Access and Invitations: Increasing COVID-19 Vaccination in Kenya

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Abstract

We examine the impact of a vaccination campaign in Kenya that sent healthcare providers to homes inviting adults, with relatively proximate healthcare services, to get a COVID-19 vaccine nearby. The intervention increased the cumulative number of doses given by 8.7 per 100 people on the day of the intervention, equivalent to about a 10% increase over the baseline number of doses in the control group. The greater number of doses in the treatment group persisted in the 3 months following the intervention, indicating that the intervention induced people to get vaccinated who would not have done so otherwise. A machine learning analysis of heterogeneity reveals that treatment effects are largest among more disadvantaged groups — women, those with less income, and those with less education. To examine whether social image considerations influence vaccination behavior, we borrow a design from DellaVigna, List, and Malmendier 2012 and DellaVigna et al. 2016 used in the contexts of charitable giving and voting, and randomized an announcement of the home visit and vaccination offer ahead of time. This strategy allows those unwilling to be vaccinated to avoid the visit without facing the social repercussions of declining in-person. Contrary to expectations, there was no evidence that social pressure influenced vaccination. Instead, the announcement *increased* the probability of getting vaccinated by 3.8 percentage points, primarily driven by older participants who are at higher risk of severe disease. A cost-effectiveness exercise suggests that our intervention is comparable to other vaccination campaigns, at 34$ marginal cost per marginal dose.

1. Introduction

Despite being a vital prevention tool against common diseases and pandemics, vaccines often remain on shelves. In the case of COVID-19 vaccination, only 1 in 4 adults in low-income countries has completed a primary series (WHO, 2023). After initial vaccine supply constraints in low-income countries were resolved, people raised concerns over lack of motivation and vaccine skepticism (Chutel and Fisher 2021). While vaccine hesitancy has been proposed as a main contributor to low COVID-19 vaccination rates (Lazarus et al. 2022), this paper investigates a more fundamental barrier to vaccine take up – accessibility and convenience (Miguel and Mobarak 2022).

We evaluate an intervention in Kenya where healthcare providers visited homes inviting adults to get a COVID-19 vaccine nearby, and find that it significantly increased vaccine uptake. Building on evidence suggesting that increasing access to healthcare services in remote contexts can have large effects on uptake (Reza et al. 2022, Mobarak et al. 2022), this paper demonstrates that expanded accessibility also increases vaccine uptake in areas with relatively proximate healthcare services. We also examine which types of people are more affected by the intervention, finding that the intervention is the most effective among more disadvantaged groups in society. Furthermore, we investigate whether treatment effects are due to social pressure from healthcare providers to get vaccinated, finding no evidence of this. Finally, we measure the marginal cost per marginal dose administered and suggest future directions to decrease intervention costs.

In collaboration with the Kenyan government, we conducted a field experiment, between July and August 2022, sending healthcare providers to randomly selected homes inviting adults for vaccination at a nearby site in their community. At that time, Kenya was administering COVID-19 vaccine boosters. Despite individuals in our sample residing on average less than two kilometers away from a health facility offering COVID-19 vaccines, 50% had not received a single dose prior to the intervention. The intervention increased the number of COVID-19 vaccine doses delivered (of any kind - including first, second and booster doses) by 8.7 doses per 100 people on the day of the intervention, equivalent to a 10% increase over the baseline of 89.6 doses per 100 people in the control group. Note that 89.6 doses per 100 people does not correspond to a 89.6% vaccination rate among the control group because the cumulative number of doses is not evenly distributed across individuals. The greater number of cumulative doses in the treatment group persists in the 90-day post-intervention period, indicating that the intervention induces people to get vaccinated who would not have done so otherwise. Using machine learning techniques, we then identify which types of people are most responsive to treatment. Specifically, we find that the intervention had the largest effect among those more disadvantaged in society: women, those with less income, and those with less education. These observable predictors could be used by policymakers to inform where such outreach could be targeted.

Home visits by healthcare providers could increase take-up not due to convenience, but rather by exerting undue social pressure. Prior studies highlight the influence of social considerations on immunization (Karing 2023). Social norms around COVID-19 vaccination have varied by context and social group; it became politicized in the US and other countries (Rabb et al 2022, Agranov et al 2021). In Kenya, patients who receive a face-to-face invitation from a healthcare provider to get the COVID-19 vaccine could feel pressure to accept. Research outside of healthcare settings demonstrates social pressure drives decision making in cases of charitable giving (DellaVigna, List, and Malmendier 2012) and voting (DellaVigna et al. 2016), suggesting that any assessment of net benefits of door-to-door campaigns should consider the utility costs of saying no to the solicitor. Adopting this design for health behavior, where a social norm may emerge due to the positive externalities of vaccination, we examine the role of social pressure in individuals' vaccination decisions.

To investigate this question we included a second intervention where some individuals were informed about the home visit ahead of time. If individuals prefer to get vaccinated, the announcement of a home visit allows them to plan to be home and should increase vaccination rates, relative to the unannounced condition. If instead, individuals prefer not to be vaccinated and face some social cost of declining the invitation from the healthcare provider, then announcing the visit allows them to leave home or not answer the door to avoid getting vaccinated without incurring the social cost of saying no. In this case, vaccination rates should decrease relative to the unannounced condition.

Contrary to the hypothesis that people are getting vaccinated due to social pressure, announcing the visits increased the probability of getting vaccinated by 3.8 percentage points. This is consistent with individuals valuing the private benefits of vaccination. The effect of the announcement comes entirely from those 50 years and older, who are at higher risk of severe disease (Ngere et al. 2022). This further suggests that our results are driven by the private benefits of vaccination, rather than the social costs of saying no. This result is also policy relevant, as policymakers may aim to target those with a greater risk of severe disease. Announcing the program helped increase take-up among this sub-group.

This study shows that even in a context with relatively proximate healthcare services, making COVID-19 vaccination more convenient — by bringing vaccines to the local community and visiting individuals at their homes to invite them to get vaccinated — can substantially increase takeup. We do not find evidence that the treatment effects are due to social pressure from healthcare providers to get vaccinated. These findings have implications for future pandemics and routine immunization campaigns. Making immunization as convenient as possible may be crucial, particularly in cases when citizens may want to free ride on the vaccination behavior of others, may no longer see vaccination as an urgent need, or may value vaccination less than governments (Grossman, Phillips & Rosenzweig 2018). Finally, our cost-effectiveness exercise suggests that our intervention is comparable to other vaccination campaigns, at 34$ marginal cost per marginal dose.

2. Results of Home Visits

2.1 Experimental design and COVID-19 in Kenya

The experiment took place between July 12 and August 5, 2022, in one urban subcounty (Kisumu East) and one rural subcounty (Muhoroni) of Kisumu County in Kenya. In these localities, we randomly sampled 112 villages total, and in each village, we randomly selected which individuals (at most one per household) participated in the study. Each study participant was visited by an interviewer who conducted a household survey on the intervention day, prior to the home visit.

We randomly assigned participants with approximately equal probability to one of two treatment arms (announced or unannounced home visits) or to the control group, which did not receive a home visit (Fig S1).[1] Following the Kenyan National COVID-19 vaccine deployment policy (Ministry of Health Kenya, 2021), the intervention used government nurses to visit homes and deliver the vaccines at the nearby site. Nurses were the approved healthcare workers trained and permitted to administer the COVID-19 vaccines in Kenya at the time of the study. However, in future research or policy implementation, other individuals, such as community health volunteers, could be used to visit homes and make people aware of the opportunity to get vaccinated.

Experimental vaccination sites were set up to be a maximum of 15 minutes walking distance from households. In our sample the median distance from households to the vaccination site was 252 meters (mean 777 meters). Nurses were available at each site to administer COVID-19 vaccines for a single day. The nurses visited individuals

[1] In each village at the end of the intervention day we sent the nurses to 5 households that were not previously surveyed to also offer the vaccine. The purpose of this group was to understand rates of finding people at home without a prior appointment with an enumerator. We exclude these individuals from the main analysis since they were not part of the main experiment. However, since some individuals received a dose, we include those doses in the cost-effectiveness analysis.

assigned to the treatment groups at their homes to ask if they wanted the COVID-19 vaccine. If the respondent agreed, the nurse directed them to the nearby site and then administered the vaccine there. Nurses did not administer the vaccine at the respondent's door because the policy in Kenya at the time prohibited this, to minimize movement of the vaccines.

The study sample of 2,695 individuals consisted of 57% women, the average age of participants was 46 years, 40% had achieved at least a secondary education, and 44% had received at least one COVID-19 vaccine dose.[2] The average distance from sample households to the nearest health facility was 1.26 kilometers. The average distance to a health facility offering COVID-19 vaccines was 1.44 kilometers. Our intervention cut this distance roughly in half. Sample characteristics are balanced across treatment and control groups (Table S1).

Kenya was experiencing its sixth COVID-19 wave during the study period, but the number of daily infections was much lower than in previous waves. As of July 2022, Kenya had approximately 337,000 total confirmed cases of COVID-19, equivalent to about 6,231 cases per million people (Our World in Data), and 5,670 total deaths, or about 105 deaths per million people (WHO COVID-19 Dashboard).

The first COVID-19 vaccines were available in Kenya beginning in March 2021. Like other low and middle-income countries, Kenya faced initial vaccine supply constraints, which meant mass delivery was delayed. By the time of the study, four types of vaccines (AstraZeneca, Moderna, Johnson & Johnson and Pfizer) were available in the country. Vaccination was available at 81 health facilities in Kisumu County and local sites, such as community centers, markets and churches, during government-operated community vaccination days. At the time of the study, approximately 40% of adults in Kisumu County and 53% of adults in Kenya had received at least one COVID-19 dose (Ministry of Health, Kenya 2022).

In this context of COVID-19 in Kenya, we explore the impacts of improving convenient access and announcing home visits on vaccine uptake.

2.2 The effects of convenient vaccination sites and home visits

We find a statistically significant and meaningful difference in the cumulative number of COVID-19 vaccine doses received and the probability of getting an additional dose

[2] We first conducted a household listing and then randomly selected individuals to participate from that list. Of the 2,800 sampled individuals, 2,695 consented to participate in the study (attrition rate was 3.75%).

between the treatment and control groups, up to three months after the intervention. Fig.1 plots cumulative COVID-19 doses received by treatment assignment for a six-month period from 3 months (90 days) before the day of the intervention to 3 months (90 days) after. This measure includes both primary series and booster doses. As expected, there is no significant difference in the number of doses between the treatment and control groups in the pre-intervention period. On the day of the intervention, the treatment leads to an increase of 8.7 doses per 100 people (p-value of 0.033, associated with a t-test difference in means). This effect is equivalent to a 10% increase over the baseline of 89.6 doses per 100 people in the control group. Regression estimates including controls and village fixed effects have consistent results, presented in Table S2.

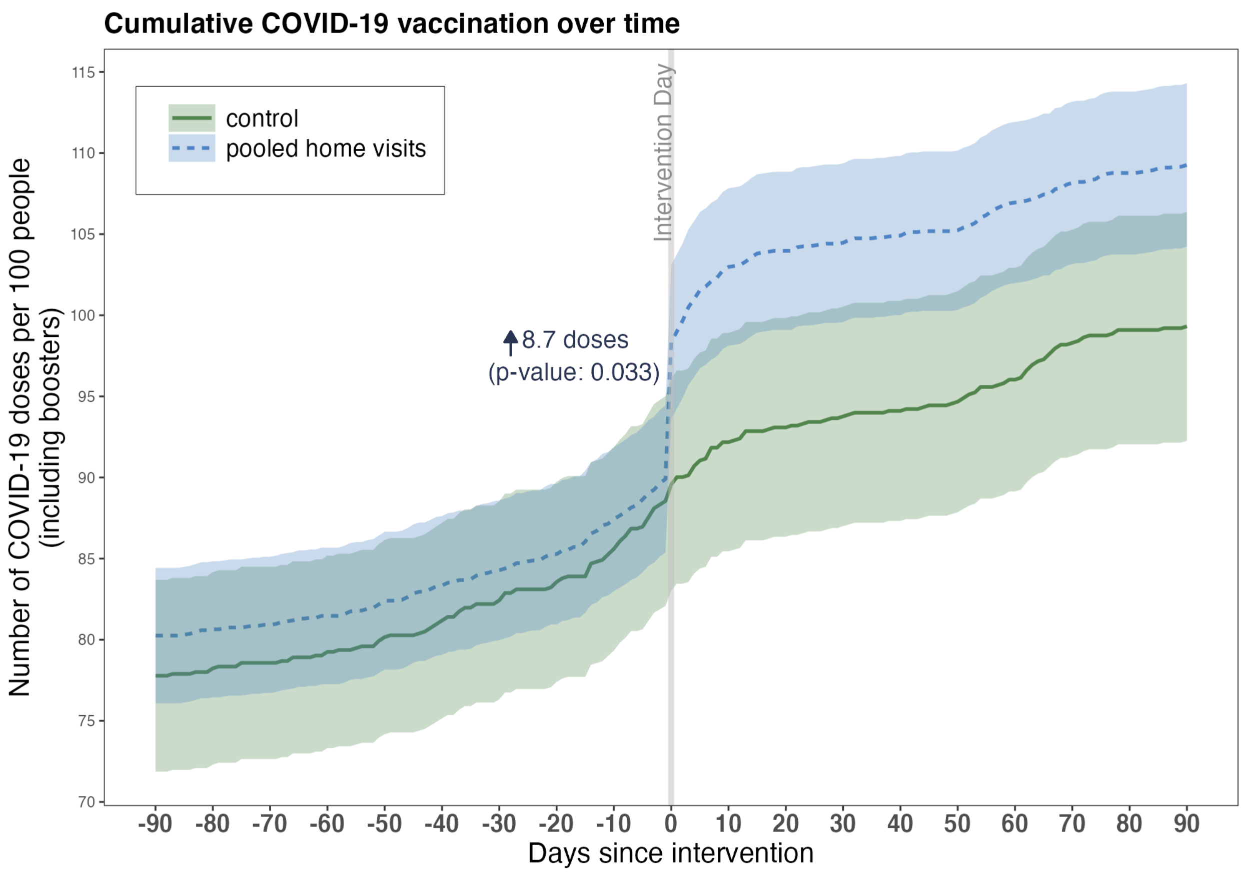


Fig 1 Time trends of COVID-19 cumulative vaccination over a six-month period, starting 3 months (90 days) before the intervention to 3 months (90 days) after. The intervention day, which occurred on different calendar dates in each village, is normalized and labeled as day “0”. Lines represent sample means – solid green for the control group and dashed blue line for the pooled home visits treatment groups. The shaded regions represent 95% confidence intervals around the sample means. Data source is the

MChanjo COVID-19 administrative system the Government of Kenya uses to track vaccinations.

The effect of the intervention persists for the full 3 month period analyzed following the intervention.[3] Specifically, seven days after the intervention, we observe an increase of 8.5 doses per 100 people in the treatment group, corresponding to a 11% increase over the control condition of 77.1 doses per 100 people. At 1 month and 3 months post intervention, the treatment group maintains a higher number of doses, with a difference of an additional 8.8 and 7.8 doses per 100 people, respectively, compared to the control group. This persistent gap indicates that the intervention does not simply displace vaccination rates across time for people who were planning to be vaccinated soon. Instead, it induces people to be vaccinated who would not otherwise have done so within 3 months, which is why we observe a persistent difference in cumulative vaccinations between the treatment and control groups in the post period.[4]

We also analyze the effect of the intervention on the probability of getting an additional dose and find that the intervention doubles the likelihood of getting vaccinated compared to the status quo. Using linear regression models, we find that the intervention increases the probability of getting vaccinated by 7-9 percentage points (Table S2). This percentage point increase corresponds to a 92.5% and 43% increase over the control group baselines at 1 week and 3 months after the intervention, respectively.

2.3 Who should policymakers target?

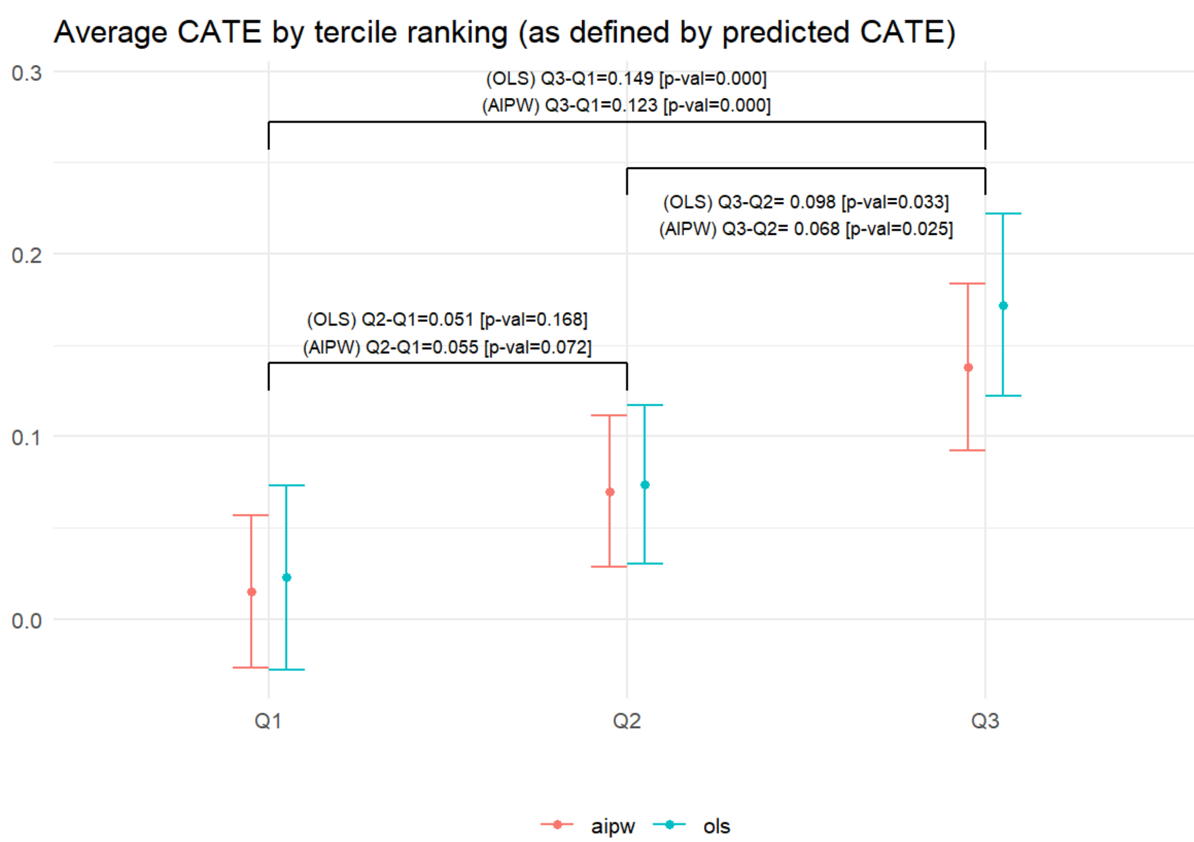
Using a data-driven machine learning approach, we examine which subgroups are more responsive to the home visits to understand which types of people might be best served by such an intervention. We estimate treatment effect heterogeneity using generalized random forest machine learning methods (Wagner and Athey 2018). We plot the estimated conditional average treatment effect (CATE) divided into tercile ranks from fitting a causal forest on a predefined vector of covariates (Fig.2, Panel A). The CATEs produced by the causal forest are sorted and used to split the sample into subgroups based on the magnitude of estimated impact on vaccination rates (up to 1 week after

[3] We preregistered analyzing the effect of the intervention on vaccination at one week, one month, and three months post intervention (AEARCTR-0009743).

[4] We use a time-dependent hazard model to estimate the hazard ratio of getting an additional dose comparing treatment to control before, during, and after the intervention. We find that the most notable impact from the intervention is around the intervention day (we consider 0-2 days to account for lags in recording vaccinations in the MChanjo dataset) and that the hazard ratio is statistically indistinguishable from 1 in the post-intervention period (Fig S3). This supports our interpretation that the intervention induces people to be vaccinated who would not otherwise have done so.

the experiment) on respondents in the treatment group (pooled home visits) relative to the control group. We use cross-fitting across folds so that we can generate ranks within each fold based on models fit on observations not in that fold. We estimate the CATE using both (1) simple difference-in-means estimates within each tercile produced by an Ordinary Least Square (OLS) model, and (2) Augmented Inverse Probability Weighting (AIPW) estimates. Both methods yield similar results. Estimates from OLS show a first tercile CATE (Q1) of a 2.3 percentage point increase (p-value=0.37), a second tercile CATE (Q2) of a 7.4 percentage point increase (p-value<0.001), and a third tercile CATE (Q3) of a 17.2 percentage point (p-value<0.001) increase in probability of getting vaccinated. The difference between the Q1 and Q3 CATEs is statistically significant at 1% level [p-value (OLS) < 0.001; p-value (AIPW) < 0.001]. The difference between the Q2 and Q3 CATEs is also statistically significant at the 5% level [p-value (OLS) = 0.033; p-value (AIPW) = 0.025]. These results hold also after adjusting for multiple hypothesis testing using the Romano-Wolf (2005) correction (Fig.3, Panel A).

Panel A



Panel B

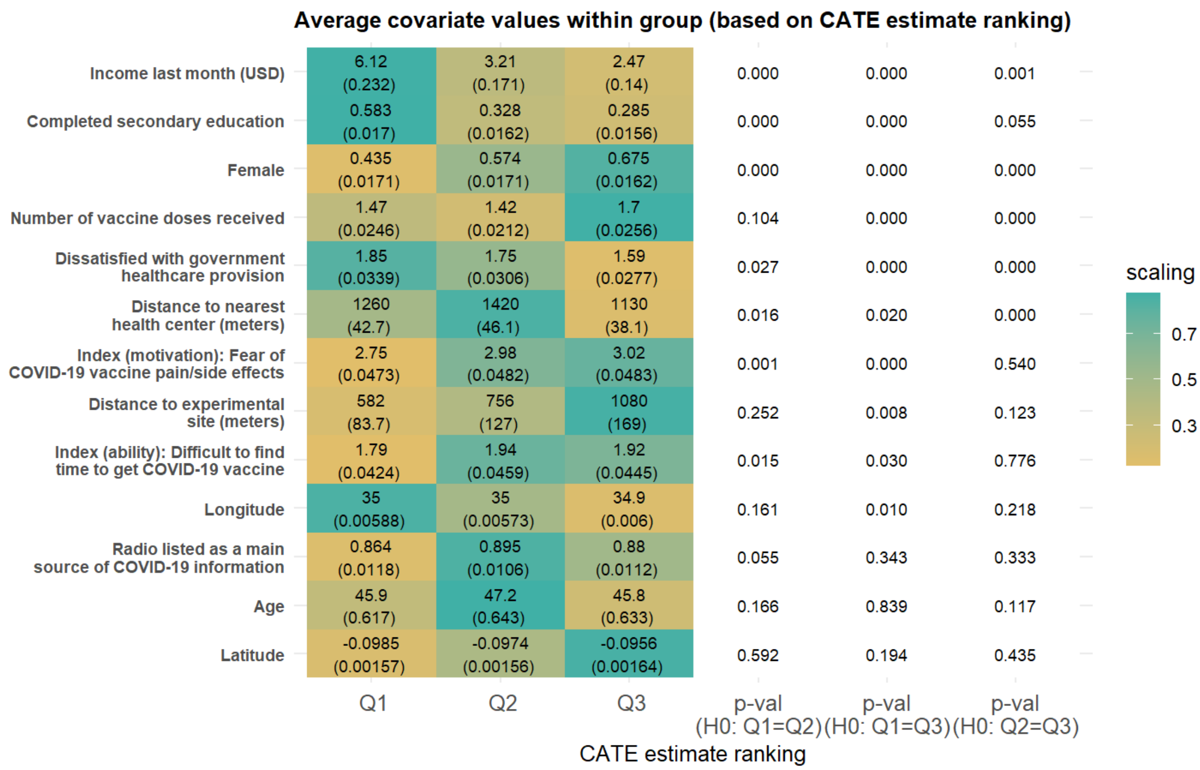


Fig 2: Conditional Average Treatment Effects (CATE) of home visits and average covariate values across terciles. Panel A plots the estimated conditional average treatment effect (CATE) divided into tercile ranks from fitting a causal forest (Wagner and Athey 2018). We estimate the CATE using difference-in-means estimates produced by an Ordinary Least Square (OLS) model and Augmented Inverse Probability Weighting (AIPW) estimates. Panel B describes characteristics of respondents for each CATE ranking by regressing the covariate on the CATE ranking using OLS. The covariates represent the full list included in the model and are ordered in the figure (from top to bottom) by the extent to which the CATE rankings explain the total variation in the covariate. The scaling of the grid cells represents deviations of the covariate value for a particular ranking standardized against the distribution of the covariate value for all rankings. P-values for the test of the differences between the terciles are also reported.

Analyzing the covariate profiles of each of the tercile groups, we find that respondents who are most affected by the intervention (the Q3 subgroup, which has the largest CATE), tend to be more disadvantaged in society compared to the other subgroups: the group is more likely to be female, have less income, and be less educated compared to both the Q1 and Q2 subgroups (Fig.2 Panel B). The Q3 relative to Q1 subgroup has a higher number of COVID-19 vaccine doses received, is also less dissatisfied with healthcare services generally, and closer to the nearest health center. This is also true for the Q3 relative to Q2 subgroup. In addition, the Q3 relative to Q1 subgroup is more fearful of side effects from the vaccine, perhaps suggesting they are more hesitant toward vaccination at baseline, yet this intervention was particularly effective in improving take up among them. Finally, the Q3 subgroup relative to Q1 subgroup is closer to the experimental site and reports being less able to find time to get vaccinated. Of note, in this analysis, we do not find any subgroup that we investigated for whom the treatment effect is negative.

In summary, we find that the intervention is most effective among more disadvantaged groups. Future interventions and policies may want to consider specifically targeting these subgroups, thus to more efficiently use resources. Furthermore, considering the risk of severe disease from COVID-19 for older age groups (Ngere et al. 2022) and that the intervention was just as effective for those age 50 and above (and more effective when home visits were announced, see Fig. 3 next), it may also be more efficient to target older individuals.

3. Results of Announcing Home Visits

While home visits may make vaccination more convenient, they could also increase vaccination by introducing social pressure due to the face-to-face interaction with a nurse. To test whether the observed increase in vaccination was driven or not by social pressure, about one-third of the sample was informed that a nurse would visit them in 30 minutes to ask if they wanted the COVID-19 vaccine, following the experimental design of DellaVigna, List, and Malmendier 2012 and DellaVigna et al. 2016. If people decide to get vaccinated because of the private benefits, the announcement should increase the probability of finding the respondent at home and vaccination rates. If instead, social pressure drives vaccination, the announcement should decrease the probability of finding the respondent at home and vaccination rates, as it allows people to avoid the nurse and the associated social costs of declining the vaccination offer.

Despite possible concerns about social pressure driving the effect of the visits, our results go in the opposite direction of the social pressure hypothesis: announcing the home visits increased vaccination by 3.8 percentage points (p-value=0.048), a 16% increase over the baseline rate of 23.1% in the unannounced condition. While the announcement increased the vaccination rate, it did not have a substantive effect on finding individuals at home (increase of 1.1 percentage points, p-value=0.569, Table S3, Panel A, column 1). This lack of nurse avoidance in the announced condition indicates that social pressure to accept the vaccine does not play a meaningful role. Any social pressure costs of declining the vaccine were not high enough to induce subjects to avoid the nurse. Furthermore, we observe a substantial share of respondents refusing the vaccine conditional on being home (72.2% of respondents were home, but only 31.9% accepted the vaccine offer, in the unannounced condition). If social pressure drives the effects of the home visit, it must be that the social costs of declining the vaccine are high. The observed lack of nurse avoidance, combined with the high rates of declining the vaccine, suggests low social costs of declining the vaccine. Therefore our results are inconsistent with our hypothesis of social pressure. Instead, the results support the idea that addressing convenience barriers can have a substantial impact on vaccination rate.

Using the same data-driven method described in Section 2.3 above, we also analyze treatment effect heterogeneity for announcing the home visit, compared to the unannounced condition (Fig S4). We do not observe statistically significant differences in estimated CATEs for the announced and unannounced groups. There is one tercile where we observe a negative CATE of -0.02 (p-value=0.58), but this is not statistically significant. Due to the small sample size, we cannot draw concrete conclusions from this analysis, as the estimates are noisy and the results are sensitive to specifications such as the randomization seed and number of folds used for *k*-fold validation to evaluate the prediction models.

These findings contrast with studies examining other non-health settings in which respondents were asked to undertake actions that provided benefits to society but did not provide private benefits, in particular, in the cases of charitable giving (DellaVigna, List, and Malmendier 2012) and voting (DellaVigna et al. 2016). COVID-19 vaccination differs from these actions because of its direct private benefits on individuals' health. Unlike in our setting, both of these studies find that door opening rates are lower in the announced condition, indicating avoidance of the visit due to social pressure. In our context of vaccinations, there is no evidence of this kind of avoidance of the visit, as the announcement does not change the probability of finding the respondent at home. This difference between our result and the previous literature highlights that the impact of social pressure likely depends on the relative importance of private and public benefits of an action.

To shed additional light on whether the private or public benefits of vaccination drive our results, we exploit the natural variation in private vaccination benefits that arise due to age. Because older people are at greater risk of severe COVID-19 (Starke et al. 2021), they also have greater private benefits to being vaccinated. If private motivations drive the effects of the intervention, then one would expect to see a larger increase in the announced condition among those with greater private benefits (older people). Indeed, when we disaggregate these results by age (below or equal/above 50 years old), we find that older respondents appear to be more responsive to the announcement compared to younger respondents (Fig.3).

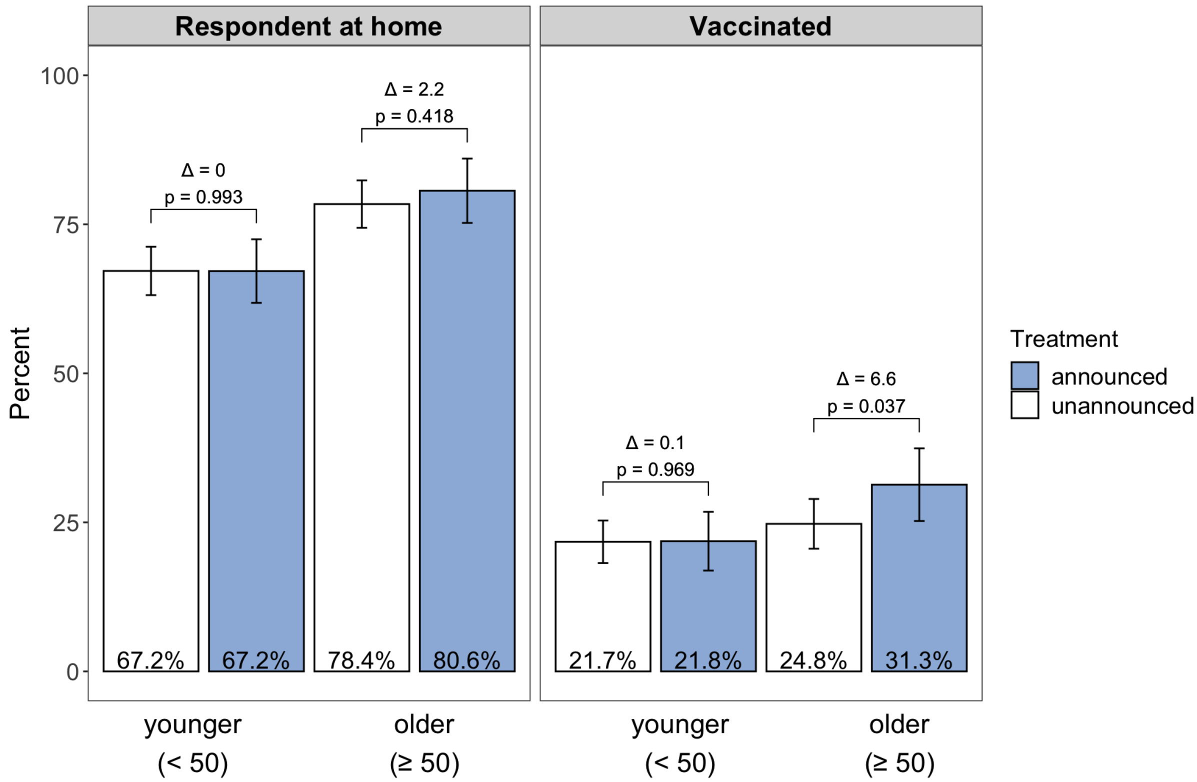


Fig 3: Rates of respondents being found at home and vaccinated by unannounced (white) and announced (blue) conditions for younger (<50 years old) and older (>=50 years old) respondents.

The announcement has no effect on respondents being home when the nurse arrives (we estimate a 2.2 percentage point increase that is statistically indistinguishable from zero, p-value = 0.418). Among those 50 and older, knowing ahead of time that the nurse is coming makes them 6.6 percentage points (p-value=0.037) more likely to get vaccinated. Among those younger than 50 years, we estimate near-zero treatment effects across both outcomes. These results are consistent with the hypothesis that those with the greatest private benefit from vaccination are more likely to respond to the announcement, demonstrating that announcing the visits can increase the intervention's effectiveness at reaching individuals at greater risk of severe disease.[5]

[5] It is noteworthy that among older respondents, the announcement significantly increased the whether the individual was vaccinated, but not the probability of being found at home. This could be due to some selection out (individuals who do not want to be vaccinated avoiding the nurse when announced) counterbalancing the selection in (individuals who do want to be vaccinated intentionally staying home for the nurse when announced), resulting in no significant difference in rates of finding the respondent at home, but higher probability of being vaccinated in the announced

Disaggregating the effects of the announcement by age is both theoretically motivated and policy relevant. Announcing the visit could be a policy tool to help better target vaccines toward older people. Furthermore, if private rather than social benefits drive the individual decision of being vaccinated, then we would expect the announcement to have the largest effect for older people, for whom the private benefit of COVID-19 vaccination is the largest, and this is what we find.

4. Cost Effectiveness

We conducted a cost-effectiveness analysis of the COVID-19 vaccination campaign conducted in our study. We measure cost-effectiveness as the marginal cost per marginal dose, calculated as the difference in costs divided by the difference in doses administered to individuals in treatment and control groups.

In terms of effectiveness, we use the number of doses administered during the study period as recorded either by the nurses visiting households and administering vaccines, or by the Kenyan national vaccination database (MChanjo) (Table S10). Our nurse data reported 501 doses administered, while MChanjo reported a similar number of doses one month after the intervention, suggesting some delays in recording doses into the national database and potentially MChanjo underestimating the total doses. As we do in our main analysis of the impact, we also use the different data sources (for doses delivered on the day of the intervention, and up to 1 week, 1 month and 3 months after the intervention) to estimate a range of cost-effectiveness.

In terms of costs, we recorded all costs from implementing our intervention in 112 villages, totaling $9,670 or approximately $86 per village (Table S11). This includes nurse and data officer wages, as well as their communication and their transportation to villages, and honoraria for community health volunteers (CHVs) local to these communities. We also include the costs of procuring and delivering one COVID-19 dose ($14.90) in Kenya, as estimated by Orangi et al. 2022a.[6] We exclude research costs.

condition due to this selection. An alternative interpretation is that knowing about the nurses visit ahead of time made individuals more likely to say they wanted the vaccine and actually take it, perhaps due to mental or physical preparation or having time to consult with others.

[6] We divided the total *economic costs* of procurement and delivery per person vaccinated with 2-doses by 2 to calculate the corresponding costs per one dose (see Table 5 in the article, 30% coverage scenario). In the articlecosts were also classified as either financial or economic costs. Financial costs only reflect the actual costs incurred by the government of Kenya for COVID-19 vaccines and its delivery. For instance, financial costs excluded the vaccine costs procured by COVAX facility and those donated through bilateral negotiations, salaries of all staff involved, volunteer costs, and development and cloud hosting of the electronic logistics management information system. On the other hand, economic costs

We calculated that the marginal cost per marginal dose administered varies between \$34.28 and \$83.48 on the day of the intervention. The latter estimate, however, could be higher due to the delays in recording the number of doses in the national database. By 3 months after the intervention, the marginal cost per marginal dose administered is \$41.76 (Table S13).

A similar study by Mobarak et al. (2022), which targeted last-mile COVID-19 vaccine delivery in Sierra Leone, estimated a cost of \$32 per person vaccinated, which is also considerably lower than other studies (see Figure 6 in Mobarak et al. 2022). However, it excludes the costs of procuring and delivering COVID-19 vaccines, which we included even though it was not part of our implementation costs. By excluding the costs of the doses, we estimate a marginal cost per marginal dose administered of \$19. Unlike Mobarak et al. 2022, we did not incur training costs for nurses who were already trained on vaccination, but we spent more on transportation as we used private vehicles to transport nurses to communities. As a benchmark, the WHO estimates that in 2020 the average cost of fully vaccinating a child under 24 months against 11 diseases (17 doses in total) is about \$58 (with a range of \$37 to \$101) in LMICs (UNICEF, 2020). Among other examples, the canonical paper testing vaccination camps without and with incentives to increase childhood immunization in Pakistan (Banerjee et al. 2010) calculates an average cost to fully immunize a child to be \$56 and \$28 for the two interventions, respectively.
Several potential avenues to reduce costs could be considered for implementing similar interventions (assuming the effectiveness remain the same, Table S12). First, we could use less skilled and less expensive healthcare providers, such as CHVs. While other healthcare providers might not be as trusted or well respected as nurses, there might also be some gains when building capacity in CHVs, who are already part of the community, as they could be employed to administer vaccines in future campaigns or other health-related activities. We assume CHVs to be paid at their current salary (which is about 37% of nurses' salary in Kenya), but we also added a one-day training to learn how to administer vaccines (at our research costs) including per diem and transport. Second, we assume that nurses could use cheaper forms of transportation such as motorcycles estimated on average 250 KSh per way, equivalent to about 4\$ both ways in July 2022. Finally, we assume a combination of the previous two scenarios. Table S12 shows how the marginal cost per marginal dose varies depending on the sources

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reflect the opportunity costs and covers the value of all resources used including those not captured in financial outlays by estimating their value. Therefore, economic costs included all financial costs as well as the value of donated vaccines, volunteer time, and staff salaries. As we consider economic costs, our calculations are quite conservative.

of data used for the number of doses administered. Both the utilization of CHVs or alternative transportation increases the cost-effectiveness of the intervention. By combining both scenarios, we find a marginal cost per marginal dose as low as $22.74.

Furthermore, policy at the time of the study dictated that we could not actually bring vaccines to each household, which is why we set up central vaccination locations in the community. Yet, it is possible that bringing doses to doors or asking health providers to reach a targeted number of households within the community, could help increase the effectiveness of the intervention. While it is hard to assume how many more doses health providers would be able to administered to be able to incorporate it in the cost-effectiveness analysis exercise, with improved cold storage or in cases where vaccine movement is less of a concern, policymakers may want to consider sending healthcare providers door-to-door to provide vaccination at home in an effort to make takeup even more convenient (Attwell, Hannah, and Leask 2022).

Finally, as discussed above and in line with results from cost-effectiveness analysis in Kenya (Orangi et al. 2022b), depending on the risk profile of the next pandemic, policymakers should consider subgroups to target with such interventions - for instance, disadvantaged groups who are less able to weather the health and economic shocks of a pandemic and may be less able to find opportunities to access health facilities or those with greater risk of severe disease such as older people in the case of COVID-19. While our findings show that our proposed mode of delivery has a meaningful impact on vaccine uptake, we argue that the variations to this intervention described above could be even more cost-effective.

5. Conclusion

Given the current unprecedented era of technological, demographic and climatic change leading to infectious disease outbreaks (Baker et al. 2022), the successful delivery and acceptance of new vaccines will likely remain a top priority for policymakers and healthcare providers across the globe (Siedner et al. 2022, Glennerster, Snyder, and Tan 2022).

While increasing vaccine access is especially critical for citizens living in remote areas where travel costs are high, this paper demonstrates that even in less remote settings where households are on average less than two kilometers away from health facilities, making vaccination more convenient and having a trusted healthcare provider (Moucheraud, Guo, and Macinko 2021) invite individuals to take it can increase uptake. Our findings show that home visits inviting individuals for vaccination at a nearby site can significantly increase the probability of getting an additional dose of the COVID-19 vaccine in Kenya, and is most effective among more disadvantaged groups in society. These findings offer insights into a mode of vaccine delivery that addresses both supply and demand barriers. Previous studies evaluated different methods of vaccine delivery to increase take-up addressing either only supply (Banerjee et al, 2010) or the demand side (Morris et al. 2004, Barham and Maluccio 2009, Karing 2019, Banerjee et al. 2019, Banerjee et al. 2021). Most similar to our study is Mobarak et al. 2022, which used mobile teams to bring COVID-19 vaccines to communities in Sierra Leone, and increased take-up by 20 percentage points.

This study contributes to the existing literature by showing that increasing the convenience of vaccine delivery with invitations by healthcare providers substantially increases vaccine takeup even in *less* remote areas. Furthermore, we find that announcing such visits ahead of time can further increase vaccine uptake among older individuals, and this does not result in undue social pressure. At $34 per dose administered, such an intervention has the potential to increase access to life-saving vaccines for a reasonable cost, in line with other vaccination campaigns.

Materials and Methods

4.1 Experimental Intervention

We conducted a randomized controlled experiment (RCT) to test the effect of sending nurses to households inviting them for vaccination at a nearby site on COVID-19 vaccination (Fig S1). 2695 survey respondents (one per household) across 112 villages were randomly assigned to one of three treatment conditions with roughly equal probability:

Unannounced Home Visits: Respondents were not told by enumerators at the end of the survey that the nurse was coming to offer them the COVID-19 vaccine. The nurse visited the household shortly after the end of the survey to offer the possibility of getting vaccinated at a nearby site.

Announced Home Visits: Respondents were told by enumerators at the end of the survey that a nurse was coming to their home in 30 minutes to offer them the opportunity to get vaccinated at a nearby site. The nurse visited the household approximately 30 minutes after the end of the survey.

No Home Visits. Respondents were not visited by the nurse after the survey. They could access vaccines at a local health facility or public area vaccination campaign, which is the status quo.

We first conducted a household survey designed to understand the determinants of COVID-19 vaccine acceptance. At the end of the survey, enumerators were provided a script to read to the respondent which corresponded to the respondents' treatment assignment. Enumerators were blinded to treatment assignment until the end of the survey. For respondents in *Unannounced* and *Announced Home Visits* conditions, at the end of the household survey, the enumerator team contacted the nurse team with the name of the household by sending an SMS with the details of the household. Nurses, who were Kisumu Department of Health employees, were instructed to conduct home visits not before 30 minutes elapsed after the household survey to offer the possibility of getting vaccinated at a nearby community site (Fig S2 presents the nurse visit survey form).

The 30-minute window was selected after piloting and consulting with the vaccination and survey implementation teams. Studies that pioneered this design of providing advance notice (DellaVigna, List, and Malmendier 2012 and DellaVigna et al. 2016) give a full 24 hours period between the announcement and the visit. Due to time and resource constraints we were unable to do that in our study.

Due to some logistical issues, in some cases nurses visited the home before 30 minutes had passed and in other cases much after 30 minutes (Fig S5). Given that the empirical predictions rely on respondents being aware of the time the nurse would visit their home (so they could plan to be unavailable if they so choose), we test for differences in the distribution of time of the nurse visits across treatment arms (Table S5) and conduct robustness checks among the sample of respondents who were visited more than 30 minutes (Table S6) and between 30 and 60 minutes (Table S7) after the survey. While there is a statistically significant difference in distributions between treatment arms, as given by non-parametric K-S tests (Table S5), this does not appear to lead to an economically significant difference in waiting times. Further, treatment effect estimates in Tables S6 and S7 are robust when conditioning on a minimum waiting time of more than 30 minutes or conditioning on a waiting time between 30 and 60 minutes.

The vaccine was administered at a nearby site within 15 minutes walking distance from the respondent's home. The location of the site was determined prior to the survey. The field team first met with a village elder to list several easily identifiable locations for the vaccination site and densely populated enough to sample respondents for the survey, such as close to markets, churches, schools. The field team then randomly selected one of these sites out of an average of 4 in each village. During the day of the survey, the nurse team set up their vehicle to administer the vaccines at the selected site. The nurses administered the vaccines at the nearby site rather than offering the vaccine at the respondent's door because the Kisumu Department of Health wanted to minimize excess movement of the vaccines. The Kisumu Department of Health's COVID-19 vaccination campaign standard protocols were followed when delivering the vaccine.

Nurses were not informed of whether their visit was announced or unannounced. Nurses were asked to visit the household, while the data officers stayed back at the vaccination site to guard the vaccines and medical equipment. If the respondent was home, the nurse offered her and any interested household members the opportunity to get vaccinated at the nearby site. If the respondent was not home, the nurse proceeded to the next household. Only if the respondent expressed wanting the vaccine, the nurse informed the respondent of the nearby site and that they could meet them there or walk with them there. Respondents who then showed up and got vaccinated were recorded in the national database (MChanjo) as well as in our study data by the data officers.

4.2 Sample

We conducted our survey and experiment in two sub-counties in Kisumu: Kisumu East (urban) and Muhoroni (rural), where full vaccination rates were 42% and 50%, respectively, as reported by the Kisumu Department of Health in February 2022, before the start of research activities.

Thirteen sub-locations were randomly selected in each sub-county, and then 4 villages were randomly selected per sub-location. A household listing survey was conducted in June 2022 to collect demographic information of members of at least 50 households in each village, sampled by a random walk around the selected community vaccination site. In July 2022, 25 respondents were then randomly sampled from the listing, each from a unique household, stratified on age and gender. Pregnant women were over-sampled for the survey because they are a vulnerable group who may be less likely to accept the COVID-19 vaccine owing to rumors about safety of the COVID-19 vaccine during pregnancy. We targeted half the sample to be over 50 years old, but the actual sample had a larger share of respondents under 50 (56%) due to misreported age during the household listing survey. The target sample of respondents was about 2800. 99.1% of them were interviewed between 12, July 2022 and 5, August 2022, while the rest were unavailable for the survey. Enumerators were instructed to survey 25 respondents per village per day over the 21 days. We were unable to find 17 people as they were present at their workplace during the survey. Additionally 4 could not be surveyed due to poor weather conditions, and another 4 were not available as they were attending political rallies in nearby areas. After conducting some data cleaning and keeping only valid entries, the final sample size for analysis was 2695 respondents (Table S4).

Prior to the household visits by nurses, all randomly sampled study participants were visited by a trained enumerator who conducted a 45-minute survey about their experience with and attitudes toward COVID-19. One of the criteria for the treatment was that the nurses visited the household at least 30 minutes after the conclusion of the enumerator survey. However, for about 2% of the respondents, the tablet was not set on the right time which resulted in a negative difference between the time of the nurse visiting the household and the enumerator survey. In 293 cases (16%), the difference was also higher than 60 minutes. Dropping the cases with negative differences, on average, nurse visits were conducted 45 minutes after the survey (min=0 minutes, max=269 minutes, SD=50 minutes). Table S6 and Table S7 show a subsample analysis of the main effects of nurse visiting the household, by whether the time between nurse visiting the household and enumerator survey to be more than 30 minutes or between 30-60 minutes respectively. Results are robust and statistically significant.

4.3 Data

In collaboration with a local research firm (REMIT), we conducted a household survey designed to understand the constraints individuals face in getting the COVID-19 vaccine and reasons why they may be hesitant to get vaccinated, with a focus on ability and motivation factors (Fogg 2009). The survey asked about (i) demographics; (ii) physical, routine or monetary constraints to accessing a vaccine (so called 'ability' barriers in the Fogg model); (iii) mental or social constraints, as well as lack of knowledge or misconceptions around the COVID-19 vaccine (so called 'motivation' barriers in the Fogg model); (iv) trust in health service providers; (v) attitudes around accessing healthcare and vaccine-seeking behavior; (vi) knowledge and experience with COVID-19 and the vaccine; (vii) perceptions and attitudes about the COVID-19 vaccine and policies; (vii) perceptions of social norms around the COVID-19 vaccine. The survey finally asked (ix) self-reported past vaccination status and future intentions.

During the household visit, the nurses were asked to fill in a paper survey form (see Fig S2) which was later digitally entered by data officers using SurveyCTO at the nearby site. They recorded whether the respondent was home or not. If the respondent was home, they also recorded whether they expressed wanting the vaccine and whether they showed up to the site and got vaccinated. We use these outcomes on stages in the respondent's interaction with the nurse to estimate the impact and explore mechanisms of the differences in behavior between treatment groups (announced and announced home visits).

The nurse team also recorded information, uploaded to the national vaccination database (called MChanjo), on respondent vaccination eligibility, type of dose, and date of dose, along with demographic information. COVID-19 vaccination records from the national database contains vaccination records of all individuals vaccinated either at the health facility or during governmental community vaccination campaigns. We worked with 13 data collection officers at the Kisumu County Government Ministry of Health to match our respondents to the records, based on national ID, phone number, name, age and gender to individuals. We considered individuals matched to the databases if (i) national ID and phone number matched; (ii) name and phone number matched, and the age difference was mostly lower than 5 years (main worry is individuals with the same name sharing a phone number within the household); (iii) name matched, and national ID, age and phone numbers were similar enough. Ultimately, 53% matched. This data was collected up until three months after the end of our survey (December 2022).

The sample (2695) consisted of 56.5% women, 4.9% pregnant at the time of the interview. On average respondents were 46 years old, 98% were Catholic, 89.3% were from the Luo ethnicity, and 40% completed secondary education or higher. Before COVID-19 hit, their income was 7790 KES (about 77$) per month. 44% of the sample had received at least one COVID-19 vaccine dose prior to the intervention. The average distance to the closest health center was 1.26 km. Characteristics are balanced across treatment and control groups (Table S1).

4.4 Estimation strategy

The random assignment of interventions across respondents allows us to identify the causal effect of (announced or unannounced) home visits on vaccination outcomes. We estimate effects using ordinary least squares (OLS):

(1) $Y_{iv} = \alpha + \beta * \mathit{[Pooled]}_{iv} + \delta X_{iv} + v + \varepsilon_{iv}$
(2) $Y_{iv} = \alpha + \beta 1 * \mathit{[Announced]}_{iv} + \beta 2 * \mathit{[Unannounced]}_{iv} + \delta X_{iv} + v + \varepsilon_{iv}$

where Y is the primary vaccination outcome of interest for respondent i living in village v. To capture vaccination intentions and behavior we use a combination of household survey data, administrative vaccination records from the nurses, and administrative data from the Kisumu Department of Health. In the pooled analysis (equation 1), our primary outcome of interest is whether individual i received an additional vaccine dose within the 3 months after the survey. When we explore the impact of announcing the home visits (equation 2), we further explore whether individual *i* opened the door when the nurse arrived; whether she wanted the vaccine when the nurse offered it; whether she showed up at the close vaccination site to get the vaccine; and whether the vaccine was administered by the nurse to individual *i.*

[Pooled] is an indicator for whether the individual i received any nurse visit; *[Unannounced]* is an indicator for whether the individual i received an unannounced nurse visit; *[Announced]* is an indicator for whether the individual i received an announced nurse visit; the comparison group *[No Home Visits]* refers to the status quo and include individuals who did not receive any home visit after the survey, and they could access vaccines at a local health facility or in a public vaccination campaign.

The empirical model also controls for a number of predetermined observables, X, which include whether the respondent was female, and whether she was pregnant; whether the respondent was 50 years or older; distance to the closest health center (meters), pre-treatment cumulative number of doses or past self-reported vaccination status. We control for differences across villages by including fixed effects (*v,* 112). Standard errors are clustered at the village level. Robustness checks include the following covariates: whether the respondent was female; pregnant; age, age^squared; distance to the closest health center (mt), whether completed secondary education or higher, whether married, income prior to COVID-19; distance from the experiment site (mt); and, past self-reported vaccination status (Tables S8 and S9).

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