Last-Mile Delivery Increases Vaccine Uptake in Sierra Leone

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Abstract

Less than 30% of Africans received a dose of the COVID-19 vaccine even 18 months after vaccine development. Motivated by the observation that residents of remote, rural areas of Sierra Leone faced severe access difficulties, we conducted an intervention with last-mile delivery of doses and health professionals to the most inaccessible areas, along with community mobilization. A cluster randomized controlled trial in 150 communities shows that this intervention with mobile vaccination teams increases the vaccination rate by about 26 percentage points within just 48-72 hours. Moreover, auxiliary populations visited our community vaccination points, more than doubling the number of shots administered. The additional people vaccinated per intervention site translates to an implementation cost of US$ 33 per person vaccinated. Transportation to reach remote villages accounts for a large share of total intervention costs, so bundling multiple maternal and child health interventions on the same trip would lower costs *per person treated* even further. Current scholarship on vaccine delivery maintains a large focus on individual behavioral issues like hesitancy, but this research demonstrates that prioritizing mobile services to overcome access difficulties faced by remote populations in developing countries can generate larger returns in terms of uptake of health services.

1. Introduction

By March 10th, 2022, over a year after COVID-19 vaccines arrived on the market, 80% of the populations living in high-income countries had received at least one dose compared to only 15% of the people in low-income countries.[1] As of April 23, 2023, only 37% of the population of Africa has received at least the first dose of COVID-19 vaccine.[2] Low rates of vaccination keep many African countries vulnerable to the threat of disease recurrence, and a renewed possibility of costly lockdowns capable of undermining employment, income generation, and food security.[3] Low vaccination coverage also raises the hazard of new sub-variants emerging that puts the entire globe at risk.[4]

To understand why vaccination rates remain low, we assembled data on vaccination beliefs, hesitancy, and access from several countries in late 2021.[5] Nationally representative data from Sierra Leone revealed that getting access to a COVID-19 vaccine required the average Sierra Leonean to travel three and a half hours *each way* to the nearest vaccination center, at a cost that exceeds one week of wages.[6] This motivated the design of an intervention we implemented in March/April 2022 in partnership with the Sierra Leone Ministry of Health and Sanitation (MoHS) and the international NGO Concern Worldwide. The centerpiece of this intervention was to simply take vaccine doses and nurses to administer vaccines to remote, rural communities, preceded by seeking permission and community mobilization. A cluster randomized controlled trial (RCT) across 150 communities shows that the vaccination rate in treatment villages rises by about 26 percentage points in response to this intervention. In addition, large numbers from neighboring communities also show up to receive vaccines at our temporary clinics. In treatment villages, the average number of people vaccinated per community increases from about 9 people pre-intervention to 55 people within the intervention period of about 2-3 days, at a cost of US$33 per person vaccinated.

These results suggest that low vaccination rates are related to deficiencies in access, and that a cost-effective intervention is capable of overcoming that deficiency. Sierra Leone MoHS operates a network of "peripheral health units" (PHUs), but a significant proportion of Sierra Leoneans - particularly those in inaccessible rural areas - live outside the 5-kilometer catchment area of any PHU. That necessitates interventions like the one we conducted in communities outside PHU catchment areas, to ease the burden of access.

This result carries broader implications for global public health. The child mortality rate (CMR) in Sierra Leone was 10.5% in 2021[7] as many children die from preventable diseases that immunizations and other simple interventions could address. The situation is almost as dire in neighboring Guinea and Liberia. In contrast, efforts at community engagement in Bangladesh, including simple acts of taking maternal and child health interventions to rural populations, contributed to increasing the infant vaccination rate from 1% in the early 1980s to more than 70% within 10 years.[8] Remote West African populations have proved more challenging to reach, but our intervention serves as a proof of concept that it may be

similarly possible to tackle the high rates of child mortality in West Africa by cost-effectively delivering simple health interventions to rural populations. In fact, bundling multiple health interventions together would allow us to lower the cost of delivery *per person treated*, given the high fixed transportation costs of reaching each remote community.

These results are relevant for donors and international pharmaceutical companies, who have cited cases of unused vaccines reaching the expiration dates in Africa[9] to explain why poor countries did not receive adequate supplies of vaccine doses early in the pandemic.[10,11] Our implementation efforts taught us that Sierra Leone MoHS needed to engage in "learning by doing" to develop novel distribution systems to reach remote populations with those doses. But it is a catch-22: the required experimentation is only possible once a steady and dependable supply of vaccine doses is made available.

To benchmark our results against other vaccination strategies, we conduct a comprehensive systematic review that identifies 234 unique interventions in 144 RCT studies that use information, nudges, community engagement, social signaling, non-financial and financial incentives to increase vaccination rates across many settings around the world. Over a third of these interventions produce null effects. Our access intervention produces a larger percentage point effect size than 223 (95%) of the treatments reviewed. This is not surprising, since vaccinating the first 50% of the population in remote parts of low-income countries requires solving the fundamental problem of access, which we address. Once access issues are addressed, misinformation and hesitancy may loom large in the effort to vaccinate the last 20% of the population of high-income countries who stubbornly hold out, and this is the target of the bulk of the literature. Even in high-income settings, access constraints were relevant in the earliest phase of COVID vaccine delivery.[12]

This implies that we may need to further emphasize access interventions if we are to increase the global vaccination rate and improve vaccine equity. CDC and WHO guidelines highlight the importance of "bringing services closer to the people", and our RCT is a proof of concept that such approaches can increase vaccination rates quickly and cost-effectively, even under difficult circumstances in the most remote communities. The mobile delivery concept has produced large effects on HIV testing,[13] but rigorously demonstrating effectiveness in vaccine delivery is critical, given the persistent low rates of vaccination in low-income countries. Our systematic review revealed thousands of studies on vaccine hesitancy and misinformation but only a handful on vaccine supply and access, with a clear bias in favor of high-income contexts. This imbalance is emblematic of a wider debate on the relative importance of individual-specific behavioral factors versus systemic deficiencies, in limiting the diffusion of welfare-improving technologies among poor populations.[14] Prominent behavioral scientists have recently acknowledged our excessive focus on individual behavioral peculiarities ("i-frame") at the expense of systemic solutions ("s-frame").[15]

2. Context and Research Design

We conducted a pre-registered cluster randomized controlled trial in 150 rural villages in Sierra Leone. We first mapped all PHUs where the Ministry of Health and Sanitation (MoHS) was offering COVID-19 vaccines, plus the "catchment areas" of a PHU defined by MoHS as the 5-mile radius around each PHU. We then compiled a list of all communities situated *outside* these catchment areas, and randomly selected 150 communities from this list. 100 communities were randomly assigned to receive the intervention and the other 50 were assigned to the control group. During March and April 2022, a research team first visited all communities to conduct a village population listing and a baseline survey. Immediately afterwards, mobile vaccination teams coordinated by MoHS visited the 100 treated villages for two to three days per village (see Figure A1 in Supplementary Information Section 1).

On the first day of intervention, a "social mobilization team" – trained and supervised by MoHS – organized a conversation with all village leaders, including the Town Chief, Mammy Queen, Town Elders, the Youth Leaders and Religious Leaders, and any other important stakeholders including the Paramount and Section Chiefs if they were available (Step 1 in Figure 1). The social mobilizers we employed were previously vetted and trained by ministry staff, and commonly engaged for short-term projects like vaccination campaigns. This cadre is referred to as "MoHS volunteers" because they are paid per-diems against project work, and not a regular civil servant salary. The mobilizer team explained the purpose of the visit, answered questions about the available vaccines, and asked leaders for their cooperation in encouraging eligible community members to take the COVID-19 vaccine.

Social mobilizers then asked leaders to convene a community meeting that same evening (when people return home from farms) to allow mobilizers to talk directly with all village residents about vaccine efficacy and safety, the importance of getting vaccinated, and to address villagers' questions and concerns. This "Step 2" ended with social mobilizers explaining the location and timing of the mobile vaccination site that they were about to set up.

Vaccine doses, nurses to administer vaccines, and MoHS staff who could register the vaccinated were brought into the community either the same evening or early the next morning (Step 3 in Figure 1). The vaccine doses and staff often traveled in on motorbikes or on boats, given the difficult terrain they had to traverse to reach these remote communities. Once the team was in place, the temporary vaccination site started operating in a central location in the village (Step 4 in Figure 1). Villages in our sample were small with houses closely clustered, so walking distances to the vaccination site were small. The vaccination site remained operational from sunrise to sunset over the next two days, allowing people to visit when convenient. Nurses and registration staff remained stationed at the temporary clinic, while the mobilizers continued to provide vaccine information to various community members (Step 5).

We randomized the exact nature of these additional Step 5 mobilization activities. Half the treatment villages were randomized into an individualized "Door-to-Door" campaign (Step 5A), where social mobilizers went to 20 randomly selected structures to privately discuss any concerns about that vaccine that the household residents had, and to encourage them to visit the vaccination site. The other 50 treatment communities were randomized into "Small-Group" outreach (Step 5B), where mobilizers targeted social groups who gathered at fixed spots in and around the villages (e.g., groups of farmers in fields, mosque attendees, women collecting water). Social mobilizers engaged the group to have joint conversations about the vaccines. There was equipoise about whether individualized or small-group outreach would be more successful in persuading people to get vaccinated, so we tested both strategies.

3. Effects on COVID-19 Vaccination Rate

Our primary outcome is "verified" vaccine uptake, measured using a respondent-level question on whether the person took a COVID-19 vaccine of any type, checked against their vaccination card (if consented). This provides us with a site level count of vaccine doses administered.

To calculate a village level vaccination *rate*, we had to first enumerate the population in all 150 treatment and control villages. Such community census lists typically do not exist in Sierra Leone. Our research team therefore walked to all structures in every village to tally the number of households (39 on average, SD = 23), and the number of individuals living in those households (29,587 individuals across the 150 villages, or about 197 people per village).

The population of these villages was on average 22.3 years old, 26.5% of households were female-headed, 64.5% of people lived in a household of 6 or fewer people. Only 20.1% lived in a household where the household head had any form of formal schooling, and about 86.1% lived in a household where the head was primarily engaged in farming. Respondent characteristics are well balanced across the treatment arms (see Table 2), except for the baseline vaccination rate, proportion of households employed in agriculture, proportion of households that own a radio, and the proportion of women breastfeeding and the proportion that owns land. While an overall F-test does not reject the equality of means across the full set of outcomes, we add these covariates in part of our analysis below.

Figure 2 shows that at baseline the average vaccination rate in control villages was 6.2%, compared to 9.5% in treatment villages (difference 0.03, p = 0.015). Post intervention, the vaccination rate increased to 30.2% in treatment villages. We report effects from linear regression specifications of the Intent-To-Treat effect with randomization fixed effects and heteroscedasticity-robust standard errors clustered at the village level in the Extended Data,

Table 3. Section 10 provides full methodological details. The Intent-To-Treat effect is 26 percentage points (standard error = 0.018, $p < 0.01$). The results remain qualitatively similar (25 percentage points) when covariates for respondent characteristics are added that were imbalanced at baseline (ao. vaccination status), or when we aggregate the data up to the village level (28 percentage points).

This increase in the vaccination rate is an underestimate of the total number of vaccines administered over those 2-3 days as it does not include vaccines given to migrant returnees and others from nearby villages. The average uptake also masks considerable heterogeneity between villages. In 2 out of the 100 treatment villages there was zero increase in vaccinations because the village authorities either dissuaded villagers from getting vaccinated, or refused permission for the intervention to take place, causing the intervention to essentially fail in Step 1 depicted in Figure 1. On the other hand, the full distribution of vaccination rates displayed in Figure A2 in Supplementary Information Section 1 shows that in five villages, over 50% of adults enumerated in the community census were vaccinated during the course of our intervention. A similar large degree of variation is evident from the total count of shots set per village (see Figure A2 in Supplementary Information Section 1).

4. Effects on Total Vaccination Count

Many of the people who attended our temporary clinics to receive a vaccine were not enumerated during the community census. These additional people fall into one of three categories: residents of other nearby villages (who heard about the clinic and were interested to take advantage of the easy access to a vaccine); recent migrant returnees who were not present during the village listing; and others – like high-frequency commuters – not captured in the census. For these auxiliary populations, we do not have a denominator and can thus not estimate a vaccination *rate*. We can however, provide results on vaccination counts.

At baseline there were on average about five people vaccinated in control villages, and about nine people in treatment villages (difference 3.45, $p < 0.056$). Figure 3 shows that after the intervention was implemented over the following 2-3 days, the number of vaccinated individuals increases to about 55 people on average per treatment site, a sixfold increase. This is the full impact of our mobile vaccination drive. Amongst individuals vaccinated who were not enumerated in the census, 53% (12-13 people per treatment community) were visitors who came in from nearby villages to get vaccinated, whilst the remaining 47% (11-12 people) included short-term, circular commuters or migrant returnees who were not present on the day of the census and could not be matched to our listing records, as well as individuals whose "community of origin" was unknown. The Intent-To-Treat regression estimates with heteroscedasticity-robust standard errors and additional covariates are in-

cluded in Extended Data Table 4. In total the teams vaccinated 4,771 people aged 12 or above. Of these 39% received a Johnson & Johnson vaccine, 29% Pfizer, 17% Sinofarm and 16% received AstraZeneca. A variety of vaccine types were administered because there was no steady supply of any specific type of vaccine dose in Sierra Leone when this intervention was conducted, so we had to make use of the vaccines available in the Ministry of Health stocks in any given week.

5. Effects of Home Visit

Both types of mobilisation activities implemented in Step 5 had similar effects on the vaccination rate. The evidence on whether the "Door-to-Door" or "Small Group" activities were more effective is mixed. When we compare across communities, we find that the Door-to-Door program increased the adult vaccination rate by about 29%p compared to 23%p in villages assigned to the Small Group mobilisation activities (difference, p=0.015), see Column 1 in Extended Data Table 1). However, when we study individual households randomly assigned to a visit against those who are not *within* "Door-to-Door" villages, we do not detect any differential uptake. In these 50 villages, up to 20 randomly selected structures were visited for a private or semi-private conversation with residents about the vaccine and to encourage them to visit the temporary clinic. The random selection of structures allows us to report experimental results on the effects of receiving this extra nudge on the propensity to receive a vaccine. We interpret this activity as a "demand-side treatment", in that the visit and conversation gives that resident an opportunity to discuss their concerns or questions about vaccines in private, which could be useful to overcome potential hesitancy. Extended Data Table 1, Column 3 shows that this extra effort did not generate additional demand beyond the effect of our "supply side" activities to enhance vaccine access. The adult vaccination rate at the end of the vaccination program among those who received the home visit by mobilizers was 27 percentage points, and those who did not receive the extra nudge had a rate of 28 percentage points. Social mobilisers received extensive training and close supervision, but the lack of impact from this additional demand-generating activity may reflect low effort by social mobilisers. Within-village spillovers may also dampen these individual treatment effects. Unfortunately we lack data on distances and other channels of interactions between households to test this formally. However, this type of spillover may be small due to the relatively short time interval between the home visits and the vaccine drive.

We do not have an equivalent analysis of the individual effect of the Small Group treatment since that was not randomized within villages, and the enumerators were not able to exactly track which households participated in the Small Group sessions.

6. Mechanisms

While our vaccine access intervention significantly raises the vaccination rate, it is also clear that we remain far short of reaching the WHO goal of near-universal uptake. We collected individual-level data in all treatment villages after the intervention from both vaccine takers and non-takers. These data can shed some light on why and how our access intervention was more or less successful for types of certain people.

Meeting Attendance: "Step 2" of our intervention (see Figure 1) was to organize a community-wide meeting to inform all village residents about the vaccine clinic. The field team registered which community members attended that meeting, and overall, 41% of households participated in these meetings. 44% of those who chose to attend the meeting subsequently chose to get vaccinated. One cannot impose any causal interpretation to this correlation: people who were already interested in getting vaccinated may have been the ones who chose to attend the meeting.

We can make a slightly stronger inference by examining the subset of people who stated in our baseline survey that they were *unwilling* to get a vaccine (see Extended Data Table 5). Within this sub-group, 53.8% of those who attended meetings ultimately took the vaccine, while the vaccination rate was only 14.4% amongst those who did not attend. Even within the converse subgroup (those who stated at baseline that they *were* willing to take the vaccine), meeting attendance was strongly predictive of subsequent vaccine uptake: 64.6% vaccination rate among attendees and 39.4% among non-attendees.

These are not causal, but the strength and direction of these correlations suggest that the information shared in the meeting, and the answers that were provided to the community's questions, are unlikely to have dissuaded people from getting vaccinated. These correlations – combined with our team's on-field experience – suggest that holding these meetings was helpful and form a necessary part of any access intervention. Encouraging greater attendance in meetings in any future replications would probably be a good idea.

Vaccination Knowledge and Trust: We also collected data on another intermediate outcome in a subset of villages: people's knowledge and attitudes regarding the COVID-19 vaccine. Figure 4 shows that the treatment improved people's knowledge about and trust in vaccines: an increase of 0.11 points in people who know about the vaccine, and an increase of 0.13 points ($p<0.1$) in the 5-point Likert scale about trust in the safety of vaccines. The change in trust implies that our intervention was not solely about improving access: the community interactions and the information we shared were also relevant parts of the intervention package. People's beliefs about vaccine efficacy did not change due to treatment (the magnitude is 0.097 points with $p=0.183$). The treatments do not change what source people trust the most for receiving health information. Extended Data Tables

6 and 7 provide the associated regression estimates. Note that as this is an exploratory exercise where we test treatment effects across several outcomes, the Tables report the FDR adjusted q-values accordingly.

Heterogeneity Across Demographic Groups: Figure 5 shows the differences in treatment effect for specific demographic sub-groups. Extended Data Table 8 provides associated regression results. The treatment effect is 7 percentage points larger for men than for women ($p<0.0001$). Treatment increased the adult female vaccination rate by 23.1 percentage points, and 30.1 percentage points for men. The treatment effect is 18.1 percentage points for the 18-24 age group and rises to 27.4 points for the 25-54 age group ($p < 0.001$), and 31.6 points for those aged 55 and above ($p < 0.08$). There is no difference in treatment effects across education, land ownership, or food security status.

7. Discussion

Comparison to Other Vaccination Efforts. As shown in Figure 2, our simple intervention to solve last-mile challenges in vaccine delivery triples vaccination rates within 48-72 hours, and vaccination counts increase by over 250% (Figure 3). While such percentage increases appear dramatic, this is the gain off a very low base rate: just 6-9% were vaccinated at baseline. Another relevant benchmark is our *percentage point* effect size, and how that compares to other vaccination campaigns evaluated in the literature.

We conduct a systematic review of all vaccination strategies that have been evaluated via randomized controlled trials. The methods section provides inclusion criteria for this review. We identified 144 different published RCT studies that report the results of 234 unique interventions.

These interventions varied across multiple dimensions, spanning time, space, and strategy, often as part of the same study with multiple components. For clarity and brevity, we identified five major intervention "families", which could be further fragmented into more granular intervention "types". The "families" into which interventions were sorted were: Education; Community Actions; Communications; Incentives; and Healthcare Improvement and Worker Training. Amongst the 144 relevant studies, only three focused on the essential theme of vaccine *access* in a low-income context, none of them centered on COVID-19 vaccines.[16,17,18]

Figure 6 demonstrates the immense heterogeneity of effect sizes across those 234 different treatments. Of all treatments reviewed, 35% had no statistically significant effect on vaccine uptake. Perhaps unsurprisingly, due to the variety of incentive types and sizes, the "incentives" group is strongly positively skewed, accounting for five of the top ten effect sizes overall. However, the highest median effect size is found amongst educational

interventions.[19,20]

The intervention we conducted in Sierra Leone – where mobile health teams visit remote communities for 48-72 hours to ease their access burden – produces a larger percentage point effect size than 223 (95%) of the treatments reviewed.

Table A1 in Supplementary Information Section 2 provides the details of the intervention approach used in each study. The vast majority of these studies were conducted in high income settings. Many of the vaccination campaigns evaluated are nudges and reminders via text messages, telephones or mailings. Nudges are cheap, but often produce very small or null effects. Other strategies involve visiting parents to educate them about the benefits of childhood immunization, or sending community health workers. Yet others offer direct financial incentives against a verified vaccination.

Of special interest are recent studies that attempted to promote COVID-19 vaccinations. A study in Sweden[21] offered monetary rewards of US$24 to get a COVID-19 vaccine, and this increases the vaccination rate by an extra 4 percentage points, from 72% to 76%. A financial incentive of US$10-50 combined with other nudges in the United States did not produce any effect.[22] City and state-wide lotteries offering financial rewards in the United States[23,24] produced very small or negative effects. Text-based reminders in the U.S.[25,26] and defaulting people into a vaccination appointment in Italy (so that they are forced to opt out)[27] increase vaccination rates between 0 and 3.5 percentage points.

Cost-effectiveness Relative to Other Strategies. Sending text message reminders or running city-wide lotteries are relatively cheap to implement while delivering vaccines in remote areas is costly. It is therefore useful to compare not just percentage point effect sizes, but also the cost of administering various programs *per vaccinated individual*. Moreover, we chose to work in the most remote areas not covered by the Sierra Leone MoHS vaccination programs, precisely because they are too far away even from Peripheral Health Units. We collected detailed cost data on our program to compute this metric, and compare it to other studies that provide such cost information.

The total costs of our intervention to reach 100 villages was US$ 156,023.5, or approximately US$ 1,560 per village. This includes all travel, administration and management and supervision costs, but excludes the cost of the vaccine doses, which were provided to Sierra Leone by the COVAX program for free. This translates to a cost per dose administered of about US$ 33.

Appendix Table 9 provides a detailed breakdown of the fixed and variable components of our implementation costs. Of the US$ 33, around 27% (US$ 9) were fixed costs of training project staff, 73% (US$ 23) were variable costs. The most expensive category (38% or US$ 12.50) is transportation to these remote villages, which includes the cost of renting vehicles and fuel. Salaries and subsistence allowances for the social mobilization and vaccination teams account for another quarter of the total costs.

To conduct this intervention again at larger scale, the variable costs would need to be repeated, but not the fixed costs of training. At scale, the cost of this intervention would thus approach about US$23 per person vaccinated. The wide availability of a cadre of staff known as "Ministry of Health volunteers" - individuals already vetted by the ministry and available to work as mobilizers on special projects against per-diems – increases the potential for scaling this project nationwide in Sierra Leone. One potential challenge of replicating this project to other countries is to find trained staff who can take on that mobilization role.

Note that here we are looking at cost-effectiveness from the perspective of the planner (i.e., the government), and do not consider the costs imposed on households. Depending on context, meeting attendance can be inconvenient or costly. In our context, villages are small. On average, people had to walk less than a couple of hundred meters to attend the meetings. Also, to minimize the inconvenience, meetings were held in the early evenings after people returned from their farms. As a result, the opportunity cost of time is low for most meeting participants.

Figure 7 provides the "cost per vaccinated person" in year 2000 dollars for the subset of studies in Table A1 Supplementary Information Section 2 that reported detailed enough cost information for us to be able to compute this metric. Of the 234 different treatments identified in our systematic literature review, only 33 (14%) directly stated the cost of the intervention per successfully administered vaccination. Furthermore, of these 33 interventions, 7 did not report a cost specific to the treatment arm, but only the overall cost averaged over all arms of the study. Most vaccination campaigns exceed our US$ 33 benchmark. The mean value in Figure 7 is US$ 102 (SD = 162), even after top-coding the most expensive approaches.

A study in rural India[28] pursues a similar strategy to ours by setting up measles vaccination clinics. That treatment costs US$75 (in 2022 dollars) per vaccine administered but adding an incentive for the parents to bring their children to the clinic lowers the cost to US$38 per child vaccinated. The only other COVID-19 vaccine study in our systematic literature review to provide cost information[21] offered US$24 as a financial incentive to get vaccinated in Sweden. Unfortunately, this study does not report the costs of other program components, such as the cost of administering the incentive program, verifying individual-specific vaccination information in the administrative records, sending two text message reminders, etc.

Policy Implications. Vaccine equity remains an important policy goal.[10] Vaccination rates are severely lagging among rural Africans, so achieving equity requires us to devise an effective strategy to reach this population. Our study provides some clear guidance on how to formulate that strategy.

The most immediate and direct implication of our results is for the government of Sierra Leone to replicate and expand this cost-effective program to reach the 59% of the

country's population who reside in similar remote, rural areas outside of PHU coverage. The bulk of our intervention cost is the transport cost of reaching remote communities, so an obvious implication is that we should bundle COVID-19 vaccines with other necessary mother/infant/child health interventions that can be delivered simultaneously on the same trip. That could dramatically reduce costs *per person treated*. This would still be expensive for a resource-constrained MoHS to launch at scale, and international partners must provide support. A recent study in rural Western Kenya[29] demonstrates that such integrated approaches combining HIV testing with other preventative health services like bednets and water filters can be successfully implemented.

We have begun building the necessary coalition to implement such a bundling strategy to improve the cost-effectiveness and scalability of this last-mile-delivery intervention. Sierra Leone MoHS has prioritized HPV vaccination for girls aged 10-12, and routine immunizations (DTP, Measles, Polio) for children aged zero to six to bundle with any further COVID-19 vaccine delivery. It is reasonable to wonder whether COVID-19 vaccine distribution is a high-priority investment, given the low incidence of COVID-19 in Africa. But as the Indian experience from April 2021 shows, new COVID-19 variants have the capability to devastate public health systems in developing countries. Health infrastructure in the typical African nation is even more fragile than it is in India. If we pay the transport cost to take a bundle of health interventions to these remote communities, COVID-19 vaccines could easily be an element of that bundle. Operationally, this leverages existing but underutilized (peripheral health unit) clinic infrastructure in a hub-and-spoke model to provide mobile vaccination services near citizens' doorsteps, and bring health services more cost-effectively to the most remote communities that currently lack access.

The other direct implication is to replicate such a program in neighboring countries with similar last-mile delivery challenges. The majority of sub-Saharan Africans reside in rural areas, so overcoming access challenges through such initiatives holds enormous potential for both achieving vaccine equity and maximizing global coverage.

Our study shows that low-income countries need to experiment with creative ideas to overcome stubborn logistical challenges, such as setting up temporary clinics and sending both doses and nurses to remote locations on motorcycles. A broader implication for international development partners and pharmaceutical companies is that they need to facilitate and underwrite such experimentation by making vaccine doses and budgets readily available to allow ministries of health to learn what approaches work best in a given context. Local institutions need to engage in "learning by doing", which is impossible without a reliable supply of vaccines, and incentives for staff to tinker with innovative ideas.

Study Limitations. The intervention we implemented had two important limitations. The US$ 33 cost (per person vaccinated) varies substantially across villages because the number of individuals per village that we managed to vaccinate varies. Village leaders did

not allow us to conduct the intervention *at all* in 4 of the 100 treatment villages, which inflates the overall average cost of our intervention. Any replication should try to identify early the villages where such refusals might occur, and find ways to avoid having the entire vaccination team travel to such villages.

Second, we observe large cross-team variation in performance. Figure A4 Supplementary Information Section 1 shows that some of our teams administered over twice as many vaccines as other teams. Some of these differences could be due to differences in village characteristics, but our implementation experience suggests that variability in team effort also played a role. Providing good performance incentives to teams could improve the cost-effectiveness of this exercise. Given that a large portion of the cost of the intervention is the cost of traveling to the remote village, we should strategize to ensure that we maximize the vaccination rate within the 48-72 hour window once we get there.

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8. Figures

Figure 1: Vaccination Team Visits Procedure

(a) Step 1: Meet with the community leaders

(b) Step 2: Organise community meeting

(c) Step 3: Bring vaccines and nurses to these remote communities

(d) Step 4: Set up a temporary clinic for the next two-three days

(e) Step 5A: Treatment 1 - Door-to-door mobilisation

(f) Step 5B: Treatment 2 - Small group mobilisation

Figure 2: **Vaccination *Rate* Amongst Adults Enumerated During Census Before and After Mobile Vaccination Program**

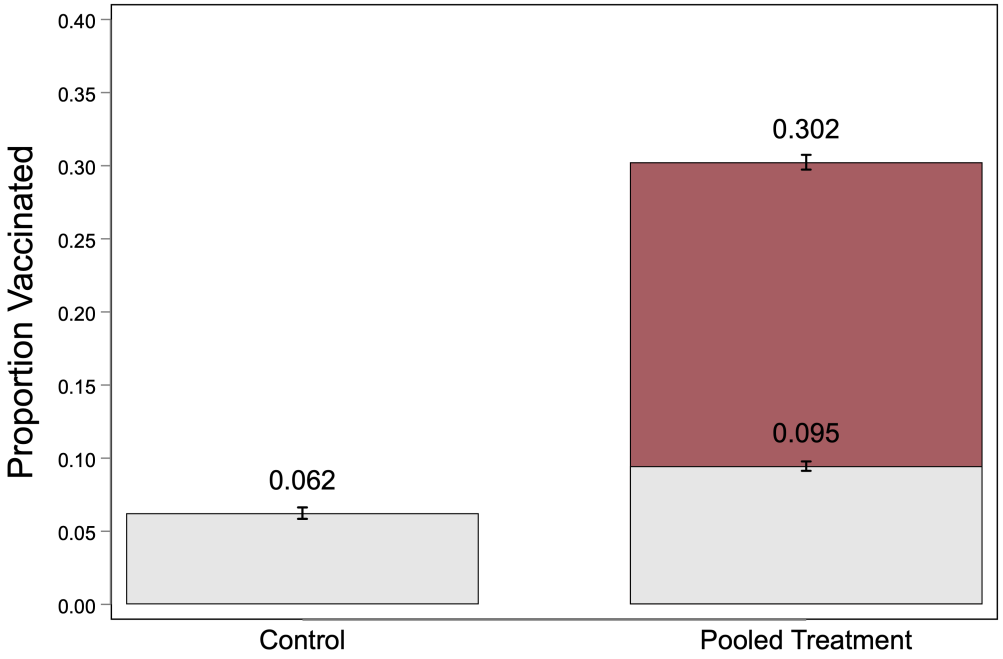


Figure 3: **Count of People Vaccinated per Site Before and After Mobile Vaccination Program**

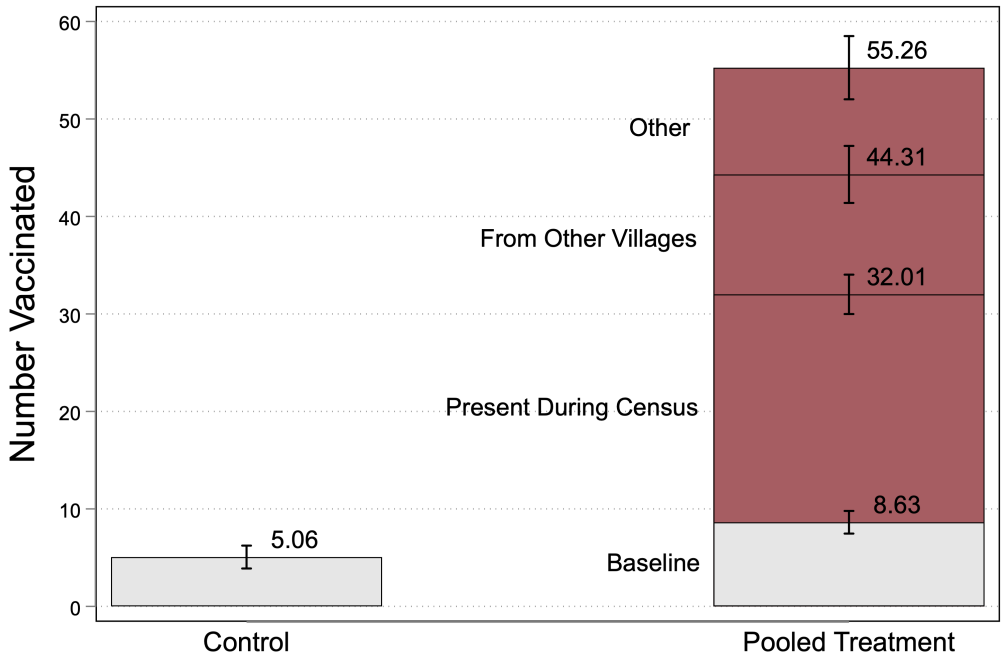


Figure 4: **Effect of Pooled Treatment on Knowledge and Attitudes Among Adults Enumerated During Census**

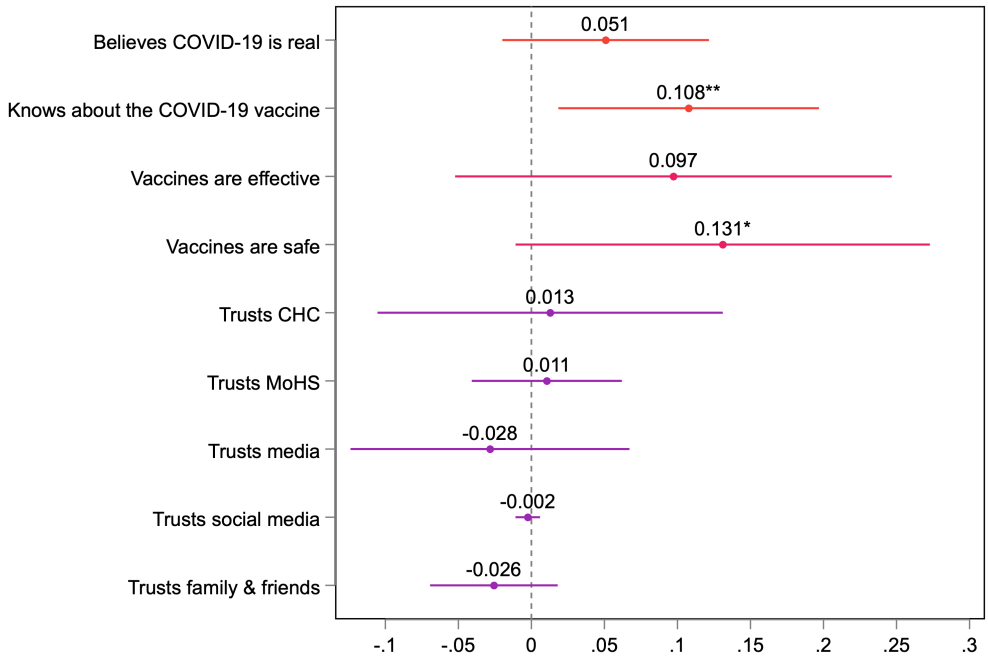


Figure 5: **Effect of Pooled Treatment by Respondent Characteristics Among those Enumerated During Census**

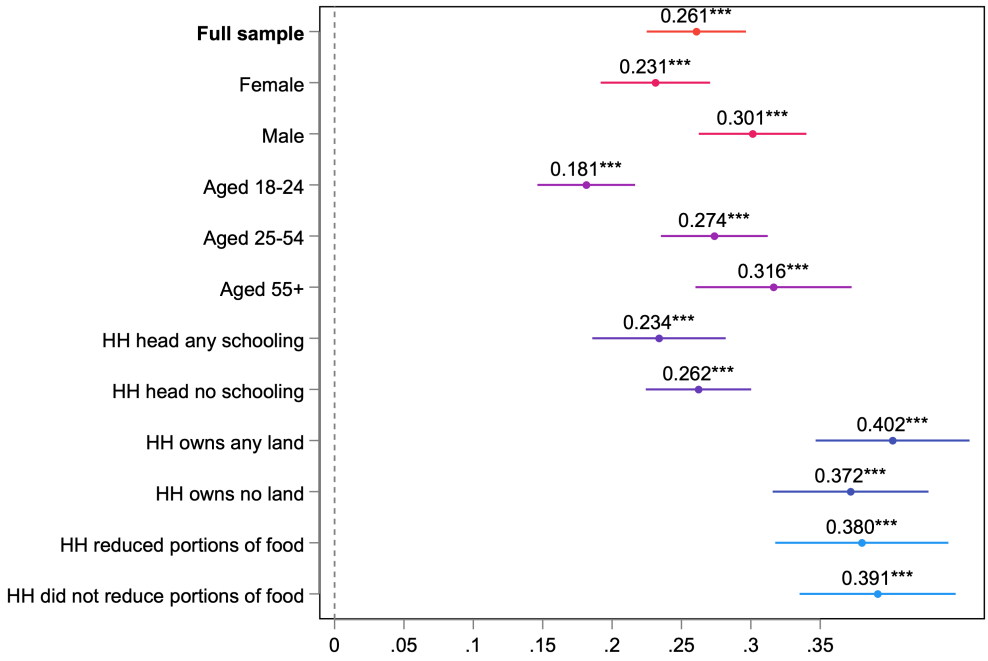


Figure 6: **Effect Sizes in Other Vaccine Uptake RCTs**

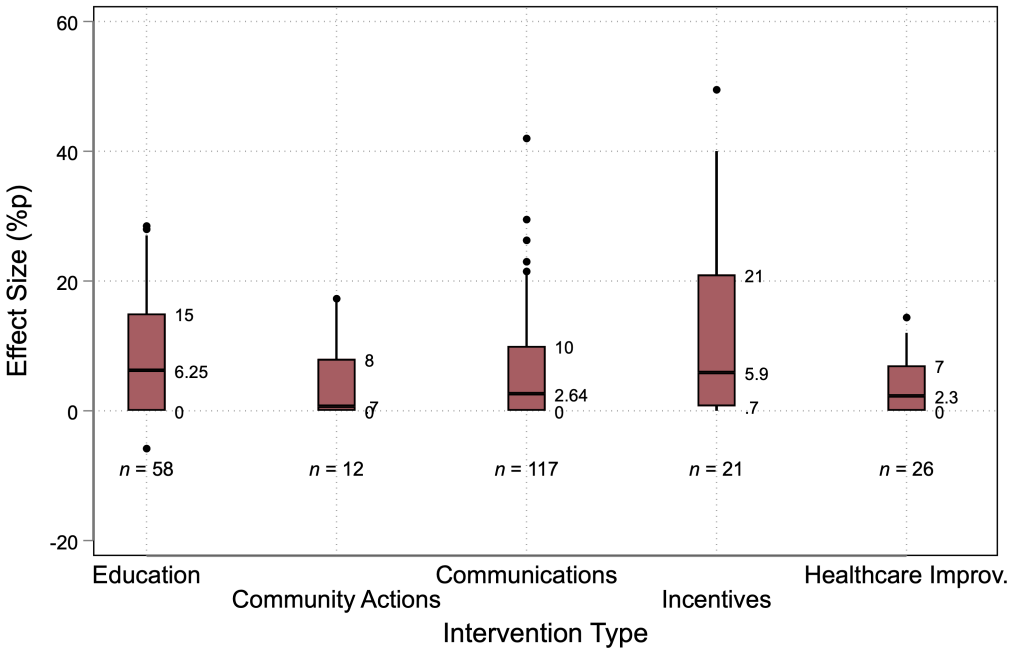
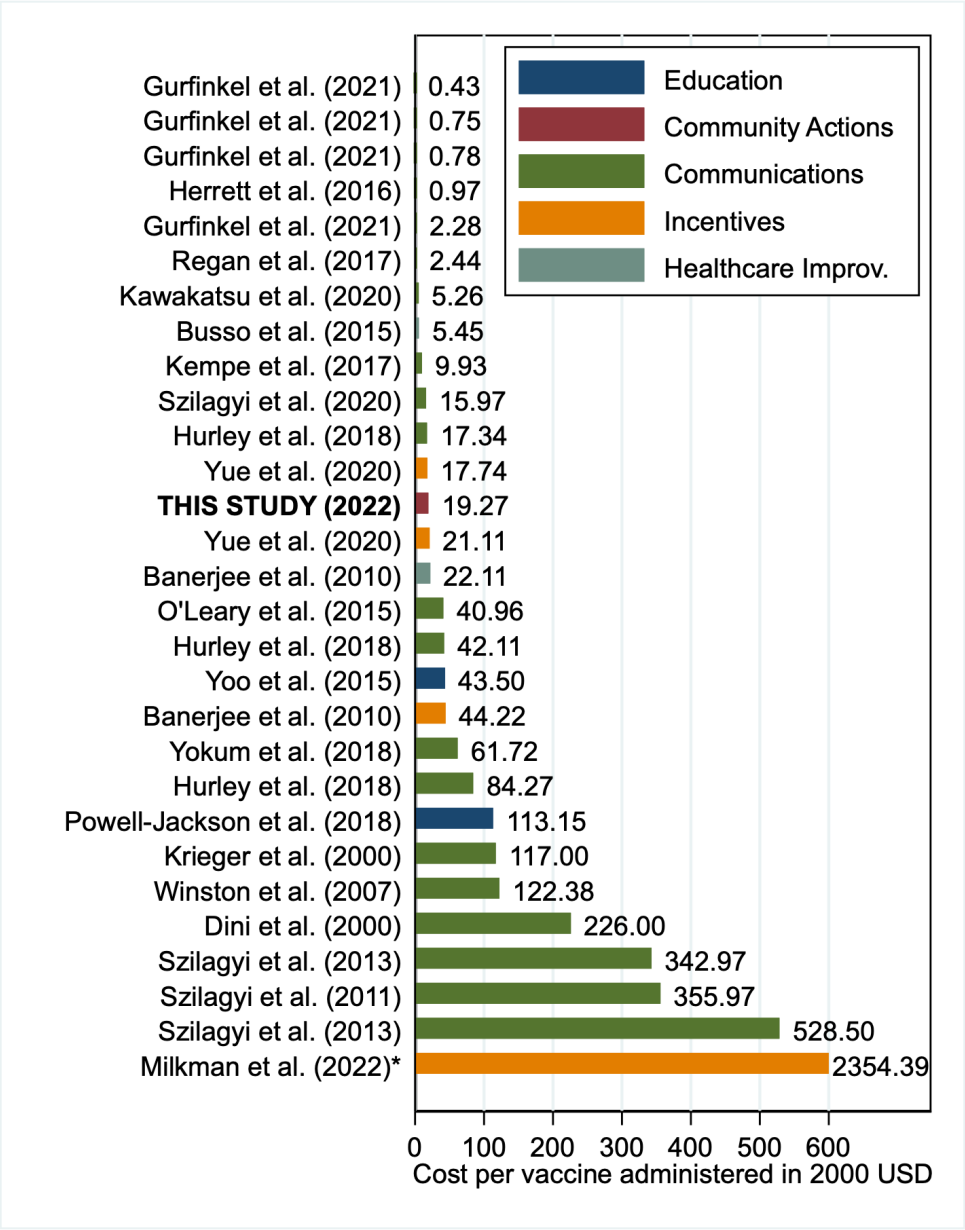


Figure 7: **Cost Per Person Vaccinated Compared to Other Studies**



9. Main Text Figure Legends

Main Text Figure Legends

Figure 1: Vaccination Team Visits Procedure

Figures (a) - (f) lay out the steps taken by the Vaccination Teams in each village for the mobile vaccination clinic. (a) MoHS social mobilization team organize a meeting with village leaders. (b) Social mobilizers convene a community meeting to talk directly with all village residents about vaccine efficacy and safety, the importance of getting vaccinated, and to address villagers' questions and concerns, and the location and timing of the mobile vaccination site. (c) MoH staff bring vaccine doses and staff to the village. (d) MoH staff set up 48-72 hour mobile vaccine clinic in a central location in the village. (e) Social mobilizers provide vaccine information to community members in private during Door-to-Door visits. (f) Social mobilizers targeted social groups at fixed spots in and around the villages.

Figure 2: Vaccination *Rate* Amongst Adults Enumerated During Census Before and After Mobile Vaccination Program

The figure shows the proportion of vaccinated adults that were enumerated during the census before and at the end of the study in control and pooled treatment villages. The analysis includes the 12096 people (18+) in 150 villages. Data are presented as mean values +/- s.e.m. In the control group 6% were vaccinated at baseline, whereas 9.5% were vaccinated in treatment arms. At endline 30% were vaccinated. The Intent-To-Treat treatment effects estimated using OLS and include randomization block fixed effects and heteroscedasticity-robust and standard errors clustered at the village level are included in Extended Data Table 3.

Figure 3: Count of People Vaccinated per Site Before and After Mobile Vaccination Program

The figure shows the number of the people vaccinated before and by the end of the study. Data are presented as mean values +/- s.e.m. The analysis includes 150 villages. In the control group on average 5 people were vaccinated, whilst in treatment villages this was 9 people. Treatment increased the count to 55 people, including 22-23 individuals that were enumerated during the census group, 12-13 people from nearby villages, and and 11-12 short-term, circular commuters or migrant returnees who were not present on the day of the census and could not be matched to our listing records, as well as individuals whose "community of origin" was unknown. The Intent-To-Treat treatment effects estimated using OLS and including randomization block fixed effects and heteroscedasticity-robust standard errors are included in Extended Data Table 4).

Figure 4: Effect of Pooled Treatment on Knowledge and Attitudes Among Adults Enumerated During Census

figure shows Intent-To-Treat estimates of community treatment assignment for each outcome

listed on the Y-axis. Treatment effects are estimated using OLS and including randomization block fixed effects and heteroscedasticity-robust standard errors clustered at the village level. Each dot is labelled with the exact coefficient (to three decimal places) and ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. Bars represent 95% confidence intervals of treatment estimates. The analysis includes 45 villages and 817 households surveyed at endline for which we observe complete randomization blocks. Associated regression results are included in Extended Data Table 6 and Table 7 including corresponding sample sizes. Reported estimates do not correct for multiple hypothesis testing. Extended Data Tables report the associated FDR-adjusted q-values. The survey measures for the "Believes COVID-19 is real" comes from a survey question: "Do you believe that COVID-19 exists in the world?" [Yes/No]. "Knows about COVID-19 Vaccination" comes from a survey question: "Do you know about the COVID-19 vaccine/marklate?" [Yes/No], "Vaccines are Effective" is 1 if respondents completely agree with the statement: Vaccines are effective.", "Vaccines are safe" is 1 if respondents completely agree with the statement "How much do you agree with this statement: Vaccines are safe.". Trust in sources of information are from a multiple select question "Who do you most trust getting information about COVID-19?" [CHC, MoHS, Media (News, TV), Social Media (Facebook etc), Family/Friends, etc]

Figure 5: Effect of Pooled Treatment by Respondent Characteristics Among those Enumerated During Census

The figure shows Intent-To-Treat estimates of vaccination rate of the pooled treatment arms for each subgroup listed on the Y-axis. Treatment effects are estimated using OLS and include randomization block fixed effects and heteroscedasticity-robust standard errors clustered at the village level. Each dot is labelled with the exact coefficient (to three decimal places) and ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. Bars represent 95% confidence intervals of treatment estimates. The dependent variable is the vaccination status of adults at the end of the study enumerated during the census. Gender, age and schooling data come from the census. Land ownership and food insecurity come from the baseline sample. Associated treatment estimates and associated sample size for each subgroup are included in Extended Data Table 8. The indicator for "HH Head Any Schooling" indicates if the household head had schooling above the primary level; "HH owns any land" indicates if the household owns land; "Reduced portions of food" indicates if any household member had reduced food portions during the prior week.

Figure 6: Effect Sizes in Other Vaccine Uptake RCTs

The figure shows boxplots with the percentage-point change in reported vaccine uptake relative to control group across the 234 treatments assessed as part of our systematic literature review (see Table A1 in Supplementary Information Section 2). This includes a sizeable group of treatments for which there was no significant effect of the intervention (82 treat-

ments, constituting 35% of all interventions reviewed). Effect sizes are summarized over five broad types of interventions. Each box represents the Interquartile Range (IQR), horizontal line is the median, whiskers indicate the 5th to 95th percentile, whilst outliers beyond these extremes are indicated with a marker.

Figure 7: Cost Per Person Vaccinated Compared to Other Studies
The figure includes the cost per vaccination administered(in 2000 USD, calculated using inflation data sourced from the US Bureau of Labor Statistics). These are treatments included in Table A1 in Supplementary Information Section 2 that explicitly provided information about the cost of the intervention per vaccine actually administered. This cost specifically refers to the intervention, and does not include the cost of the vaccine itself. Studies that did not unequivocally state the cost of the intervention per vaccinated person were not included. The colour of each bar indicates the broad type of intervention. The cost per person vaccinated in our study is US$ 32.70, which is approximately US$ 19.27 in 2000 USD. We top-coded the most expensive approach from $2354.39. The mean value in this figure is US$ 101.62 (SD = 161.85).

10. Methods

10.1 Ethics Approval

We received Institutional Review Board (IRB) approval from the Sierra Leone Ethics and Scientific Review Committee (SLERC 20220210), Yale University (2000031541) and Wageningen University (WUR 20220222). The research protocol was pre-registered at ISRCTN (study ISRCTN 17878735, see `https://doi.org/10.1186/ISRCTN17878735`). All study participants completed informed consent.

The study was implemented in close collaboration between the researchers, the Government of Sierra Leone's Expanded Programme on Immunization (EPI) at the Ministry of Health and Sanitation (MoHS), their National COVID-19 Emergency Response Centre, and Concern Worldwide: an international NGO who partners with MoHS on health projects. This collaboration came together because all partners had the joint goal of addressing barriers to vaccine adoption in rural Sierra Leone. While all partners are responsible for the research design, only the Ministry of Health team was responsible for actually distributing and administering vaccines. We had a memorandum of understanding in place to govern this collaboration.

10.2 Village Study Sample

To determine the sample size, we ran a power calculation assuming a 5% significance level with 80% power. We assumed an ICC of 0.15 as decisions to take a vaccine are likely highly correlated within a village. Average village populations are 2480 people. We assumed an eligible population of 50% and a baseline vaccination rate of 2.5%. Based on the treatment effects reported in the literature for similar studies, we took a conservative approach and set our expected MDE at 0.05. We oversampled slightly and the final design included 150 communities across the three treatment arms, in 1:1:1 ratio.

We chose study sites in collaboration with the MoHS. We started with the 2015 Sierra Leone Census which contains data on 20,659 communities in 166 Chiefdoms across 16 Districts. We selected 7 largely rural districts (Koinadugu, Falaba, Karene, Kambia, Tonkolili, Bombali, Port Loko), limiting the sample to 8,784 communities in 54 Chiefdoms. We then restricted our sampling frame to communities that according to the 2015 census had no health clinic within five miles of the community center, the standard PHU catchment area (see Table 10), resulting in 1,849 communities. From this list we excluded very small communities which contained fewer than 19 structures as well as communities for which latitude and longitude were missing. The final sampling frame consisted of 420 communities located in 49 Chiefdoms and 7 Districts. Within each District, we then matched communities on the following strata: i. the share of the population that was immunized, ii. the age of the population, iii. literacy levels, and the iv. distance from the closest clinic. This allowed us

to identify communities that had the most similar characteristics within a district and used this to assign the most similar communities to one of the treatment group and establish comparable "triplets". This resulted in 106 triplets in total. We then randomly selected 50 triplets using district as a blocking variable. The final list included: 9 triplets each for Koinadugu and Falaba District, 8 triplets for Karene District and 6 triplets each for Port Loko, Tonkolili, Kambia and Bombali District.

10.3 Randomization

Randomization to Vaccine Access Treatments: Within each of the 50 triplets, we randomized villages into control, Door-to-Door and Small-Group treatment arms. This results in 50 villages assigned to control, 50 to Door-to-Door, and 50 to Small Group, see Figure A1 in Supplementary Information Section 1. The sample is well balanced on observable characteristics, the F-statistic at the bottom of Extended Data Table 2 is small and not significant.

Household-level Random Assignment to Door-to-Door Treatment: Within the villages randomly assigned to the "Door-to-Door treatment" arm, we randomly selected up to 20 residential structures from the community census list to receive a visit from the social mobilisation team.

10.4 Data Collection

Community Census Listing and Baseline: Before any intervention activities took place, the research team implemented a community census to enumerate all households in all 150 villages. The research team went door to door to each residential structure and asked how many households resided in the structure. They then interviewed each household head to create a roster of those who "eat from the same pot; and reside under the same roof for at least the past 9 months (aside from newborn babies)." For each household member enumerators asked about the gender, age, and vaccination status. The total census includes N=31,913 people. Migrant household members who were temporarily away on the day of the visit would have been missed from this listing.

Next, the research team randomly selected a sample of 20 households per village from the households listed in the census to conduct a short (baseline) survey with the household head, to record household characteristics (age, gender and education), access to land and food security. Some villages contained fewer than 20 structures. The total baseline sample included N=2,240 respondents.

Exit and Endline Surveys: After the interventions were implemented, the research team conducted an exit survey of those who took a vaccine at each mobile vaccination clinic. The

survey recorded the vaccination status verified using visual inspection of the vaccination card, as well as age and gender.

During the exit survey, enumerators also recorded where people came from and their District and Village name (if different from the implementation site). To assess between-village spillovers, we then matched the names of reported villages back to our list of control villages. Using a hard match on District names and then a Levenshtein distance metric to match village names, allowing for a string distance of 2, we find only 8 matches. Using a more conservative cut-off of 1, find no overlap whatsoever. Our within-sample spillovers are small or non-existent due to the large physical distance between pairs of sample villages. The minimum straight-line distance between project treatment and control villages was 8.5 miles, which would take at least 2-3 hours to traverse by foot. Any spillover benefits largely accrued to others who were not part of the experimental pool.

For a sub-sample, the research team conducted a follow-up survey to capture knowledge of COVID and COVID vaccines as well as trust in various sources of information. We use data from 878 respondents in 45 villages for which we observe triplets (i.e., where we have information on all treatment arms and a 1:1:1 ratio). We collected data in a total of 105 villages (50 control, 30 Door-to-Door, and 25 Small-Group treatment arm villages), however only for 45 villages do we observe all three treatment arms and therefore provide a clean comparison. Respondents in this sub-sample of villages are very similar to those in the overall sample. An overall F-test does not reject the equality of means: p-value = 0.649, see Extended Data Table 10.

In treatment villages, these questions were part of the exit survey and implemented one day after intervention activities were completed. In control villages, households were visited only once. From a design perspective, we would have ideally captured outcomes at both baseline and endline in each village. It was however highly unlikely that these remote places would have been visited by other health personnel from MoHS, or NGOs in the 5 day period between baseline and endline, or that a large number of people would have incurred the cost of visiting the CHC for receiving a COVID vaccine. In addition, the costs of revisiting communities in these remote locations are high (the largest line item on the budget relates to transportation costs, see Table 9. We verified that there was no vaccination drive conducted during this period. Further, we use the fact that our baseline survey was conducted over a few weeks across communities to inspect the temporal trends in the data. A simple regression of baseline vaccination rates on the date of the baseline survey does not reveal any trend. This reduces the concern that our choice to not revisit control villages affects the conclusions we draw.

Research assistants were blinded with respect to treatment arm and study hypothesis.

10.5 Intervention Details

Timeline of Activities: The research team collaborated closely with the Ministry of Health vaccination team. Both the team of vaccinators and social mobilizers from the Ministry of Health and Sanitation and enumerators in charge of the survey received extensive training on implementation protocols. Only those individuals who were considered proficient after examination were retained for implementation or data collection. Within each village the teams followed several steps outlined below. Please see Figure A1 in Supplementary Information Section 1 for further details.

- Day 1-2 – Research team implemented census listing and baseline surveys described above.
- Day 3–5 – Social mobilizer team engaged in Small-Group and Door-to-Door mobilization; Vaccination Drive by MoHS; Exit Survey by Research team in treatment villages
- Day 6 – Research Team implemented endline for sub-sample of households in 45 villages

Social mobilization: MoHS trained community mobilizers on COVID-19 vaccine safety and efficacy, on vaccine types and availability. All mobilizers were trained on how to respond to questions, and counter any mis-information about COVID-19. They were also trained on WHO-recommended safe practices relating to COVID-19, and were instructed to maintain social distancing protocols, and wear masks when social distancing could not be guaranteed. Additional masks were made available for free for community members.

Community social mobilizers arrived at the village before the mobile vaccination teams. The community mobilizer engaged with local community leaders including the Town Chief/Section Chief/Paramount Chief, Mammy Queen, Town Elders, Youth leaders, CHOs, Imams, and any other relevant authorities, to seek permission to organize a village information session. The information session took place at a central location, often the community center or any other convenient location amenable to safe COVID-19 practices.

At the information session, the mobilizer informed community members about COVID-19, available vaccines, and evidence about the safety and efficacy of vaccines in preventing transmission and severe illness. People were also informed about the mobile vaccination team and operating procedures during the vaccination drive. They encouraged participants to spread this message to other members of the community not present during the meeting.

In four treatment villages the MoHS vaccination team did not receive permission from village authorities to conduct the vaccination drive.

Door-to-Door Campaign: In 50 of the 100 villages randomly selected for treatment, community mobilizers approached up to 20 structures randomly selected from the census list, after the group information session was completed. The proportion of each community assigned to treatment therefore varied with the population of the community. In four small communities all structures were assigned. Due to logistical complexities and costs, in some communities mobilizers did not include very remote village structures (more than 15 minutes walk from the village center. This excluded a total of 10 structures (including 40 people aged 12 and above). Social mobilizers met in private with residents and delivered the same information as was presented at the community meeting. In addition, they addressed people's concerns in private. If the individuals were immediately convinced to get vaccinated, the social mobilizer would guide them to the vaccination site before moving on to the next household. Neighbors not assigned to receive a home visit were present during the information session in a few cases. In 75% of the communities, these "compliance issues" were limited to representatives of three or fewer control households, and the majority of communities had no non-compliance of this kind.

Small-Group Mobilization: In the other 50 treatment villages, after the group information session, social mobilizers searched for small groups of people around the village to converse with. Such groups included women washing clothes around the river, individuals gathered at the ataya (tea) shops, residents playing a game of drafts, groups of people around the mosque or church or farm, or residents gathered near the Town Chief's house. Social mobilizers repeated the same information presented during the community information session. If people inside the small group had already taken the vaccine before this second session, they were invited to talk about their experience. After the session, if residents wanted to take the vaccine, the social mobilizer would guide them to the vaccination site before moving on.

Mobile Vaccination Drive: Vaccines were transported in approved cool boxes or vaccine carriers appropriate for transportation to remote locations. In each treatment village, the MoHS Mobile Vaccination teams worked with community leaders to select a suitable venue for the vaccination drive. The venue was chosen with the following requirements in mind: it needed to accommodate a waiting area (with some shelter); an arrival and check-in area – where patient information can be gathered maintaining confidentiality; space for clinical assessment and vaccine administration including vaccine preparation, maintaining patient confidentiality, privacy and social distancing; area and system for post-administration observation of patients.

Individuals below 12 years of age were excluded from vaccinations. MoHS teams determined on-site whether a person deemed "at risk" (e.g. pregnant or suffering from severe disease) would be excluded also. After the vaccine is administered, recipients were asked

to remain in close proximity to the vaccination team for a minimum of 15 minutes, in the event that they experienced any unexpected side-effect.

Vaccine teams were compliant with MoHS requirements for the storage, preparation, administration, and disposal of the vaccine and associated materials. They followed Infection Prevention and Controls (IPC) and checked the eligibility of people to be vaccinated using the patient checklist.

Mobile teams adhered to MoHS guidelines on informed consent to receive COVID-19 vaccination, ensuring it was taken only by people with the mental capacity to consent to the administration of the vaccines, and taken freely, voluntarily, and without coercion. Participants were allowed to withdraw consent at any time.

All vaccine teams received training on vaccinations including the management of Adverse Events Following Immunization (AEFIs). All AEFIs had to be reported using national reporting systems to the MoHS.

10.6 Statistical Analysis

To estimate the impact on the adult vaccination rate, presented in Extended Data Table 3, we estimate Intent-To-Treat effects using OLS on individual-level data:

$$Y_{i,j} = \alpha_k + \beta_{1,j} T_{pooled} + \epsilon_{i,j} \tag{1}$$

where $Y_{i,j}$ is the vaccination status of individual i, in village j, T_{pooled} is the village assignment to either Door-to-Door or Small-Group treatment arms. α_k is a vector of randomization block fixed effects (ie triplet) and $\epsilon_{i,j}$ are heteroscedasticity-robust standard errors clustered at the village level. We estimate effects using a linear estimator that accounts for high dimensional fixed effects.[30] In additional analysis we add to the right hand side of this equation $Y_{i,j,bl}$ the baseline vaccination status, and X_j - the vector of covariates that were unbalanced at baseline. We also estimate (1) at the village level and for each arm, ie by estimating both $\beta_{1,j} T_{DoortoDoor}$ and $\beta_{2,j} T_{SmallGroup}$ for the Door-to-Door or Small-Group treatment arms, see Table 1.

To estimate the vaccination count, presented in Extended Data Table 4, we estimate a *village-level* Intent-To-Treat effect using OLS on village level data:

$$Y_j = \alpha_k + \beta_{1,j} T_{pooled} + \epsilon_j \tag{2}$$

where Y_j is the number of people vaccinated in village j, T_{pooled} is the village assignment to either Door-to-Door and Small-Group treatment arms. α_k is a vector of randomization block fixed effects (i.e. triplets) and ϵ_j is the heteroscedasticity-robust standard error. We estimate equation 2 for several types of respondents, ie those that were part of the village census, migrants, returnees and those not present during census, and those from other

villages, and additionally add X_j a vector of covariates that were unbalanced at baseline.

To assess the individual-level effect of the Door-to-Door campaign, we restrict our sample to the 50 villages assigned to the Door-to-Door campaign (ie $T_{DoortoDoor}$ =1), and estimate Intent-To-Treat effects using OLS:

$$Y_{i,s} = \alpha_j + \delta_i T_{DoortoDoor} + \mu_{i,s} \tag{3}$$

where $Y_{i,s}$ is the vaccination status of individual i in structure s (hut or house), $T_{DoortoDoor}$ the individual level assignment to receive a visit by the social mobilisation team to a structure, α_j is a vector of randomization block fixed effect (ie the village) and $\mu_{i,s}$ is the heteroscedasticity-robust standard error clustered at the structure level.

For the survey-based outcomes on COVID-19 vaccine knowledge and trust, we estimate equation (1), replacing the dependent variable with the survey responses described above, using the sub-sample of 45 villages where this data was collected and we have data on the full randomization blocks.

For our the treatment effects by subgroup, we estimate equation (1) separately for men, women, various age groups (18-24, 25-54, 55+), and sample splits based on whether the household head had any schooling, owns any land, or reduced portions of food. To compare across groups we use a Chi-Square test from a Seemingly Unrelated Regression estimation.

In the results Extended Data Tables 3, 4, 6 and 7, we also adjust for the fact that we conduct multiple tests on the same dataset by implementing false discovery rate (FDR) corrections and report the FDR q-values.[31] We also report the bootstrapped p-value[32] to account for regressions with a small number of clusters.

10.7 Systematic Review of Vaccination Uptake RCT Studies

We conducted a systematic review of articles in PubMed published between Jan 1, 2000 and Jan 7, 2023 using the search terms "(vaccin*[Title/Abstract] OR immun*[Title/Abstract]) AND *additional search term*[Title/Abstract]) AND (Randomized Controlled Trial[Publication Type])", with additional search terms: "access"; "community-based"; "cost effect*"; "demand"; "hesitant"; "incentive*"; "intervention*"; "mobile"; "nudge*"; "rural"; and "supply". These searches yielded 3,615 unique articles. We screened out articles that are not related to vaccine uptake or that did not rely on an RCT, reducing the sample to 141 articles. We appended a further 20 relevant studies that were identified by snowballing from this sample and rejected 17 papers that did not have a control group; did not report the percentage-point change in vaccine uptake; or did not include a test statistic. The final list of 144 articles comprises 234 unique interventions for which we can report a percentage-point change relative to a control group (see Table A1 Supplementary Information Section 2). Of these, 33 interventions (14%) report information about the cost of the intervention per vaccine

administered. This cost specifically refers to the cost of implementing the intervention and does not include the cost of the vaccine itself. Studies that did not unequivocally state the cost of the intervention per vaccinated person were not included in our cost-effectiveness comparisons. Two studies reported the cost in currencies other than USD,[33,34] and these costs were converted to USD equivalent for the year the study was published, using exchange rate data from the respective countries' national statistics agencies.

10.8 Deviations from Pre-registered Hypotheses

We pre-registered our research protocol and hypotheses at ISRCTN (study SRCTN 17878735, see `https://doi.org/10.1186/ISRCTN17878735`).

We report on our main hypothesis in Figure 2 and Extended Data Table 3. In addition to reporting on our main pre-registered outcome (adult vaccination rate) we also report on the total shots given per vaccination site, because many more people showed up to our temporary clinics from neighboring villages or were not present during the pre-intervention census, and we had not anticipated this. Figure 3 and Extended Data Table 4 therefore report on the count of all individuals (aged 12 and above) who visited our clinics to take a shot. This metric is necessary to compute cost-effectiveness correctly.

The heterogeneity analysis reported in Figure 5 where we study whether vaccination rates differ by age, gender, schooling, and wealth variables, was not pre-specified and follow heterogeneity tests that are common in the vaccine literature.[35]

10.9 Data Availability

The primary survey data forming part of this study were collected using SurveyCTO software, version 2.81. These de-identified datasets are available via Harvard Dataverse, `https://doi.org/10.7910/DVN/PRXF5Z`.

10.10 Code Availability

All analysis for this paper was conducted using Stata SE 17. Replication files and de-identified data are available via Harvard Dataverse, `https://doi.org/10.7910/DVN/PRXF5Z`.

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Author contributions

N.M, A.M.M., M.V, S.C and D.K conceptualized the project; V.R and M.L curated the data; V.R., M.L., E.T., M.R. and M.V. undertook formal analyses; N.M, A.M.M. and M.V acquired funding; N.M, M.L, M.R. and S.C. S.K. and J.N performed the investigations; N.M, A.M.M., M.V, V.R and M.L designed the methodology; N.M and M.V administrated the project; M.L, V.R, S.K., J.N. and M.R. supervised the work; all authors validated the findings; V.R, E.T., M.V and M.R. visualized the data; A.M.M. and M.V. wrote the first draft, with all authors contributing to writing, and all authors contributed to the review and editing of the paper.

Competing interests

The authors declare no competing interests. The authors did not receive financial or non-financial benefits from the donors, NWO, Weiss Asset Management and the IGC, or any other partners related to any of the interventions presented here.

A. Extended Data Tables

Table 1: Intent-To-Treat Effect of Door-to-Door and Small-Group Treatments

	(1)	(2)	(3)
Door-to-Door	0.293***	0.279***	-0.011
	(0.023)	(0.023)	(0.019)
Small-Group	0.231***	0.235***	
	(0.021)	(0.020)	
Proportion Vaccinated at Baseline		0.628***	
		(0.076)	
Additional Covariates	No	Yes	No
Observations	12096	12096	3760
Mean in Control	0.062	0.062	0.282
No. of Villages	150	150	50
No. of Structures	3479	3479	1120
$P(\beta_{Door-to-Door} = \beta_{Small-Group})$	0.014	0.042	
R^2	0.137	0.157	0.140

Table 2: Baseline Descriptive Statistics and Statistical Balance

	Control Mean (SD)	Control-Door to Door Diff (SE)	Control-Small Group Diff (SE)	Door to Door-Small Group Diff (SE)	N
Community Characteristics from 2015 Census					
Percent of infants in locality fully immunized	0.502	0.015	-0.016	0.031	150
	(0.300)	(0.034)	(0.034)	(0.035)	
Percent of locality that is literate	0.284	-0.028	-0.016	-0.013	150
	(0.206)	(0.023)	(0.028)	(0.026)	
Average age	21.483	-0.118	0.053	-0.171	150
	(2.366)	(0.321)	(0.308)	(0.341)	
Lives five miles or more from health facility	0.960	0.022	-0.000	0.022	150
	(0.196)	(0.018)	(0.011)	(0.018)	
Percent of locality that is Christian	0.119	-0.005	0.027	-0.032	150
	(0.249)	(0.038)	(0.044)	(0.044)	
Percent of locality that is Muslim	0.867	0.010	-0.014	0.024	150
	(0.261)	(0.040)	(0.045)	(0.044)	
Percent of locality born in the same Chiefdom	0.931	-0.020	0.008	-0.028	150
	(0.115)	(0.022)	(0.020)	(0.023)	
Percent of locality that is employed in agriculture.	0.937	-0.076	-0.052	-0.024	150
	(0.078)	(0.032)	(0.029)	(0.032)	
Percent of locality with Access to Internet	0.027	-0.010	-0.016	0.007	150
	(0.073)	(0.011)	(0.010)	(0.008)	
Living in formal structures	0.795	0.046	0.003	0.043	150
	(0.354)	(0.056)	(0.059)	(0.056)	
Own land	0.989	-0.022	-0.023	0.001	150
	(0.023)	(0.017)	(0.016)	(0.019)	
Lives within five miles of primary school	0.616	-0.117	-0.019	-0.098	150
	(0.478)	(0.081)	(0.078)	(0.080)	
Lives within five miles of water source	0.942	-0.041	-0.047	0.006	150
	(0.211)	(0.051)	(0.049)	(0.054)	
Owns a radio	0.508	-0.105	-0.080	-0.024	150
	(0.293)	(0.054)	(0.055)	(0.051)	
Owns a cell phone	0.315	-0.026	-0.020	-0.005	150
	(0.249)	(0.048)	(0.047)	(0.046)	
With formal roofs	0.546	0.055	0.060	-0.005	150
	(0.365)	(0.051)	(0.059)	(0.058)	
Average number of assets	1.704	-0.112	-0.063	-0.049	150
	(0.699)	(0.159)	(0.150)	(0.146)	
Characteristics from Village Census					
Village population	187.080	-5.400	35.920	-41.320	150
	(118.683)	(23.612)	(26.227)	(25.538)	
Proportion of adults vaccinated at baseline	0.061	0.064	0.029	0.034	150
	(0.093)	(0.025)	(0.022)	(0.026)	
HH head has had any schooling	0.177	-0.009	0.028	-0.037	150
	(0.108)	(0.022)	(0.022)	(0.021)	
Owns land	0.678	-0.086	-0.013	-0.073	149
	(0.175)	(0.041)	(0.040)	(0.043)	
Reduced portion sizes in last week	0.372	-0.005	0.058	-0.063	149
	(0.215)	(0.045)	(0.042)	(0.044)	
Age	22.514	0.004	-0.171	0.174	150
	(2.864)	(0.544)	(0.479)	(0.524)	
HH head is female	0.249	0.027	0.004	0.023	150
	(0.123)	(0.023)	(0.024)	(0.025)	
Is breastfeeding	0.119	0.021	0.020	0.001	150
	(0.065)	(0.012)	(0.010)	(0.011)	
Is pregnant	0.052	-0.018	-0.013	-0.005	150
	(0.088)	(0.012)	(0.012)	(0.009)	
Joint F-test p-value		0.635	0.906	0.934	

Table 3: Intent-to-treat Estimates of Vaccination Rate of People Enumerated During Census

	(1)	(2)	(3)
Pooled Treatment	0.261***	0.254***	0.283***
	(0.018)	(0.019)	(0.025)
Proportion Vaccinated at Baseline		0.659***	
		(0.076)	
Additional Covariates	No	Yes	No
Bootstrapped P-Value	0.000	0.000	0.000
Mean in Control	0.062	0.062	0.061
No. of Observations	12096	12096	150
No. of Villages	150	150	150
R^2	0.13	0.16	0.66

Table 4: Intent-To-Treat estimates of the Count of People Vaccinated per Site After Mobile Vaccination Program

	(1)	(2)	(3)	(4)	(5)	(6)
Pooled treatment	26.920***	38.960***	50.140***	26.109***	39.584***	50.488***
	(2.533)	(3.608)	(4.016)	(2.931)	(4.405)	(4.713)
Proportion Vaccinated at Baseline				36.897***	43.539***	42.348**
				(13.418)	(16.511)	(17.867)
Additional Covariates	No	No	No	Yes	Yes	Yes
Bootstrapped P-Value	0.000	0.000	0.000	0.000	0.000	0.000
Mean in Control	5.060	5.060	5.060	5.060	5.060	5.060
No. of Observations	150	150	150	150	150	150
R^2	0.59	0.58	0.65	0.64	0.62	0.68

Table 5: Proportion Vaccinated by Baseline Willingness to Take Vaccines and Meeting Attendance

	Attended meeting		
	No (1,066)	Yes (636)	Total
Would take COVID-19 vaccine if offered			
No (279)	0.144	0.538	0.272
Yes (1,423)	0.394	0.646	0.491
Total	0.350	0.631	0.455

Table 6: Intent-To-Treat estimates for Knowledge and Attitudes Towards Vaccines in Sub-sample

	(1) Believes COVID-19 is real	(2) Knows about the COVID-19 vaccine	(3) Believes vaccines are effective	(4) Believes vaccines are safe
Pooled Treatment	0.051	0.108**	0.097	0.131*
	(0.035)	(0.044)	(0.074)	(0.070)
Bootstrapped P-Value	0.274	0.070	0.311	0.153
FDR Q-value	0.162	0.084	0.162	0.117
Mean in Control	0.876	0.776	0.267	0.244
No. of Observations	817	817	686	686
No. of Villages	45	45	45	45
R^2	0.09	0.12	0.14	0.14

Table 7: Intent-To-Treat estimates for Which Source People Trust Most for Information on COVID-19 in Sub-sample

	(1) Community Health Clinic	(2) Ministry of Health and Sanitation	(3) Media	(4) Social media	(5) Family and friends
Pooled Treatment	0.013	0.011	-0.028	-0.002	-0.026
	(0.059)	(0.025)	(0.047)	(0.004)	(0.022)
Bootstrapped P-Value	0.855	0.707	0.604	0.626	0.375
FDR Q-value	.	.	.	.	.
Mean in Control	0.213	0.066	0.290	0.011	0.066
No. of Observations	817	817	817	817	817
No. of Villages	45	45	45	45	45
R^2	0.07	0.11	0.25	0.04	0.08

Table 8: Intent-To-Treat Estimates for Demographic Sub-groups

	(1) Full sample	(2) Female	(3) Male	(4) Aged 18-24	(5) Aged 25-54	(6) Aged 55+	(7) HH head any schooling	(8) HH head no schooling	(9) HH owns any land	(10) HH owns no land	(11) HH reduced food portions	(12) HH did not reduce food portions
Pooled Treatment	0.261***	0.231***	0.301***	0.181***	0.274***	0.316***	0.234***	0.262***	0.402***	0.372***	0.380***	0.391***
	(0.018)	(0.020)	(0.020)	(0.018)	(0.019)	(0.028)	(0.024)	(0.019)	(0.028)	(0.028)	(0.032)	(0.028)
Mean in Control	0.062	0.056	0.070	0.036	0.064	0.094	0.059	0.063	0.106	0.073	0.115	0.084
No. of Observations	12096	6797	5299	2662	7512	1922	2582	9514	1761	913	1072	1602
No. of Villages	150	150	150	150	150	145	139	150	147	144	139	149
R^2	0.13	0.13	0.16	0.11	0.14	0.20	0.15	0.14	0.23	0.24	0.22	0.24

Table 9: Cost-Effectiveness Analysis

	Cost Type	Total Cost	Cost Per Vaccination	N
Training Costs				
Training venue	Fixed Cost	14610.6	3.07	4771
DSA for trainees (76 team members + 7 DOOs)	Fixed Cost	14094.8	2.96	4771
Debriefing				
Training venue	Fixed Cost	5844.3	1.23	4771
DSA for trainees (76 team members + 7 DOOs)	Fixed Cost	7047.4	1.48	4771
Materials				
Printing of Vaccination cards	Variable Cost	6809.1	1.43	4771
Printing of screening forms	Variable Cost	8511.4	1.79	4771
Transport/Communication				
Vehicle hire + Fuel	Variable Cost	47401.5	9.95	4771
Fuel for DOOs	Variable Cost	7507.0	1.58	4771
Mobile phone top up	Variable Cost	4919.8	1.03	4771
Salaries Vaccination Teams				
Daily rate for vaccinators	Variable Cost	13661.8	2.87	4771
DSA for vaccinators	Variable Cost	25615.8	5.38	4771
Total		156023.5	32.70	4771

Table 10: Comparison of Full Sample to Sub-sample

	Full Sample N	Full Sample Mean (SE)	Restricted Sample N	Restricted Sample Mean (SE)	Difference (p-value)
Community Characteristics from 2015 Census					
Percent of infants in locality fully immunized	150	0.502	45	0.566	-0.064
		(0.293)		(0.283)	(0.078)
Percent of locality that is literate	150	0.269	45	0.265	0.004
		(0.192)		(0.191)	(0.877)
Average age	150	21.462	45	21.009	0.453
		(2.648)		(3.135)	(0.171)
Lives five miles or more from health facility	150	0.968	45	0.955	0.013
		(0.174)		(0.208)	(0.560)
Percent of locality that is Christian	150	0.126	45	0.099	0.028
		(0.253)		(0.209)	(0.384)
Percent of locality that is Muslim	150	0.866	45	0.886	-0.021
		(0.257)		(0.226)	(0.523)
Percent of locality born in the same Chiefdom	150	0.927	45	0.948	-0.021
		(0.125)		(0.105)	(0.177)
Percent of locality that is employed in agriculture.	150	0.894	45	0.894	0.000
		(0.199)		(0.210)	(0.987)
Percent of locality with Access to Internet	150	0.018	45	0.010	0.008
		(0.048)		(0.017)	(0.177)
Living in formal structures	150	0.812	45	0.756	0.055
		(0.330)		(0.352)	(0.181)
Own land	150	0.973	45	0.972	0.001
		(0.087)		(0.104)	(0.913)
Lives within five miles of primary school	150	0.570	45	0.561	0.009
		(0.477)		(0.479)	(0.878)
Lives within five miles of water source	150	0.913	45	0.909	0.005
		(0.258)		(0.265)	(0.886)
Owns a radio	150	0.446	45	0.421	0.026
		(0.284)		(0.288)	(0.473)
Owns a cell phone	150	0.300	45	0.289	0.011
		(0.253)		(0.255)	(0.722)
With formal roofs	150	0.584	45	0.511	0.074
		(0.350)		(0.341)	(0.092)
Average number of assets	150	1.646	45	1.650	-0.005
		(0.766)		(0.714)	(0.962)
Characteristics from Village Census					
Village population	150	197.253	45	203.222	-5.969
		(120.936)		(126.285)	(0.694)
Proportion of adults vaccinated at baseline	150	0.092	45	0.091	0.001
		(0.131)		(0.143)	(0.955)
HH head has had any schooling	150	0.184	45	0.165	0.018
		(0.117)		(0.109)	(0.208)
Owns land	149	0.647	45	0.623	0.025
		(0.214)		(0.228)	(0.354)
Reduced portion sizes in last week	149	0.391	45	0.364	0.027
		(0.221)		(0.219)	(0.329)
Age	150	22.458	45	22.210	0.248
		(2.639)		(2.481)	(0.453)
HH head is female	150	0.259	45	0.298	-0.039
		(0.119)		(0.110)	(0.009)
Is breastfeeding	150	0.133	45	0.134	-0.001
		(0.060)		(0.067)	(0.902)
Is pregnant	150	0.041	45	0.033	0.008
		(0.055)		(0.027)	(0.225)
Joint F-test p-value		0.668			

B. Extended Data Legends

Extended Data Legends

Table 1: Intent-to-Treat Effect of Door-to-Door and Small-Group Treatments

The table presents Intent-To-Treat estimates of the adult vaccination rate. Column 1 presents the adult vaccination rate for the sub-treatment arms. Treatment effects are estimated using OLS and include randomization block fixed effects (triplets) and heteroscedasticity-robust standard errors clustered at the village level. Column 2 adds covariates that were imbalanced at baseline. Column 3 presents the results of the individual Door-to-Door campaign where up to 20 structures were randomly assigned to be visited by the social mobilisation team. The sample is restricted to the 50 villages assigned to the Door-to-Door treatment arm, and non-peripheral structures within these villages. Treatment effects are estimated using OLS including randomization block fixed effects (villages), with heteroscedasticity-robust standard errors clustered at the structure (hut, house) level. The p-value in the bottom panel is from a two-sided t-test on the quality of means of both treatment arms. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level and refer to two-sided tests without multiple comparison adjustments.

Table 2: Baseline Descriptive Statistics and Statistical Balance

The table presents baseline data for the 150 study communities using 2015 census and the village census. Column (1) shows the mean and standard deviation for the control group. Columns (2) and (3) display regression coefficients and standard errors of the Door-to-Door and Small-Group treatment arms compared to the control group. Column (4) indicates differences between the two treatment arms. Column (5) shows the number of communities included in the regression. Regressions include randomization block fixed effects and heteroscedasticity-robust standard errors. All the measures are constructed from household level data aggregated to the community level. The last row reports p-values from two-sided Joint Orthogonality tests, from a multinomial logit regression with the treatment indicator as the dependent variable, regressed on all the variables in the table.

Table 3: Intent-to-treat Estimates of Vaccination Rate of People Enumerated During Census

This table presents Intent-To-Treat estimates corresponding to Figure 2. The dependent variable in Columns (1) and (2) is the individual level vaccination status at endline. Treatment effects are estimated using OLS including randomization fixed effects (ie for each

triplet) and heteroscedasticity-robust standard errors clustered at the community level. Included covariates in Column (2) are: the baseline adult vaccination rate; proportion of households employed in agriculture; proportion of households that own a radio; the proportion of women breastfeeding and proportion of households that own land. In Column (3), the dependent variable is the proportion of adults vaccinated in each community. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level and refer to two-sided tests without multiple comparison adjustments. Bootstrapped p-value is the p-value resulting from a wild bootstrap test of Pooled Treatment == 0, with 999 repetitions.

Table 4: Intent-To-Treat estimates of the Count of People Vaccinated per Site After Mobile Vaccination Program

This table presents Intent-To-Treat estimates corresponding to Figure 3. Treatment effects are estimated using OLS including randomization fixed effects (ie for each triplet) and heteroscedasticity-robust standard errors clustered at the community level. The dependent variable is the count of people vaccinated by the end of the study. In Column (1) we restrict our sample to those enumerated during the census. In Column (2) we add people who travelled from other communities for vaccination. In Column (3) we do not restrict our sample. Columns (4)-(6) add covariates imbalanced at baseline. Included covariates are: the baseline adult vaccination rate; proportion of households employed in agriculture; proportion of households that own a radio; the proportion of women breastfeeding and proportion of households that own land. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level and refer to two-sided tests without multiple comparison adjustments. Bootstrapped p-value is the p-value resulting from a wild bootstrap test of Pooled Treatment == 0, with 999 repetitions.

Table 5 Proportion Vaccinated by Baseline Willingness to Take Vaccines and Meeting Attendance

Each cell indicates the vaccination rate for adults surveyed in the baseline by whether they attended the village meeting crossed by whether they indicated if they were willing to take the COVID-19 vaccine during the course of the pre-meeting baseline survey.

Table 6 Intent-To-Treat estimates for Knowledge and Attitudes Towards Vaccines in Sub-sample

This table presents Intent-To-Treat estimates of the corresponding to Figure 4. The dependent variables are indicators of knowledge and attitudes of COVID and COVID vaccines, included in Figure 4. Treatment effects are estimated using OLS including randomization

fixed effects (ie for each triplet) and heteroscedasticity-robust standard errors clustered at the community level. Sub-sample comprises 45 villages and 817 households surveyed at endline. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level and refer to two-sided tests. Bootstrapped p-value is the p-value resulting from a wild bootstrap test of Pooled Treatment == 0, with 999 repetitions. FDR q-values are included to account for testing across several outcomes.

Table 7 Intent-To-Treat estimates for Which Source People Trust Most for Information on COVID-19 in Sub-sample

This table presents Intent-To-Treat estimates of the corresponding to Figure 4. The dependent variables are indicators for which source respondents trust for information relating to COVID-19, included in Figure 4. Treatment effects are estimated using OLS including randomization fixed effects (ie for each triplet) and heteroscedasticity-robust standard errors clustered at the community level. Trust indicators are constructed from the household level endline. Sub-sample comprises 45 villages and 817 households surveyed at endline. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level and refer to two-sided tests. Bootstrapped p-value is the p-value resulting from a wild bootstrap test of Pooled Treatment == 0, with 999 repetitions. FDR q-values are included to account for testing across several outcomes.

Table 8 Intent-To-Treat Estimates for Demographic Sub-groups

This table presents Intent-To-Treat estimates of the pooled treatment for demographic sub-groups included in Figure 5. Treatment effects are estimated using OLS including randomization fixed effects (ie for each triplet) and heteroscedasticity-robust standard errors clustered at the community level. Dependent variable is the vaccination status at the end of the study of adults enumerated during the census. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level and refer to two-sided tests without multiple comparison adjustments.

Table 9 Cost-Effectiveness Analysis

Authors' calculations based on implementation budget and financial reports from implementing partners

Table 10 Comparison of Full Sample to Sub-sample

This table presents baseline balance data for the 150 villages and the 45 villages in the

restricted sample used in Tables 6 and 7. Column (1) and (3) report the complete and restricted sample. Column (2) and (4) report the variable mean and standard error for each sample. Column (5) reports the mean difference. The last row reports p-values from Joint Orthogonality tests, from a multinomial logit regression with the treatment indicator as the dependent variable, regressed on all the variables in the table.