CANDOUR Ghana Protocol: Ghana COVID-19 Vaccinations and Financial Incentives

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Motivation

Achieving global vaccination against COVID-19 is a critical worldwide challenge. While COVAX is planning mass vaccination of Africa in 2022, there are substantial challenges. Cash incentives have been proposed as a way to improve the efficiency and equity of the roll-out in Africa. There is a large body of experimental evidence suggesting that financial incentives can promote the adoption of preventive health habits – a recent example is Hussam et al. (2022). The evidence with respect to the promotion of COVID-19 vaccination uptake has been mixed. Financial incentives are reported to be effective based on experimental evidence from a random control trial in Sweden (Campos-Mercade et al., 2021) and the analysis of the Ohio cash lottery program (Barber and West, 2022). On the other hand, there is evidence from a California RCT suggesting that vaccine update does not respond to financial incentives (Chang et al., 2021) and a recent analysis of the U.S. cash lottery programmes suggests they have no significant effect on COVID-19 vaccine uptake (Law et al., 2022)

To evaluate whether cash incentives affect the willingness to get the COVID-19 vaccine, we are undertaking a field experiment, designed and conducted in consultation with the University of Ghana and the Ghana Health Service. The experiment will evaluate the impact of cash incentives on vaccine uptake.

The primary goal is to determine whether financial incentives significantly increase vaccination rates in the Ghana population. We have the following hypotheses:

- H1: Subjects offered financial incentives in the combined Low and High Cash treatment conditions will have higher vaccine rates than subjects in the Placebo treatment condition who receive no financial incentives for vaccinations;
  - H1a: Subjects offered financial incentives in the High Cash treatment conditions will have higher vaccine rates than subjects in the Placebo treatment condition who receive no financial incentives for vaccinations;
  - H1b: Subjects offered financial incentives in the Low Cash treatment conditions will have higher vaccine rates than subjects in the Placebo treatment condition who receive no financial incentives for vaccinations;
- H2: Subjects offered financial incentives in the combined Low and High Cash treatment

conditions will have higher vaccine rates than subjects in the Health Message treatment condition who receive no financial incentives for vaccinations;

  - H2a: Subjects offered financial incentives in the Low Cash treatment conditions will have higher vaccine rates than subjects in the Health Message treatment condition who receive no financial incentives for vaccinations;
  - H2b: Subjects offered financial incentives in the High Cash treatment conditions will have higher vaccine rates than subjects in the Health Message treatment condition who receive no financial incentives for vaccinations;

- H3: Subjects offered financial incentives in the High Cash treatment condition who receive a $10.00 financial incentives for vaccinations will have higher vaccine rates than subjects in the Low Cash treatment condition who receive a $3.00 financial incentives for vaccinations;

- H4: Subjects in the Health Message treatment will have higher vaccine rates than subjects in the Placebo treatment;

A secondary hypothesis is that:

- H5: Self-reported vaccination intentions will be higher than actual validated vaccinations.

A major concern associated with financial incentives for vaccines is that they have negative spillover effects on the vaccination rates of those not receiving these financial incentives. We have the following hypotheses:

- H6: The Low and High Cash treatments will have a negative effect on vaccination rates of subjects assigned to the Placebo treatment in, respectively, Low and High Cash treatment villages. This implies that the overall village cluster effect for each of the treatment arms will be lower than the estimated effect for individuals treated with their respectively assigned treatments.

- H7: The Low and High Cash treatments will have a negative effect on vaccination rates of non-experimental subjects that will be correlated with their proximity to Low and High Cash treated subjects.

- H7: The Low and High Cash treatments will have a negative effect on vaccination rates of the non-treated spouses of individuals that are treated.

Design

The randomized control trial is designed to measure the direct impact of financial incentives on individual vaccine update rates in the Ghana rural population. The clustered randomized control trial will randomly select 6,552 participants from selected rural households; in 312. villages and from 6 Ghana Districts. We adopted a cluster random assignment design in order to address spillover effects that could have important implications for evaluating the overall benefits (and costs) of implementing financial incentives for vaccine uptake (Miguel and Kremer, 2004; Wilke, Green and Cooper, 2020). Village clusters are randomly assigned to receive on of four video treatment arms: a placebo, a standard health message, a high cash incentive ($10) and a low cash incentive ($3). Randomly selected participants within a village will be assigned to one of the four video treatment arms. In addition, a proportion of subjects (25 percent) within village clusters assigned to one of the three treatment arms receive the placebo treatment. We incorporate placebo treatments in the design in order to facilitate the identification of both direct and indirect treatment effects that are decoupled from the delivery mechanism (Wilke, Green and Cooper, 2020; Halloran and Hudgens, 2018).

Video Treatments

The experiment has four treatments that are delivered in a short video:

- Treatment 1: A 45-second placebo video that provides general information about the benefit of using solar power to charge household electrical appliances.
- Treatment 2: A 45-second standard health COVID-19 vaccine promotional and information video (modeled on the videos produced by the U.S. Center for Disease Control).
- Treatment 3: Low Cash Incentive treatment – the first 30 seconds are identical to the Health video – the last 15 seconds inform viewers that that they will earn $3 if they receive the COVID-19 vaccine within the next 6 weeks.

- Treatment 4: High Cash Incentive treatment – the first 30 seconds are identical to the Health video – the last 15 seconds inform viewers that that they will earn $10 if they receive the COVID-19 vaccine within the next 6 weeks.

The four treatment videos are available at
`https://www.youtube.com/watch?v=PeM1cpCU0bA&list=PLBGbIwQfB9sez5Ww6xcKmwBQa45Z3Vvwd`.

Sampling Strategy

We will conduct a cluster sample of 312 villages and households from 6 Ghana districts. We will randomly select household occupants who are over the age of 18, eligible for a COVID-19 vaccine, and who have not yet been vaccinated. We offer a detailed discussion below. Sampling 21 individuals in each village results in a total sample of 6,570. Figure 1 summarizes the sampling stages that we will implement in the experiment.

The vaccine treatments have been designed to be delivered door-to-door, by enumerators working with the University of Ghana. By adopting the door-to-door implementation we will ensure a high degree of control over which households receive each of the informational treatments and which do not. This is essential for measuring information spillovers and to estimate the cost-effectiveness of the intervention. For predominantly rural Ghana villages, door-to-door visits are also the most feasible choice given the low levels of literacy and cell-phone ownership.

We briefly describe the stages of the sampling and treatment assignment.

First sampling stage: Region and District selection

There are 16 regions in Ghana. Districts are classified in three types: Ordinary Districts with a minimum population of seventy-five thousand (75,000) people, Municipal Districts with a minimum population of ninety-five thousand (95,000) people, and Metropolitan Districts with a minimum population of one hundred and fifty thousand(150,000) people.

Working with the District offices of the Ministry of Health we selected two regions (Eastern and Central) and six districts that were scheduled to have COVID-19 vaccine supplies made available in the months of December, 2021 and January, 2022. Vaccine supplies are being made available on a district by district basis in Ghana. The six districts in our sample are:

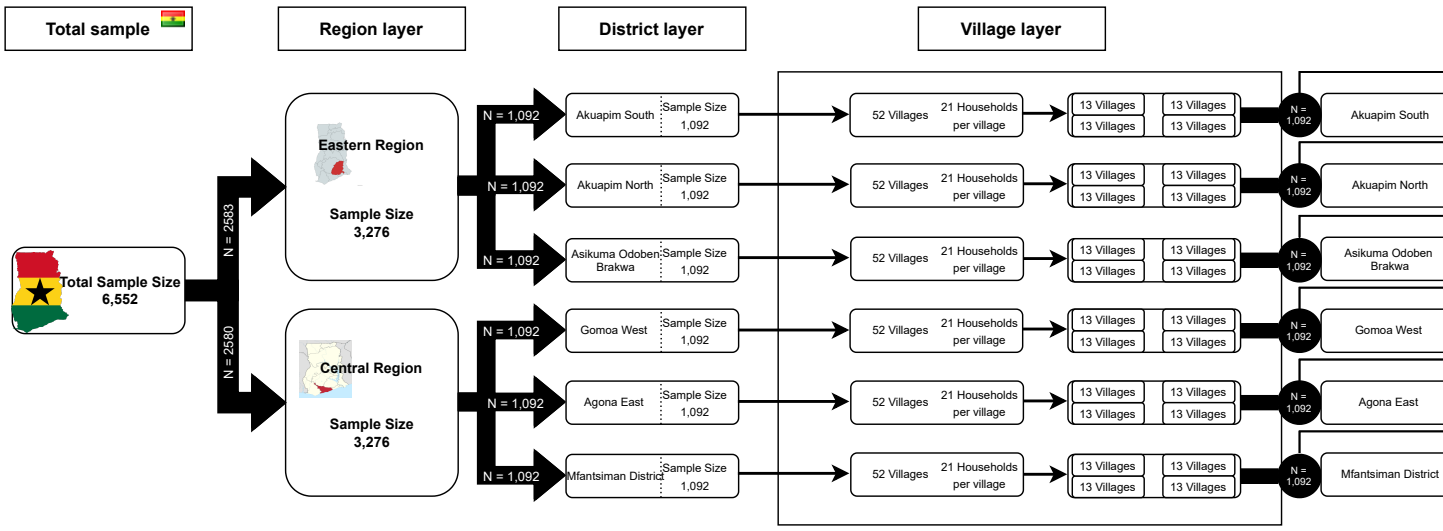

Figure 1: Experiment Flow Chart

- Akuapim South – Eastern Region
- Akuapim North – Eastern Region
- Asikuma Odoben Brakwa – Central Region
- Gomoa West – Central Region
- Agona East – Central Region
- Mfantsiman District – Central Region

Second sampling stage: Village selection

The second stage is village selection. The first step begins with identifying the list of villages in each District that can feasibly be enumerated by our enumeration team. Then, within each District, we generate a complete list of all of the villages that are candidates for enumeration. The villages are then ranked according to their population size (population statistics provide by the 2010 Ghana census). We then formed groups of four villages by putting four consecutive villages on these lists in the same quadruplet. In a typical district we would have approximately 50 quadruplets.

In each district we initially randomly select 13 quadruplets with probabilities weighted by the

quadruplet's share of the total population of the villages being considered in the district. This initial sample of quadruplets is then adjusted in consultation with the District Health officials. The adjusted selection criteria are in part driven by cost considerations – for budget reasons we are constrained to ensure reasonable travel distances between the four village clusters. We also need to ensure, in collaboration with our District Health officials, that the COVID-19 vaccines are readily available in the four village clusters selected. Within each of the chosen quadruplets, we randomly selected one village for the placebo treatment; one for the standard health video; one for the low cash incentive video; and one for the high cash incentive video. As Figure 2 indicates this will result in a total sample of 312 villages from the 6 different districts.

Third sampling stage: Household selection

A total of about 6,500 subjects are included in the experiment. Within each village, we will sample approximately 21 households and from each household a single individual will be treated. Each of the COVID-19 messaging treatment arms (Health, Low Cash and High Cash) will have a total sample size of approximately 1,300. The Placebo will have a total sample size of about 2,800. Figure 2 summarizes the distribution of sampled individuals across treatment conditions, villages and districts.

Within each village cluster team leaders using Google Earth images and input from local residents generate a rough map of the village cluster. They then choose 4 or 5 starting points, depending on the curvature of the community. The team leader allocates a number of starting points, spread out across each village. Enumerators starting from one point will interview respondents in the 3rd house, skipping two houses, in a particular direction. A coin toss after the first interview will decide if enumerators interview the next 3rd house to the left or to the right. Depending on the size of the village, the same or different enumeration teams will start from the different starting points following the same 3rd household interview sequence. This procedure ensures random selection within the village and avoids clustering. The GPS coordinates selected will also help to check for randomness.

Region	District	Village Random Asssign	Number Villages	Number of Subjects CDC Health Control	Low Cash	High Cash	Placebo Video	Subjects Total	
Eastern	Akuapim South								
		CDC Health Control	13	205	0	0	69	274	
		Low Cash	13	0	205	0	69	274	
		High Cash	13	0	0	205	69	274	
		Placebo	13	0	0	0	273	273	
		Total	52	205	205	205	480	1095	
Eastern	Akuapim North								
		CDC Health Control	13	205	0	0	69	274	
		Low Cash	13	0	205	0	69	274	
		High Cash	13	0	0	205	69	274	
		Placebo	13	0	0	0	273	273	
		Total	52	205	205	205	480	1095	
Eastern	Asikuma Odoben Brakwa								
		CDC Health Control	13	205	0	0	69	274	
		Low Cash	13	0	205	0	69	274	
		High Cash	13	0	0	205	69	274	
		Placebo	13	0	0	0	273	273	
		Total	52	205	205	205	480	1095	
Central	Gomoa West								
		CDC Health Control	13	205	0	0	69	274	
		Low Cash	13	0	205	0	69	274	
		High Cash	13	0	0	205	69	274	
		Placebo	13	0	0	0	273	273	
		Total	52	205	205	205	480	1095	
Central	Agona East								
		CDC Health Control	13	205	0	0	69	274	
		Low Cash	13	0	205	0	69	274	
		High Cash	13	0	0	205	69	274	
		Placebo	13	0	0	0	273	273	
		Total	52	205	205	205	480	1095	
Central	Mfantsiman District								
		CDC Health Control	13	205	0	0	69	274	
		Low Cash	13	0	205	0	69	274	
		High Cash	13	0	0	205	69	274	
		Placebo	13	0	0	0	273	273	
			52	205	205	205	480	1095	
							TOTAL SAMPLE		6570

Figure 2: Sample Frame

Within Household Selection

Enumerators are instructed to identify eligible respondents in the household selected. A household member would NOT be considered eligible for data collection under the following conditions:

- The household member has already received a COVID-19 vaccination.
- The household member does not agree to the COVID-19 protocol for the interview.
- The household member is unavailable for data collection within the feasible data collection days and hours specified by the enumerator.
- The household member refuses to participate.
- The household member refuses to agree to the consent form.
- The household member is under the age of 18.

If there are multiple members of the household who are eligible for participation, the enumerator would random select a participant using the following procedure:

- assign each eligible household member a number beginning with 1 for the first person; 2 for the second person; etc.
- enter the range of numbers into the random number generator on the tablet device and select the household member who has the number corresponding to the number generated by the random number generator.

Replacement When a household is ineligible for data collection or refuses to participate, data collectors move to the immediately subsequent accessible dwelling. That is, upon arriving at the 3rd household and asking for a non-vaccinated individual, and being turned away, the enumerator would not count another 3 houses. Instead they would move to the next closest household.

Moving to the immediately subsequent accessible dwelling entails:

- Among single-dwelling buildings, moving to the next dwelling in the direction the enumerator was instructed to walk (i.e., moving in the designated direction until reaching the next dwelling)

- In multi-dwelling buildings with 1 dwelling per floor, moving to the next level of the building (e.g., from the 2nd floor to the 3rd floor) ,
- In multi-dwelling buildings with multiple dwellings per floor, moving to the next apartment of the building (e.g., moving from apartment no. 6 to apartment no. 7).

Fourth stage: Household treatment assignment.

Within villages there are two treatment assignment protocols. For villages assigned to the Placebo treatment, all households receive exactly the same placebo video intervention and survey questions. For villages assigned to the other three treatment arms (Health, Low Cash, and High Cash), households are assigned to their appropriate treatment video with a probability of 0.75 and are assigned to the Placebo video with a probability of 0.25. These assignments to the Treatment and Placebo conditions are programmed within Qualtrics conditional on the Village ID – enumerators will not be aware ex-ante of which video treatment a respondent will receive.

Village Enumerators

Each village is assigned two enumerators – a female and a male enumerator. The two enumerators will both have tablets that are programmed to assign subjects to treatment arms with the probabilities noted above. Within villages that are assigned to one of the three treatment arms, subjects for both enumerators will be randomly assigned to the treatment with probability 0.75 and to the placebo with probability 0.25.

Power Calculations

The Vaccine Incentive Trial has a cluster design with treatments being assigned at the village level, and administered at the individual level to randomly selected individuals within each village. As indicated above, the implementation calls for the sampling of approximately 312 villages and the villages are randomly assigned to treatments taking into consideration village size. Within each village we sample approximately 20 individuals. There are four treatment arms with 78 clusters in each treatment arm. The total sample is about 6,500.

The power simulation parameters are based on a pilot study of 175 individuals that was conducted Dec. 21-23, 2021 in 8 village clusters located in the district of Gomoa West in the Central Region of Ghana. Village clusters in the pilot were assigned to three of the four treatments: Placebo, Low Cash and High Cash. This small sample provided some admittedly imprecise estimates of the parameters necessary for the power simulations: based on vaccine intentions (rather than verified vaccinations) we registered a High Cash treatment effect (relative to the Placebo) of 0.12 (a Cash mean of .88 compared to a Placebo mean of .76); the variance (standard deviation) of the residuals was 0.14 (0.37); and the estimated intra-class correlation was 0.02. In the case of each of the four treatment arms we expect to treat 78 clusters, i.e, villages. Within, each village we will treat 21 individuals.

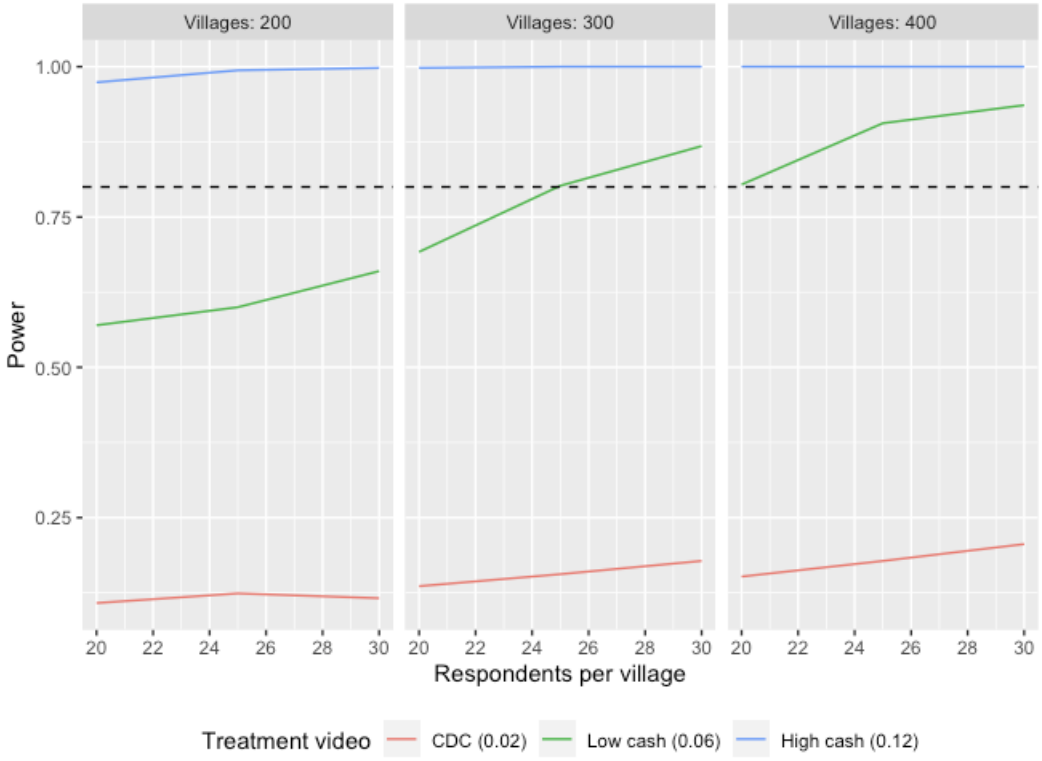

Figure 3: Power Simulations

We present here conservative power simulations for a basic hypothesis that estimates a High Cash, Low Cash and CDC Health treatment effect relative to the Placebo condition. Figure 3 summarizes the results of nine scenarios where we vary both the total number of villages (200, 300 and 400) and the number of subjects treated within each village (20, 25, and 30). The horizontal

blue line indicates the .80 power threshold. In Figure 3 we assume a Low Cash treatment effect of 0.06 (which is the treatment effect that we registered in an online experiment in the U.S. in which subjects responded to video primes about vaccine financial incentives (Duch et al., 2021)). The High Cash effect reflects the High Cash treatment effect registered in the Ghana pilot. We assume a CDC Health video effect size of 0.02. The ICC and the standard deviation of the residuals (i.e. the extent of the observed outcomes not explained by treatment) were set at the values observed in the pilot, 0.02 and 0.14 respectively. We will not be powered at 0.80 to detect an effect size of 0.02. On the other hand, assuming we include 300 villages in the design and at least 25 respondents per village we are powered to detect an effect size of 0.06. And for all of the simulations – villages ranging in number from 200 to 400 and as few as 20 treated subjects per village – we are powered at 0.80 to detect an effect size of 0.12. We therefore expect our design with four treatment arms, 312 villages and 21 treated subjects per village to be powered at approximately 0.80 to detect an effect size as small as 0.06.

Data Collection Stages

Figure 4 summarizes the phases of data collection that will make up the entire Ghana Vaccine Incentive trial. We expect data collection to begin on February 5, 2022 and to be completed by April 30, 2022.

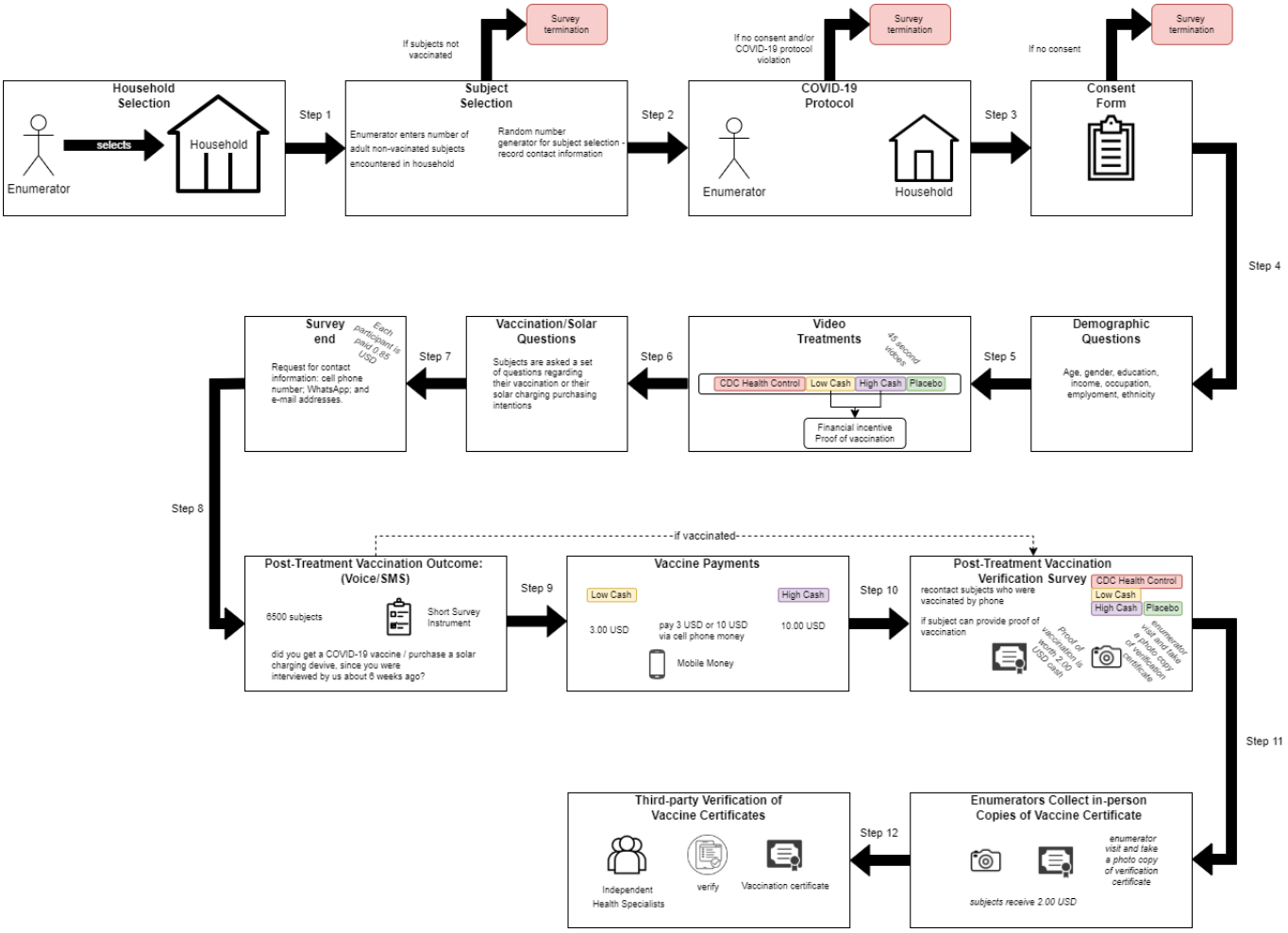

Figure 4: Flow Chart of Data Collection Phases

Step 1-Step 7: Treatment

The initial data collection and treatment phase of the experiment will begin on February 5, 2022 and continue through to approximately March 12, 2022. As described in the sampling discussion, villages are randomly assigned to four treatments: Placebo, Health, Low Cash and High Cash treatment arms. Within the Health, Low Cash and High Cash treatment arms, individuals receive the assigned treatment video with probability 0.75 and they receive the placebo with probability 0.25.

Enumerators are instructed to share the Survey Tablet with the subjects. Instructions and questions are read aloud to the subjects. The enumerators will enter the subjects' responses into the Tablet application. The phase one intervention consists of the following elements (note that the enumerator has already filtered out subjects who are vaccinated):

- Both subjects and enumerators are required to answer a set of questions to determine whether or not they have been exposed to the COVID-19 virus or show symptoms of the COVID-19 virus. If either is the case, then the interview does not take place.
- The subjects are informed about appropriate COVID-19 protocols including social distancing and wearing of masks. They must to consent to these protocols in order for the interview to take place.
- Subjects are presented with the purposes of the study and an informed consent form that they must agree to before proceeding to the survey.
- This is followed by a set of demographic questions including age, gender, education, income, occupation, employment status and ethnicity.
- Subjects are then presented with one of the short 45-second treatment videos.
- Subjects in the two Cash Incentive treatment arms are informed that they will be contacted in six weeks to confirm their vaccination status and that they will be sent their payment via cell phone money payment (they need to provide their cell-phone payment details at the end of the survey).

- Subjects are asked a set of questions regarding their vaccination intentions and their intentions to acquire a solar charging device.
- The survey ends with requests for contact information: cell phone number; WhatsApp; and e-mail addresses.
- Subjects are provided with a contact telephone/WhatsApp number plus e-mail address that they can contact if they have further questions.

All subjects are compensated for their participation in the experiment. Subjects earn 0.85 USD for agreeing to participate in the study. This is paid in cash after completing the first survey interview. We ask them for their permission to contact them at a later date, and inform them that they would be compensated again if they are contacted and complete the second interview.

The data collected during these visits are recorded on a Qualtrics App that is resident on each of the Tablets. At the end of the day, when the enumerators have internet access, these data are shared with the Qualtrics API and are registered on the Candour project Qualtrics account database.

Step 8: Post-Treatment Vaccination Outcome Telephone Survey

One outcome variable is whether subjects report having received a COVID-19 vaccine after the initial video intervention. We will begin measuring the outcome for all 6,500 subjects six-week after the video treatment. Measurement of vaccine status will initially be conducted by a short voice call survey to all participants' (cell phone numbers are collected as part of the initial intervention):

The voice survey will ask the following questions:

- Have you received a COVID-19 vaccine?
- If yes, date that you received a COVID-19 vaccine?
- Have you recently purchased a solar charging device?
- If, yes approximate date your purchased this device?
- Small number of post-treatment questions.

Step 9: Vaccine Payments

Subjects in the $3 and $10 treatment groups who report being vaccinated in Step 8 (Post-Treatment Vaccination Outcome Telephone Survey) would be sent their payment via cell phone payment. They would be informed at the end of the Step , Post-Treatment Vaccination Outcome Telephone Survey, that they would be receiving the payment.

Step 10: Post-Treatment Vaccination Verification Survey

One-week after the initial post-treatment survey, subjects indicating that they had been vaccinated would be re-contacted by phone call and asked to provide a copy of their vaccination certificate. They would be asked to arrange a time to have an enumerator visit them and take a photo copy of their verification certificate.

Step 11: Enumerators Collect in-person Copies of Vaccine Certificate

Enumerators will visit subjects in person. Enumerators will take a photo of the vaccine certificate and subjects would receive $2.00 in cash.

Step 12: Third-party Verification of Vaccine Certificates

A team of independent health specialists will be formed who are not associated with the research project. The independent team would verify the authenticity of each vaccine certificate and confirm its matched identity to the subjects in the study.

Data Collection Quality Assurance

The central research team will conduct regular data checks every day, outputting summary statistics for the most recent data, and regularly sharing them with the team. Quality checks include the following fields, and example reports:

- Numbers of surveys completed each day.
- Number of surveys by enumerator and community.

- Treatment assignment statistics to determine random assignment to treatment/placebo implemented properly.
- GPS tracking to verify random walk implemented properly.
- Survey length statistics, flagging any submitted survey that lasts less than 25 minutes.
- Check if any people required referrals, either to non-urgent medical care, or more urgent problems. For urgent problems we will reach out immediately to an NGO.
- Check contact numbers for formatting errors, and check for duplicates that might suggest problematic submissions.
- Descriptive statistics on how many contact numbers respondents give us, which will be used in follow-up surveys.
- Item non-response rate.
- Return intentions.
- Checking for logical inconsistencies in survey responses.

The principal investigators will be in Ghana to oversee the project. Ray Duch will directly supervise enumerator training, the survey pilot, and the beginning of data collection. Philip Clarke will directly supervise the beginning of the second phase of data collection which is the post-intervention survey conducted approximately six weeks after the initial intervention.

The University of Ghana team will conduct regular quality checks of submitted data and forward any concerns to us for our attention and discussion on a continual basis.

The University of Ghana team will oversee data collectors in field sites. There will be supervisors for the fieldworkers, who will escort them during all data collection and ensure that they follow the sampling technique as per the protocol.

Survey supervisor will send weekly progress reports to the research team. Progress reports include quality notes itemized by survey ID. Progress reports also include recruitment refusal rates with detailed statistics of recruitment characteristics.

The research team will build multiple quality checks into the survey instrument and check them regularly. (Early in data collection these built-in checks allow us to identify any enumerator who is

submitting problematic data. Such an enumerator would be taken off the project. All their surveys would be thoroughly checked for quality and we would manage to replace all their problematic surveys with new ones).

We will conduct continual confirmation of geographic sites of interviews, checking submitted interviews against designated research sites.

Analysis

Main Analysis

We will study whether cash incentives affect the intention to vaccinate (Intention) and actual vaccination uptake (Behavior). We model a binary outcome which is vaccination uptake (or intention to vaccinate) as a function of the four treatment conditions (with the Placebo condition as the reference category) using a random effects logistic regression model that accounts for clustering by villages. Our design allows us to do this first by regressing actual verified vaccination update on a set of treatment condition dummies.[1] The most basic specification is regressing vaccination outcome (*Behavior*) and vaccination intention (*Intention*) on the treatment status of the village cluster (ignoring individual treatment status):

$$\text{Behavior}_{ic} = \beta_0 + \beta_1 \text{Health}_c + \beta_2 \text{Low Cash}_c + \beta_3 \text{High Cash}_c + \omega \mathbf{X}_{ic} + \epsilon_{ic} \tag{1}$$

$$\text{Intention}_{ic} = \beta_0 + \beta_1 \text{Health}_c + \beta_2 \text{Low Cash}_c + \beta_3 \text{High Cash}_c + \omega \mathbf{X}_{ic} + \epsilon_{ic} \tag{2}$$

These estimated coefficients will indicate the overall impact on our two outcome measures of treating 75 percent of the village subjects with a treatment video and having 25 percent observe the placebo video. This estimated effect will incorporate any spillover that might affect the placebo control group.

We are interested though in estimating at the individual level the effect of receiving one of the treatment videos as opposed to just viewing the placebo video. Accordingly, we run the following

[1] As we indicated in the earlier section describing our hypotheses, we will test for an overall "Cash" Incentive treatment effect that will combine subjects in the Low and High cash treatments.

reduced-form regressions:

$$\text{Behavior}_{ic} = \beta_0 + \beta_1 \text{Health}_{ic} + \beta_2 \text{Low Cash}_{ic} + \beta_3 \text{High Cash}_{ic} + \omega \mathbf{X}_{ic} + \epsilon_{ic} \tag{3}$$

$$\text{Intention}_{ic} = \beta_0 + \beta_1 \text{Health}_{ic} + \beta_2 \text{Low Cash}_{ic} + \beta_3 \text{High Cash}_{ic} + \omega \mathbf{X}_{ic} + \epsilon_{ic} \tag{4}$$

All subjects will receive a short text SMS eight weeks after they have received the video treatment. This will allow us to measure "reported vaccine behavior". Misreporting (the estimated gap between *Behavior* and *Reported Behavior* may allow us to understand whether subjects are signalling norm-compliance as a function of the treatment. We are considering obtaining additional information in a post-treatment text survey about whether there is a change in injunctive and descriptive norms induced by the visit.

$$\text{Reported Behavior}_{ic} = \beta_0 + \beta_1 \text{Health}_{ic} + \beta_2 \text{Low Cash}_{ic} + \beta_3 \text{High Cash}_{ic} + \omega \mathbf{X}_{ic} + \epsilon_{ic} \tag{5}$$

where:

- Behavior_{ic} has a value of 1 if subject i in cluster c is vaccinated within the six-week period following the video intervention. Vaccination status is verified by the enumerators.
- $\text{Reported Behavior}_{ic}$ has a value of 1 if subject i in cluster c responds to the SMS text that they have been vaccinated within the six-week period following the video intervention.
- Intention_{ic} has a value of 1 if subject i in cluster c indicates, after they receive the video treatment, whether or not they intend to get vaccinated if the vaccine is available to them.
- Health_{ic} has a value of 1 if subject i in cluster c is treated with a standard CDC health message about COVID-19 vaccinations.
- Health_{c} has a value of 1 if cluster c is treated with a standard CDC health message about COVID-19 vaccinations.

- Low Cash$_{ic}$ has a value of 1 if subject i in cluster c is treated with the video offering a Low Cash incentive ($3.00) if the subject gets vaccinated in a 6-week period.
- Low Cash$_c$ has a value of 1 if cluster c is treated with the video offering a Low Cash incentive ($3.00) if the subject gets vaccinated in a 6-week period.
- High Cash$_{ic}$ has a value of 1 if subject i in cluster c is treated with the video offering a High Cash incentive ($10.00) if the subject gets vaccinated in a 6-week period.
- High Cash$_c$ has a value of 1 if cluster c is treated with the video offering a High Cash incentive ($10.00) if the subject gets vaccinated in a 6-week period.
- $\mathbf{X}_{ic}$ are a covariate controls.

Spillover Effects

Building on Duflo and Saez (2003) and more recently Giné and Mansuri (2018), we explicitly incorporate design features that will allow us to estimate the spillover effect of the video treatments. In order to estimate separately the effect of receiving the treatment video and that of just being in a village where some fellow village residents received the treatment video, we will run the following reduced-form regression:

$$\begin{aligned} \text{Behavior}_{ic} = \beta_0 + \alpha_1 \text{Health}_c + \alpha_2 \text{Low Cash}_c + \alpha_3 \text{High Cash}_c + \\ \beta_1 \text{Health}_{ic} + \beta_2 \text{Low Cash}_{ic} + \beta_3 \text{High Cash}_{ic} + \omega \mathbf{X} + \epsilon_{ic}. \end{aligned} \tag{6}$$

Health$_c$, Low Cash$_c$, and High Cash$_c$ indicate whether a given village c was assigned the each respective video treatment. Health$_{ic}$, Low Cash$_{ic}$, and High Cash$_{ic}$ indicate whether a given individual in village c was actually treated with the respective video treatments. This would allow us to estimate the average treatment effect for individuals in the three treated village (either Low Cash, High Cash or Health) and who are treated with the appropriate treatment video. The coefficients α_1, α_2 and α_3, would indicate the average treatment effect for subjects in these three types of treated villages who did not get the village-designated treatment but rather saw the placebo video.

There is another form of spillover that may affect the treatment effects of the intervention. The proximity of subjects in different treatment arms could affect their estimated treatment effect

(spillover to the experimental population). For example, if subjects in the High Cash video treatment are informed of a Low Cash treatment by neighboring subjects this might affect their vaccine uptake. Our conjecture is that this spillover will be correlated with the density of subjects in other treatment conditions, who are in close proximity. We collect the GPS coordinates for all 6,000 participants in the experiment. For each participant we identify the total number of other treated subjects who lie within a 10 kilometer radius. This results in three variables: *HighSpill*$_i$ which is, for subject i the sum of all subjects in the *High Cash* treatment who fall with a 10 kilometer radius of subject i. *LowSpill* and *HealthSpill* are similarly defined. We would then estimate the following equation:

$$\begin{aligned}
\text{Behavior}_{ic} = \beta_0 + \alpha_1 \text{Health}_c + \alpha_2 \text{Low Cash}_c + \alpha_3 \text{High Cash}_c + \\
\beta_1 \text{Health}_{ic} + \beta_2 \text{Low Cash}_{ic} + \beta_3 \text{High Cash}_{ic} + \\
\gamma_1 \text{HealthSpill}_{ic} * \text{High Cash}_{ic} + \gamma_2 \text{LowSpill}_{ic} * \text{High Cash}_{ic} + \\
\gamma_3 \text{HighSpill}_{ic} * \text{High Cash}_{ic} + \gamma_4 \text{HealthSpill}_{ic} * \text{Low Cash}_{ic} + \\
\gamma_5 \text{LowSpill}_{ic} * \text{Low Cash}_{ic} + \gamma_6 \text{HighSpill}_{ic} * \text{Low Cash}_{ic} + \\
\gamma_7 \text{HealthSpill}_{ic} * \text{Health}_{ic} + \gamma_8 \text{LowSpill}_{ic} * \text{Health}_{ic} + \\
\gamma_9 \text{HighSpill}_{ic} * \text{Health}_{ic} + \omega \mathbf{X} + \epsilon_{ic}.
\end{aligned} \tag{7}$$

The goal is a design that captures the full social impact of the vaccine financial incentives intervention. Our experimental design should provide clean estimates of treatment effects for trial subjects. The inclusion of the individual placebo treatments within treated villages should also provide an estimate of indirect spillover effects. Subject to funding we envisage a post-treatment data collection effort (surveying the non-experimental population) that would enhance our measurement of the indirect effects of the intervention. Building on the recent work of Fletcher and Marksteiner (2017), we anticipate refining our estimate of the social benefits of vaccine financial incentive by estimating the causal spousal spillover effect of our interventions. We have built into the pre-treatment data collection the possibility of conducting a post-treatment survey of a random sample of the spouses of the 6,000 subjects participating in the video interventions. We would measure validated vaccination uptake for this spousal sample.[2] The identification strategy would

[2]Another possibility here would be to sample all other household members 18-years and older. Verification of

exploit the placebo feature of the design that has been employed, for example, in get-out-the-vote experiment in which actual voting outcomes could be verified (Nickerson, 2008). Household spousal spillover treatment effects could be estimated by comparing vaccination rates of spouses in households assigned to treatment versus placebo conditions.

We are also considering the effect of the intervention on a more broadly defined non-experimental population. This would be contingent upon data availability and funding for additional data collection. We are in discussion with our partners at the District Health offices to obtain administrative data on vaccine uptakes by village. We may be able to obtain daily, time-stamped, vaccine rates by village that would allow us to assess the impact of our intervention on overall vaccine rates in treated and placebo villages. It would also allow us to assess any spillover effects in neigboring villages.

A second data collection in this regard could be conducted in conjunction with the post-treatment vaccine verification surveys that we will be conducting. We would select neighboring villages, to those that have been treated, and randomly select participants in order to ascertain verified vaccination history. We would also ask a battery of question designed to ascertain if they were aware of the treatments we administered in the neighboring village included in the initial intervention/sample.

Individual treatment effect heterogeneity

We will observe vaccination status at the individual-level. Therefore we have the opportunity to understand how our messaging interventions vary across segments of the population, by estimating how treatment effects vary across characteristics of our subjects and their locale. This analysis could, of course, have implications for enhancing the effectiveness of COVID-19 vaccination campaigns. Our sample of 6,000 subjects will include quite diverse demographic profiles; and the villages themselves will vary quite significantly in terms of size, socio-demographics and administrative features.

The covariate measures obtained in the survey include: gender, age, consumption expenditures, self-assessed financial situation, education, household size and number of children, and employment status.

household status would be more challenging than simply focusing on spouses.

We do not have strong priors as to precisely which covariates, either individual-level socio-demographic features or village-level aspects, will be the source for heterogeneous treatment effects. We will therefore exploit machine learning techniques to identify likely sources of heterogeneous treatment effects. We will use Bayesian Additive Regression Trees (BART), a non-parametric forest-based estimation strategy, to estimate the relationship between non-randomised features of our observations, subjects' treatment status, and vacciantion rates (Duch et al., 2020; Green and Kern, 2012; Hill, 2011). In a recent working paper (Robinson and Duch, 2021) we extended this BART strategy to allow us to identify covariate profiles that best partition individual-level treatment effects. We will adapt this strategy for this study by identifying which groups of individuals (defined by socio-demographic and geographic features) for which the treatment interventions are most (and least) effective. This analysis will allow us to propose optimal targeting strategies for identifiable segments of the African population.

Attrition

We anticipate some attrition in the sample of 6,000 rural Ghana participants. Although attrition should not be a serious issue given there is only a six-week period separating the initial and the follow-up contact with participants. We are planning to collect a reasonably complete set of demographic measures in the initial survey. Diagnostic comparing the demographic profiles of subjects assigned to the different treatment arms will signal the likelihood that missingness is not independent of treatment status. To the extent that we observe any significant attrition in the sample and evidence that it might be correlated with treatment status, we will implement estimation strategies designed to address any bias that might be associated with this attrition.

Inverse probability weighting would be one of the estimation strategies we would implement. It has the value of being very straight-forward – essentially modeling the attrition process as a function of observable covariates (Anderson et al., 2021). The weights are based on the predicted values from a logistic regression of a binary variable indicating whether the observation is missing on the available covariates, where all the available covariates are allowed to interact with the treatment indicators.[3] The weight is simply 1 over 1 minus these predicted probabilities.[4] We would then

[3] An alternative here, that we would consider using, is a propensity score estimation algorithm to fully model any possible nonlinearities – the *twang* package, for example (Ridgeway et al., 2021).

[4] These weights are often characterized as "unstable" – a slightly modified estimation strategy can generate more

re-estimate treatment effects on the we subset of the data where outcomes are observed and weight that estimate using these weights. The estimate from this regression is a consistent estimate for the treatment effect assuming the censoring process is observable. An alternative estimation strategy that we will consider is the bounding estimator proposed by (Lee, 2009). He assumes that attrition is monotonic, which in our setting, implies that any subject who would not be missing if assigned to the control group would also not be missing if assigned to one of the treatment arms.

Ethics

The experiment is conducted according to the University of Oxford's policy for human subjects research. The experiment was approved by the University of Oxford Social Sciences Department of Economics Research Ethics Committee (DREC) with reference ECONCIA21-22-28. Informed consent is obtained from each participant at the beginning of the survey.

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