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

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December 27, 2021

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Motavation

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. 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 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;
- H2: Subjects offered financial incentives in the 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;
- 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;

A secondary hypothesis is that:

- H4: 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:

- H5: 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.

- H6: 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 asssigned 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 placebo video that provides general information about the benefit of using solar power to charge household electrical appliances.

- Treatment 2: Standard CDC COVID-19 vaccine promotional and information video.

- Treatment 3: Low Cash Incentive treatment – the first 30 seconds are identical to the CDC video – the last 15 seconds inform viewers that that they will earn $3 if they provide proof of a COVID-19 vaccine within the next 6 weeks.
- Treatment 4: High Cash Incentive treatment – the first 30 seconds are identical to the CDC video – the last 15 seconds inform viewers that that they will earn $10 if they provide proof of a COVID-19 vaccine within the next 6 weeks.

The four treatment videos are available at
`https://drive.google.com/drive/folders/1-2csw9UyC2gQIljbFcFGuYF4QuTaa75R`.

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 6,552. 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. Our sample consists of the Eastern and Central regions. 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.

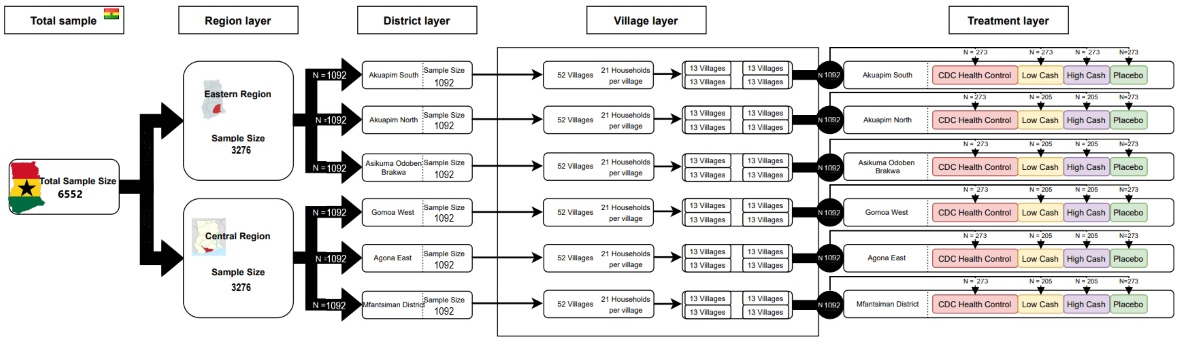

Figure 1: Experiment Flow Chart

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:

- 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 100 quadruplets. In

each district we will select 13 quadruplets in consultation with the District Health officials. The 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 these 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.

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

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, on a Google map print-out, team leaders define a number of viable starting points. The team leader allocates a number of starting points, spread out across each village. We first draw a rough map of the community and choose 4 or 5 starting points, depending on the curvature of the community. 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.

A household would be considered ineligible for data collection under the following conditions:

- The household member responding has already received a COVID-19 vaccination.
- The household member is not available for data collection within any of the feasible data collection days and hours.
- The household member refuses to participate.
- The household member is under the age of 18.

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.

Power Calculations

The Vaccine Incentive Trial has a cluster design with treatments being assigned to randomly selected individuals at the village level. 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. Treatments are assigned at the village level but administered at the individual level.

The power simulations 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 cluster 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) 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.

We present here the power simulations for the basic hypothesis (H1) noted above – a High Cash treatment effect relative to the Placebo condition. Figure 3 summarizes the results of four different simulations assuming two treatment arms of 78 clusters each: the High Cash and the Placebo. The horizontal red line indicates the .80 power threshold. In the A frame of Figure 3 we assume a 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 ICC and the standard deviation of the outcome variable is estimated to be the values, respectively that we observed in the pilot, 0.02 and 0.36. In the B frame of Figure 3 we have a more conservative estimate of the ICC, 0.10, which reduces our power for the treatment effect of 0.06. Nevertheless we are still, just, powered at 0.80 with our combined two-treatment arm sample of 3,000 respondents. The C and D frames of Figure 3 present the identical power simulations for a treatment effect size of 0.10. In this case there is no question that we will be sufficiently powered with the planned 1,500 respondents per arm. And note that this effect size of 0.10 is the one suggested by our initial pilot results.

Data Collection Stages

Treatment

The initial data collection and treatment phase of the experiment will begin on December 16, 2021 and continue through to January 15, 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. There are four components to the phase one intervention (note that the

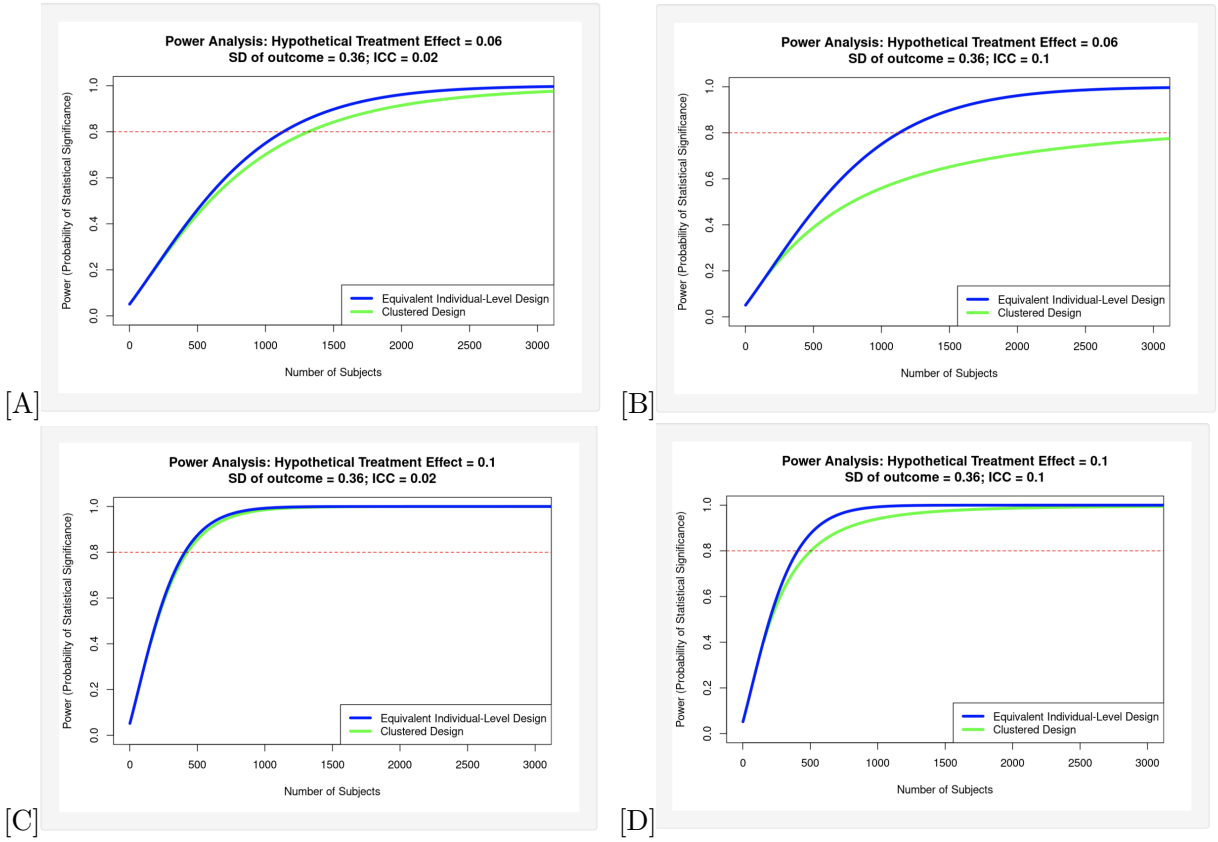

Figure 3: Power Simulations Based on Ghana Incentive Pilot Study Dec. 21-23, 2021

enumerator has already filtered out subjects who are vaccinated):

- 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. Included here will be contact information: cell phone number; WhatsApp; and e-mail addresses.
- Subjects are then presented with one of the short 45-second treatment videos.
- Subjects in the two Cash Incentive treatment arms are informed about how and when they can collect their financial incentives upon proof of vaccination status.
- Finally subjects are asked a set of questions regarding their vaccination intentions.

All subjects are compensated for their participation in the experiment. Subjects earn 1 USD for agreeing to participate in the study. This is paid in cash after the first intervention. 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.

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.

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.

Post-Treatment Vaccination Outcome: Text Message Survey

The outcome variable is whether subjects received a COVID-19 vaccine after the initial video intervention. We will measure the outcome six weeks after the initial video intervention. Measurement of vaccine status will initially be conducted by a short text message survey sent to all participants' cell phone numbers (that are collected as part of the initial intervention).

The text message survey will ask the following questions:

- Have you received a COVID-19 vaccine?
- Date that you received a COVID-19 vaccine?
- If you have received a COVID-19 vaccine;
  - If it's possible, would you please send a copy of your vaccine certificate to the Candour team via e-mail or WhatsApp? We will compensate you with an additional 50 cents cell phone money credit.

Post-Treatment Vaccination Outcome: Text Message Survey

Enumerators will re-contact the following subjects to confirm their COVID-19 vaccine status:

- Subjects who were not succesfully contacted via text messages – either because they did not respond or did not have a functioning cell phone number;
- Subjects who indicated they were vaccinated but did not send their vaccine certificate via e-mail or WhatsApp.

The re-contacting by enumerators would only involve the verification of the subject's vaccination status.

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 region.
- 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.
- Enumerators have a question at the end of the survey about who else was present, including one option indicating if authorities have been. If authorities have been present, we would stop data collection in that location.

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 survey firm's senior manager will conduct regular quality checks of submitted data and forward any concerns to us for our attention and discussion on a continual basis.

Field team managers, all long-time management at the survey firm, will oversee data collectors in field sites. There will be one supervisor for every four fieldworkers, who will escort them during all data collection and ensure that they follow the sampling technique as per the protocol.

Survey firm 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 compare vaccination uptake in each of the three treatment conditions to the uptake in the Placebo condition using GLS regression.[1] Our design allows us to do this first by regressing actual verified vaccination update on a set of treatment condition dummies. 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):

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

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

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 separately the effect of receiving one of the treatment videos as opposed to just viewing the placebo video. Accordingly, we run the following reduced-form

[1] A number of estimation strategies can be considered here including logit regression. We will implement GLS with random effects for clusters (Green and Vavreck, 2008)

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 six 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:

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

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 actually got 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. We would also have the average treatment effect for subjects in these three types of treated villages who did not get appropriate 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 ef-

fect (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. We collect the GPS coordinates for all 6,000 participants in the experiment. To measure this contagion effect of other treated subjects, we include a variable in the estimation for each of the three treatment arms: *HighSpill*, *LowSpill* and *HealthSpill*. *HighSpill* assumes a value of 1 if the respondent is NOT in the High Cash treatment and a 0 if they are in the treatment. It is multiplied by the average distance that this individual is from all other High Cash treatment subjects in their district (recall that we are sampling 1,000 subjects from six different districts in two regions of Ghana). *LowSpill* and *HealthSpill* are similarly defined. We would then estimate the following equation:

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

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 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 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.

[2]Another possibility here would be to sample all other household members 18-years and older. Verification of household status would be more challenging than simply focusing on spouses.

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.

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.

Ethics

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

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