is important that the survey team maintain contact over time with the beneficiary and control populations. In order to facilitate this, survey identifiers must be maintained and available repeatedly, possibly to different teams of enumerators. The survey firm will undertake whatever steps are necessary to guarantee confidentiality of data.

Second, at the request of the Ministry of Gender and Development, the project will be collecting information on the incidence of violence, risky behavior such as unprotected sex, and unwanted pregnancy. The enumerators must receive training in sensitive and appropriate techniques for obtaining this information. More importantly, at each round of data collection, the participants will be assured that they are under no obligation to provide this information, and that their continued participation in the program in no way depends on their willingness to answer these questions, or on the answers they give. In addition, the enumerators must understand how to respond in cases where they receive information which suggests that the respondent is at exceptional risk, for example of harm from violence. Finally, although the enumerators will ask for self-reported symptoms of sexually transmitted infections, and will not conduct physical exams, the enumerators must be provided with knowledge and training necessary to assist the respondents to seek diagnosis and treatment in cases where they suspect the possibility of infection.

**References**

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**Figure 1. Evaluation design (flow of participants across sample survey groups).**

**Figure 2. Evaluation timetable**



**Table 1. Number of participants per survey per round**

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | **Round 0** | **Round 1** | **Round 2** | **Total** |
| **Full Surveys** |  |  |  |  |
|  **Baseline** | **2500** | **0** | **0** | **2500** |
|  **Control** | **0** | **1250** | **0** | **1250** |
|  **Enrep follow-up single** | **0** | **375** | **750** | **1125** |
|  **Entrep follow-up pair** | **0** | **375** | **750** | **1125** |
|  **Wage follow-up 16-18** | **0** | **250** | **500** | **750** |
|  **Wage follow-up 19-24** | **0** | **250** | **500** | **750** |
| **Total** | **2500** | **2500** | **2500** | **7500** |
| **Exit polls** |  |  |  |  |
|  **Entrepreneurship** | **0** | **750** | **750** | **1500** |
|  **Wage Employment** | **0** | **500** | **500** | **1000** |
| **Total** | **0** | **1250** | **1250** | **2500** |