

Pre-Analysis Plan for Phase III ‘Follow-Up Survey’ of ‘The Effects of Media Messages on Social Attitudes in Uganda’

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This document summarizes key features of Phase III of the Media and Social Norms in Uganda Randomized Trial and describes the authors' pre-analysis plan (PAP). The design of Phase I of this project has been registered under the following ID: 20160119AA. The design of Phase II of this project has been registered under the following ID: 20161016AA. This document predates endline data collection, and our plan is therefore blind to outcomes. All design details not described in this PAP are described in that registered for phase II of this study.

Any contingency not accounted for in this PAP or the PAPs for Phases I and II will be dealt with according to the Standard Operating Procedures for Don Green's lab at Columbia as of May 22, 2017.

1 Design for Follow-Up on Phase II Intervention

This PAP relates to the analysis of new data collected in villages exposed to the intervention described in the PAP for phase II. We describe our new data collection in Phase II villages as the 'Follow-Up' survey.

1.1 Sampling and subjects

We sample four distinct groups of subjects, and in each case employ a slightly different sampling strategy. The four groups, as well as the manner in which we plan to analyze them in relation to previous phases of the study, are described in table 1.

Table 1: Sampling and Subjects

Label	Description	Analysis Plan
New Women	We are returning to clusters in which we had very few women compliers in the phase II endline data collection. We continue with the tracking sheets and sampling strategy employed during phase II. Since we do not know in advance which respondents interviewed in this manner will be compliers, we expect that some women respondents will be compliers and others not.	To analyze the new data on women from surveying in phase III we will pool data on the Phase II with the Phase III women according to their respective compliance status. We will control for the survey round using fixed effects.
Complier Re-Interviews	We are returning to reinterview all respondents identified as compliers in Phase II of data collection.	We will analyze long-run complier average causal effects in two ways. First, by simply analyzing effects among compliers using the data collected in phase III. Second, we will construct a panel by pooling complier data from phase II and phase III (not including women compliers), in order to increase efficiency through trend modeling, and to explore dynamics of the treatment effect (differential rates of decay and time-heterogeneous diffusion to non-compliers).
New Teenagers	In parallel to the re-interviewing of compliers from phase II, we are seeking to interview teenagers that we did not sample in phase II.	Teenagers surveyed during phase II will be pooled with those surveyed during phase III in order to conduct analysis of treatment effects among compliers. We will control for the survey round using fixed effects.
Parents of New Teenagers	When interviewing new teenagers during phase III, we will also interview their parents. These parents' compliance status is not known in advance.	When conducting analysis of intra-household spillovers (i.e. between parents and teenager children), we will pool the parent-teenager pairs from the phase II endline and the phase III follow-up, including a fixed effect for survey round. We will also analyze the new complier parents along with the other adults, again with a dummy for sampling stratum.

2 New Measures of Outcomes and Mediators

The measures that will be collected during this round of the study are drawn from surveys with adult and teenage villagers as well as from surveys with VHTs. The questionnaires for adults and teenagers are submitted as supplementary materials to the pre-analysis plan. The questionnaire for VHTs is identical to the one used in phase 2 of this study. Most of the measures which will be collected and analyzed in this round of the study have already been described in the pre-analysis plan for phase 2 of this study. In the following section, we will only describe outcome measures that have been newly added for this round of the study. The general coding rules laid out in the pre-analysis plan for phase 2 of this study still apply.

2.1 General Coding Rules

- Where not otherwise indicated, outcome measures will be constructed from single items.
- Items with two answer categories will be coded as indicator variables which take the value 1 if the respondent chooses the answer category that is more in line with the norm portrayed in our videos. In the attached questionnaires, answer options that we consider to be in line with the norm portrayed in the videos are shown in green.
- For items with more answer categories, we will create numerical variables that take higher values the more the respondent's answer is in line with the norm portrayed in the video.
- Indices will be constructed in the following manner: All variables that form part of the index will be standardized to range from 0 to 1 by dividing by their maximum possible value. Subsequently, they will be summed and divided by the number of variables that form part of the index.

2.2 IPV Mediators

Our questionnaire includes questions measuring three kinds of mediators for effects of the treatment on attitudes towards intervention in IPV (IPV conative attitudes). These mediators represent possible outcomes affected by treatment, and we will use the same kinds of statistical models described in the Phase II PAP to test for treatment effects on mediators.

1) Risk of violent spirals

The first kind concerns the respondent's perception of the risk of violence spiraling out of control: The item *spiral_risk* measures the respondent's perception of how likely it is that an isolated act of domestic violence escalates into much more severe levels of violence.

2) Empathy

The second kind concerns the empathy that a respondent feels towards victims of domestic violence: The item *empathy_pair* measures the extent to which respondents believe that victims of domestic violence suffer greatly.

3) Costs of reporting or intervening in domestic violence

The third kind concerns the extent to which respondents perceive the act of intervening in cases of domestic violence as costly:

- The item *revenge* asks whether the respondent believes that intervening in domestic violence will lead the perpetrator or his friends and family to take revenge.
- The item *intervention_consequence* measures whether the respondent believes that he or she would be scolded for informing others about an incident of domestic violence.

2.3 Conative Attitudes Toward IPV

The item *free-riding* measures the willingness of a respondent to intervene in a case of domestic violence. The wording of this outcome is randomized to either: prime the respondent to believe that he or she is the only one who has observed the incident; or prime the respondent to believe that the incident has also been observed by other community members.

2.4 IPV Incidents and Reporting

We have added two additional measures to the incidents and reporting section that ask female respondents about IPV incidents that have occurred in their households. From these questions, we will construct the following two outcome measures:

- *household_violence* will be a count variable that reflects the number of incidents that occurred in the respondent's household.
- We will create a categorical variable that takes the value 0 if *household_violence_frequency* = Less than once a month, 1 if *household_violence_frequency* = About once a month, 2 if *household_violence_frequency* = Once a week and 3 if *household_violence_frequency* = Almost every day.

Additionally, the item *beating_frequency* which was asked of all respondents in the previous round is now only asked if the respondent is male.

2.5 Teacher Absenteeism

1) Frequency of Absenteeism

In the teacher absenteeism section of the questionnaire, we added a question that measures the extent of teacher absenteeism in nearby schools (*absent_frequency*). We will use this item to create a categorical variable that takes the value 0 if *absent_frequency* = Not at all, 1 if *absent_frequency* = A few times, 2 if *absent_frequency* = Once a week, 3 if *absent_frequency* = Almost every day.

2) Extent of activism around teacher absenteeism

We also added two measures of the level of activity surrounding the problem of teacher absenteeism: *teacher_confronted* is a question that will only be asked to respondents who are teachers. It asks whether teachers in the village have been confronted by parents about their performance. Using the item *pta_frequency*, we will create a categorical variable that takes the value 0 if *pta_frequency* = Less than three times a year, the value 1 if *pta_frequency* = Three times a year, the value 2 if *pta_frequency* = Just once a month and the value 3 if *pta_frequency* = Even more than once a month.

3 New Hypotheses

Our PAP includes all hypotheses listed in the PAP for phase II – we include only those that are specific to the PAP for phase III below. All analyses will be conducted separately for men and women, by compliance status.

3.1 Outcomes

Hypothesis 1 *Free-Riding (Main Effect): The treatment will increase the proportion of compliers who state that they would take action (One-tailed).*

Hypothesis 2 *Free-Riding (Heterogeneous Effect): A treatment-induced increase in the proportion of compliers who state that they would take action is greater when respondents are primed to think that others in the community are unaware of the hypothetical IPV incident (One-tailed).*

Hypothesis 3 *Absenteeism Frequency (Compliers): Fewer or more cases of teacher absenteeism are mentioned by compliers in treated clusters (Two-tailed).*

Hypothesis 4 *Absenteeism Frequency (Never-Takers): Fewer cases of teacher absenteeism are mentioned by never-takers in treated clusters (if incidence rate decreases) (One-tailed).*

Hypothesis 5 *IPV Victimization: Fewer cases of IPV victimization are mentioned by respondents in treated clusters (if incidence rate decreases), more cases of IPV victimization are mentioned by compliers in treated clusters (if treated compliers more inclined to report to enumerators) (Two-tailed).*

Hypothesis 6 *Teacher Accountability: Teachers in treated clusters are more likely to mention having been confronted by parents (if no significant or positive effect on absenteeism frequency - One tailed). Teachers in treated clusters are more or less likely to mention having been confronted by parents (if significant negative effect on absenteeism frequency - Two-tailed).*

Hypothesis 7 *PTA Frequency: Respondents in treated clusters report higher rates of PTA meetings in treated clusters (if no significant or positive effect on absenteeism frequency - One tailed). Respondents in treated clusters report higher or lower rates of PTA meetings in treated clusters (if significant negative effect on absenteeism frequency - Two-tailed).*

3.2 Mediators

Hypothesis 8 *Empathy: A higher proportion of treated Compliers expresses empathy toward IPV victims (One-tailed).*

Hypothesis 9 *Violent Spiral: A higher proportion of treated Compliers expresses the belief that initial violence is likely to beget more violence (One-tailed).*

Hypothesis 10 *Reporting Costs: A higher proportion of treated Compliers expresses the belief that intervention in IPV cases will not be punished by members of the community (One-tailed).*

3.3 Moderators

Hypothesis 11 *Reporting Cost Heterogeneity: Positive treatment effects on intervention proclivity are higher when respondent reports higher values for reporting cost index (constructed from reporting_cost1, reporting_cost2 and reporting_cost3) (One-tailed).*

Hypothesis 12 *Husband Empathy: Positive treatment effects on intervention proclivity are lower when respondent reports higher values for husband's empathy (empathy_husband) (One-tailed).*

Note: The *empathy_husband* moderator is a post-treatment outcome. We will test for the effect of treatment on *empathy_husband*.

Hypothesis 13 *Anti-Intervention Norm: Positive treatment effects on intervention proclivity are higher when respondent reports that there is a norm against intervention in others' affairs (norm_against_intervention) (One-tailed).*

Hypothesis 14 *Dose-Response: Higher doses of the treatment will produce larger effects (One-tailed).*

Hypothesis 15 *Strategic Dose-Response: The effect of lower doses of treatment on outcomes is higher in places where the sample exhibits lower aggregate dosage at the cluster level (One-tailed).*

Note: This hypothesis arises from a model in which individuals are less likely to personally intervene when they know that a large number of people in their community have been exposed to treatment. In such

communities, higher dosages of treatment are required for a given individual to intervene. Thus, we see this as another test of the free-riding hypothesis.

4 Panel Analysis

For several respondents in the study, we will have repeated measurements both of outcomes of interest and of covariates such as age and marital status. In addition to the endline and follow up data collection in phases I, II and III, we also have a baseline survey in phase I, a lab-in-field study planned for the studies originally included in phase II, and randomized audits of 10% of the sample in every survey.

In summary, we have the following sources of data:

1. Phase I: Baseline adult survey in 28/56 clusters
2. Phase I: Audit of Baseline adult survey among 10% of respondents in 28/56 clusters
3. Phase I: Endline adult survey in 56 clusters
4. Phase I: Audit of Endline adult survey among 10% of respondents in 56 clusters
5. Phase I: LC1 baseline survey in 28/56 clusters
6. Phase I: LC1 endline survey in 56 clusters
7. Phase II: Endline adult survey in 112 clusters
8. Phase II: Audit of Endline adult survey among 10% of respondents in 112 clusters
9. Phase II: Endline teenager survey in 112 clusters
10. Phase II: Audit of Endline teenager survey among 10% of respondents in 112 clusters
11. Phase II: VHT endline survey in 112 clusters
12. Phase II: Audit of VHT endline survey among 10% of respondents in 112 clusters
13. Phase III: Follow-up adult complier survey in 112 clusters
14. Phase III: Audit of Follow-up adult complier survey among 10% of respondents in 112 clusters
15. Phase III: New parents of teenagers and women survey in 112 clusters
16. Phase III: Audit of New parents of teenagers and women survey among 10% of respondents in 112 clusters
17. Phase III: New teenagers survey in 112 clusters
18. Phase III: Audit of New teenagers survey among 10% of respondents in 112 clusters
19. Phase III: Lab survey of compliers in 56 clusters
20. Phase III: Audit of Lab survey of compliers among respondents in 56 clusters
21. Phase III: VHT follow-up survey in 112 clusters
22. Phase III: Audit of VHT follow-up survey among 10% of respondents in 112 clusters

The following panels will be constructed:

1. 56-village adult panel: PH1 baseline, PH1 baseline audit, PH1 endline, PH1 endline audit, PH3 lab survey, PH3 lab audit
2. 56-village LC1 panel: PH1 baseline, PH1 endline, PH3 lab survey, PH3 lab audit
3. 112-village adult panel: PH2 endline, PH2 endline audit, PH3 follow-up, PH3 follow-up audit

4. 112-village teenager panel: PH2 endline, PH2 endline audit, PH3 follow-up, PH3 follow-up audit
5. 112-village VHT panel: PH2 endline, PH2 endline audit, PH3 follow-up, PH3 follow-up audit

Whereas the 56-village panels exhibit temporal variation with respect to treatment status, the 112-village panels do not. We nevertheless plan to use the panel structure of the data in order to increase efficiency through trend modeling, and to explore dynamics of the treatment effect (differential rates of decay and time-heterogeneous diffusion to non-compliers).