



Impact Evaluation Mentoring for Governments in East & West Africa



Impact Evaluation Mentoring for Governments in East and West Africa Proposal

Disseminating market information via mobile phones to cashew
producers: an impact evaluation in Guinea-Bissau

Presented to

Partnership for Economic Policy (PEP)

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**Mandatory template to submit Intervention Design
1st August 2019**

Policy Initiative For Impact Evaluation

SECTION A – RANDOMIZATION

1. Implementation of random assignment

The impact evaluation of your proposed policy must be carried out through the use of a randomized control trial (RCT). This experimental methodology requires the creation of a *treatment group*, made up of eligible subjects participating in the policy initiative, and a *control group*, composed of equally eligible subjects who are excluded from program participation.

Describe the mechanism you will use in order to randomly select subjects into either group (for example, a lottery among eligible subjects, implementation in certain geographic areas and not others, a progressive inclusion of subjects into the program or phase-in mechanism, etc.).

With households as the unit of observation, the randomization will be done in three stages, following the next steps:

1. A random selection of 288 (+ replacements) villages/neighborhoods (geographical clusters) across all regions of Guinea-Bissau (using the 2009 census, or the 2019 one if available on time) and randomization of treatments at the cluster level: 96 of these villages will be allocated to the control group and 96 to each of the two treatment arms.
2. Within-village random selection of households to be contacted to check for their eligibility (also using census data). These households are visited in person by enumerators, who check for eligibility, register consent for participation in this study and complete the baseline survey for the required number of eligible households: 5 per “control village” and 10 (5 treated + 5 untreated) per “treated village” (details below).
3. Once households complete the baseline surveys, an on-the-spot within-village lottery (programmed in the tablet) among eligible households will randomly determine 5 treated and 5 non-treated households in “treated villages”. Stratification by literacy seems a potentially important element for the randomization, either at the household or village level. In control villages no lottery will take place.

2. Strategies to deal with contamination and spillovers

Two common problems affecting random assignment to treatment, and hence, the unbiasedness of results, are contamination and spillovers between treatment and control groups. Explain how your randomization strategy works to prevent these issues.

Randomizing both at the village and the household level allows us to measure spillovers directly within treated villages:

- We will be able to measure spillover effects of our interventions by comparing the outcomes of the “untreated” (control) households in the “treated villages” with those of the households in “control villages”.
- Instead, we will test the direct effect of our intervention by comparing the outcomes of the treated households in the treated villages with those of the untreated farmers in the same cluster.

By -randomly- choosing the 288 villages in our study across the whole country we make the average distance between clusters large, contributing to minimize potential spillovers or contamination between “control” and “treatment villages”.

We plan to select the 288 study villages by taking 10,000 random draws of 288 villages from the eligible set of villages, and select the draw with the maximum value of the minimum distance between the geographic centers of any two villages. This approach eliminates villages with overlapping boundaries and should potentially minimise spillovers across villages.

Still, if communication between individuals in different clusters occurred, this would produce a downward bias on our effects, as we are dealing with an information intervention with an expected positive effect.

We have agreed with the mobile partner operator that there will be not promotion/marketing of the service beyond the study villages.

3. Sample size

Enter an estimation of the size of the universe (maximum potential size) of the untreated population that will be available for random assignment into either the treatment or control group during the randomization stage.

Maximum number of untreated persons:

A gross estimate for the size of the universe of potentially eligible cashew-owning households in Guinea-Bissau produces a total of 100.000 (with about 1 million individuals): total population is about 2 million people, households have an approximate average size of 10 individuals and about 50% of them own at least one cashew plantation.

We propose to choose a sample of 2,400 households:¹

- 480 households in “control villages”, 96 villages with 5 households each.

¹ Plus 350 extra households (1 per “control village” and 2 per “treated village”) to take potential attrition into account, see section C.4. for details.

- 1920 households in “treated villages” = 2 x 960 households for each treatment arm: 2 x (480 non-treated + 480 treated households), 96 villages, 10 households each.

With an average household size about 10, the households in the study will contain about 24,000 people.

SECTION B – INTERVENTION DETAILS

1. Description of the intervention

Describe in an exact manner the experience that an eligible subject who is selected for program participation goes through as a result of being selected. This is the definition of the treatment to be tested in the study. If alternatives are to be tested as well, list each treatment below and describe.

Our intervention seeks to provide timely and accurate market information to cashew producers during their marketing season (usually between March-July)..

On the day of the baseline survey, after the within-village randomization into treatments, the enumerator explains the farmer the characteristics and timing of the treatment he or she has been assigned to:

1. Treatment 1 (baseline treatment): Explaining how to use the mobile market information service (M-MIS), possibly combining a personal explanation with a video, ask for their consent in participating in the study and to receive the first messages, and to save the M-MIS number in the contact list.

We plan to offer the service at no cost for the first 2 messages, starting with the cashew campaign in March 2020. After the free trial messages, participants in this arm will decide whether to continue receiving the weekly messages at the cost of a monthly subscription fee (activated using a simple USSD menu/in-call messages).

Once the subscription service is accepted, it will be renewed on a monthly basis (automatically), unless the user decides to discontinue the service. Each monthly subscription will give the right of the recipients to receive 4 M-MIS messages. Subscribers will have access to the latest messages on demand as part of their subscription.

Users in this arm that chose to opt-out from the service will receive monthly reminders offering them to re-activate the service.

The content of the weekly messages:

- i. Synthesised and simplified “news” about the national cashew market.
- ii. A range of the latest region-specific farmgate prices.

iii. An analyst “advice” on whether it is a good time to sell, based on a simple message to describe whether the prices are likely to move at all/upward/downward.

An example of the message² that will be sent to farmers (in this case to farmers in the Biombo region) is:

“New Chinese exporter joined. Prices in Biombo: 450-550CFA/kg. Prices rising in the next weeks. Sell, if the price reaches 600CFA/kg.”

2. Treatment 2 (financial barriers treatment): Same as the baseline treatment, except that all costs associated with the use of the mobile market information service will be covered for (**we will cover the corresponding costs with the mobile network**, such that these are free for the chosen farmers).

3. Control group: no intervention, just a baseline survey and phone-call follow-ups.

2. Application mechanism

Describe in an exact manner the procedure that a potential candidate must follow to be considered for program participation. List the documents that candidates must present and describe, in detail, the activities in which they may be required to participate during the screening process, (interviews, informative sessions, etc.).

1. Sampling: we plan to use as sampling frame the latest available census data (expected in 2019) from the National Institute for Statistics, which will include a list of all villages and households within them.
 - a. We will randomly select 288 villages from the sampling frame.³
 - b. Within each chosen village we will randomly reorder the list of households from the sampling frame of that village.
2. Screening: Upon arrival to a selected village, the data-collection team will check with the village leader the randomly re-ordered list of households to identify the first up to 20 households that own a cashew plantation, our key screening condition.
3. The data-collection team will visit and seek consent to participate in the study from the first 10 (5 in control villages) households identified in the list of the -up to- 20 potential participants, and replace any household that actually does not meet the full list of eligibility criteria (see below) with one of the -up to- 10 replacement households.

² In our best case scenario, which seems the most likely, the messages will be sent via a call with a pre-recorded message.

³ We plan to restrict the sampling frame to villages/municipalities with more than **25 households**, as this will prevent the inclusion of villages in our study sample that might be too small to have at least 10 households that fulfill the household-level screening criteria.

3. Eligibility mechanism

Detail the eligibility criteria and describe exactly how each item considered for eligibility is assessed in practice. Explain how selected candidates are informed about selection into the program.

One by one, each household who has been randomly selected as a potential candidate for the study will be visited by an enumerator (until enough eligible households from the randomly generated list are found in the corresponding village). The enumerator will briefly explain the purpose of her visit and check that the following eligibility conditions are satisfied:

1. The household owns at least one cashew plantation (we know that about 50% do). From our experience with previous surveys, we know we can trust people's word on this.
2. The household head or, alternatively, someone with good-enough knowledge of and involvement in the family's cashew business (cultivation, collection and trading) who lives permanently in the house is present. From experience, we know we can also trust people's word on this.
3. Someone permanently living in the house owns a well-working mobile phone, charged, with signal at home and knows how to use it. The enumerator will check the phone works correctly by calling the number and by sending a text message and reading it herself.

If these conditions are met, the enumerator will explain the candidate the basic elements of the study, including that a within-village lottery determining who will participate in the program will take place. If the candidate agrees to participate, he will be asked to sign a consent form. Those who consent will then complete a baseline survey.

The last stage of the randomization will take place on the spot, within-villages. All those individuals who granted consent and are assigned to a treatment will be informed of their selection into the program by the enumerator, who will explain the corresponding treatment.

SECTION C – EVALUATION DETAILS

1. Timeline

Provide estimated dates for the milestones of the impact evaluation project, listed below. Remember that sufficient time must be allowed for the results of policy interventions to materialize, depending on the nature of the policy initiative.

Approximate date – baseline data collection: by the end of 2019

- June - August: full design of the evaluation.
- September - October: random selection of villages and households using the census.
- November - December: checking for households' eligibility and registering consent, running the baseline survey and explaining the corresponding treatment to selected households.

Approximate date – start of intervention: 2020 cashew nut season (February- until the end of August)

- **January/March:** sending a **voice message** communicating the start of the annual mobile market information service to the households allocated to treatments.
- **April to end of August:** the different treatments are implemented.

Approximate date – follow-up data collection: by the end of 2020.

- **September- December 2020 :** running the endline phone survey.⁴

2. Main research question

State your main research question as succinctly as possible.

Can the communication of price information and **tailored market advice** improve farmers' market and economic performance, when production is highly concentrated on a single crop with a highly volatile price?

3. Primary outcomes

State your primary outcome of interest, i.e., the most important variable on which you expect to observe an impact as a result of the program. State up to three outcomes, listed in order of importance.

Our theory of change suggests that if we increase the information set of the farmers they will be able to increase their revenue. The most important expected outcomes:

Primary outcome: Increased farmgate prices. Selling cashew nut production at a higher price should directly imply higher household revenue in this context, as we expect a

⁴ Between November and December 2021 we could run another (potentially smaller/phone based) survey guided by the findings and results obtained in the 2020 one, to explore interesting longer-term effects.

null or very small effect of improved market information and higher prices on quantities sold (to be explored in the analysis). **A key feature of our intervention is that will provide information on the more appropriate time to sell (which should allow for some temporal arbitrage).** With high poverty incidence and revenues from cashew amounting to approximately 40% of total household income for cashew producers (WB 2017), we expect a significant impact of this increased revenue on important economic outcomes associated with improved household welfare.

Secondary outcome: Improved economic information and market understanding. Together with measuring the use of mobile market information service (through, for example, the number of messages listened, subscriptions made), the information outcome will be measured with an index constructed from a set of questions related to market understanding, including the state of the cashew season and prices (both practiced and expected).

Tertiary outcome: Higher household welfare. Higher food security of the household and improved access to education and health (including psychological health).

SECTION H – OTHER CONSIDERATIONS

1. Budget

The proposed budget combines the USD47,000 for research and data-collection from PEP with 10,000£ from a PEDL exploratory grant (**Total approx. = USD59,000**). The PEDL grant consists of a maximum of 40,000£, with the second installment for a maximum of 30,000£ to be disbursed in April, after successful completion of the first part of the project. Hence, we propose the following preliminary budget, containing only PEP money + the initial 10,000£ from PEDL. Excess financial support from PEDL or other potential partners would be used to increase sample size and the length of the endline survey, potentially making this also in the field instead of phone-based.

Item	Cost in US dollars
<i>RESEARCH</i>	21,813
Research staff	13,500
Others	8,313
<i>DATA COLLECTION</i>	25,858
Gasoil/fuel	5,861
Car rental + drivers	13,222
Generators	2,139
Enumerators	8,923
Field supervisors	4,068
Endline survey	2,393
<i>Unexpected expenses</i>	513
TOTAL	58,933

