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Evaluating the impact of interventions to improve full immunisation rates in Haryana, India

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Executive summary

Immunisation is a highly cost-effective way of improving child survival. However, full immunisation rates are low in India. In the state of Haryana, only 52.1 per cent of children aged between 12 to 24 months are fully immunised (IIPS n.d.), and rates decline rapidly after the initial vaccines in the immunisation schedule (e.g. Bacillus Calmette–Guérin, first dose of pentavalent) to the final vaccines (e.g. third dose of pentavalent and measles).

This programme built a common information structure leveraging m-Health software. This was used to evaluate the impact of several interventions aiming to increase demand for immunisation, namely: incentives, targeted reminders and the leverage of social networks in 140 primary health centres across seven districts of Haryana.

Working closely with government officials, the programme developed a new m-Health app for tablets and integrated this into the existing government programme for immunisation. Front-line health workers then used the app and recorded details of every child who was immunised at any immunisation camp in the sample primary health centres. There were three major interventions, cross-randomised at different levels:

1. Small incentives in the form of mobile credit were provided to caregivers each time they brought their child to a vaccination camp, in half of the primary health centres. The incentives were delivered to the mobile phone number provided at the time of immunisation. Incentives varied in their levels, and in whether they increased over the course of the immunisation schedule, depending on the sub-centre;
2. Targeted text and voice call reminders were sent to caregivers to remind them it was time for their child to receive a specific shot. These were provided for 0 per cent, 33 per cent or 66 per cent of eligible children, depending on the sub-centre; and
3. Key people or 'seeds' in the community social network were identified to spread information about immunisation. A rapid survey was conducted to obtain from villages the names of people generally good at transmitting information ('gossip'), people who are generally trusted ('trusted'), or people generally good at transmitting information and trusted ('trusted gossip'). During the programme, the study sent monthly text messages and voice calls to one of these people, or to a randomly selected volunteer (randomisation was at the village level), to inform their fellow villagers of the importance of immunisation.

Monitoring data show that implementation of the programme was largely successful, especially considering the scale of operations (across 140 PHCs, 755 sub-centres and 2,360 villages, and with over 295,000 unique children registered in the database). Data entered into the tablets by front-line health workers were generally of high quality – there were almost no incidences of fake child records, and the child's name, date of birth and vaccines entered were accurate over 80 per cent of the time.

Delivery rates of recharges were also high – they remained above 90 per cent throughout the programme. Delivery rates of text message reminders and seed messages were around 80 per cent for the duration of the programme, which is the industry standard.

The results of the programme show that incentives in the form of mobile credit had an overall positive impact on full immunisation rates when administered on a sloped schedule, where the amount of mobile phone credit was higher for the final two vaccines a child should receive in their first year. We see positive and significant effects in the social network experiment, when gossips are chosen as volunteers to relay information. The effects of the reminder messages were relatively disappointing, with small effects found only in the sub-centres where 33 per cent of people received reminders.

We provide a range of cost-effectiveness calculations. The cost-effectiveness analysis shows that leveraging gossip seeds to spread information was a highly cost-effective intervention, with a ratio of USD5 per additional fully immunised child. For incentives, the low slope treatment arm was more cost-effective than the high slope treatment arm (USD61 per additional fully immunised child compared to USD93 per additional fully immunised child for the high slope). Considering that incentives are a transfer, and valuing them as a marginal cost of public funds (using the rate of 20%), the cost per additional fully immunised child was USD32 when low slope incentives were provided.

On balance, this programme demonstrated that demand-side interventions to promote immunisation are feasible at scale, and can be useful complements to developing infrastructure, particularly m-Health infrastructure, to improve access to immunisation.

In terms of pure fiscal costs, the most cost-effective intervention by far was the social communication intervention, which relied on community knowledge to identify good potential relays for immunisation messages. Our study demonstrated that this can be done easily and effectively through a rapid survey of a few households in the population. Small incentives (with a sloped schedule) carry a larger extra cost, but when the cost of incentives is counted for as a transfer, they are also cost-effective.

The effect of targeted reminders – perhaps the most obvious intervention for a government intending to roll out an m-Health tablet programme – is limited by the fact that only children who have attended at least one session can be enrolled in the programme, so it could only affect the intensive margin. Here we find small and inconsistent effects.

Immediate next steps for exploring post-project policy influence include meeting with senior officials of the Haryana government, and to offer them support in considering how to integrate either a low-slope incentives scheme or a gossip scheme into its health programming and budget for the state.

Contents

List of figures and tables

Appendix figures and tables

Abbreviations and acronyms

ANM	Auxiliary nurse midwife
ANMOL	Auxiliary nurse midwife online
ASHA	Accredited social health activist
BCG	Bacillus Calmette–Guérin
CEA	Cost-effectiveness analysis
DIO	District immunisation officer
DLHS	District-level household survey
EPI	Expanded Program on Immunization
INR	Indian rupee
J-PAL	Abdul Latif Jameel Poverty Action Lab
MCP	Mother and child protection
MO	Medical officer
Penta 1,2,3	Pentavalent vaccine doses 1, 2 and 3
PHC	Primary health centre
SC	Sub-centre
TPK	Teekakaran Protsahan Karyakram

1. Introduction

Immunisation is one of the most cost-effective ways to improve child health and survival in developing countries. Yet, yearly it is estimated that approximately 19.7 million infants worldwide are still not covered by routine immunisation services (WHO 2020). In the Indian state of Haryana alone, the district-level household survey 4 (DLHS-4) showed that there are at least 224,000 children aged between 0–12 months who are not on track to be fully immunised (IIPS n.d.).

In middle-income countries, conditional cash transfers have successfully stimulated demand for immunisation by making transfers conditional on receiving vaccinations. In contrast, standard solutions for improving immunisation coverage in resource-poor settings address supply-side issues in vaccine delivery or leverage intensive door-to-door campaigns.

Our baseline data, collected in 2016, indicates that coverage rates in seven districts in Haryana are much higher for vaccines at the beginning of the immunisation schedule. A total of 94% of children received at least two vaccines, whereas 86%, 65% and 40% of children receive the third, fourth and fifth vaccines, respectively. Higher rates for the initial vaccines suggest that the main barrier is driven neither by a supply side problem nor a deep-seated resistance to immunisation (which may be caused, for example, due to religious or personal beliefs).

In this programme, we address the research and policy gap on the efficacy and optimal design of demand-side solutions with a set of three interventions: provision of small incentives to caregivers, a community intervention that leverages communication through social networks, and an information campaign conducted through phone calls and text message reminders. To our knowledge, this is the first large-scale evaluation of such a programme. The programme was implemented in collaboration with the state government across seven districts comprising 2,359 villages in the state of Haryana, India.

To address the causes of demand-side issues and determine possible policy response, we supported the government to implement three interventions. The first is based on the idea that small incentives can be effective in offsetting small costs, thereby ‘nudging’ a mother who may not have strong views about immunising her child to overcome procrastination, or compensating for low importance given to vaccination in the community.

This project builds on previous research of the authors, which demonstrate the cost-effectiveness of introducing small incentives for immunisation, albeit in a small-scale setting (Banerjee et al. 2010). In this programme, caregivers of children who visit an immunisation session camp and receive one of the five key vaccines receive mobile phone credit. This happens after the vaccine is administered and the immunisation details of the child are entered in the m-Health app used by the front-line health worker.

The second and third experiments, cross-cut with the incentives experiment and each other, were designed to leverage various communication strategies. Both of these experiments address the issues of misunderstanding and decreased salience, as outlined above.

The second experiment tests the role of village social networks. Prior work by the authors (Banerjee et al. 2013) found that community members with a higher degree of centrality (who are more connected) are more effective at spreading information in their network. These data were based on a unique dataset that maps social networks in 43 villages across Karnataka.

Further research (Banerjee et al. 2014) showed that when communities are asked to nominate individuals they think spread information ('gossips'), they tend to choose those with a high degree of centrality, and these individuals are also effective 'seeds'. Motivated by this evidence, community members in our programme were asked to nominate individuals from their village they consider to be good at spreading information ('gossips'), trustworthy in the village, or a combination of the two. These nominated seeds were asked (through monthly voice calls and text messages) to spread the information they receive about immunisation to other community members.

The third experiment – targeted reminders – leverages the digital data collection system we developed to evaluate the impact of targeted text messages and voice calls that provide reminders and information. In a subset of the sample, the text and voice call reminders were used to send information aiming to correct misconceptions about immunisation. For example, the message reminding parents to send their child for the measles vaccine in some instances reminded them that the vaccination is for a disease for which they have not yet received immunisation.

Furthermore, we have randomly varied the fraction of individuals within a village that receives reminders, thereby enabling us to identify spillover effects. To our knowledge, this is the first large-scale test on the use of text message reminders for immunisation, and the first to attempt to distinguish reminders from information and identify spillover effects.

Section 2 describes the intervention employed, the underlying theory of change and research hypotheses. Section 3 outlines the context in which the programme is implemented, and Section 4 provides a timeline of the study. Section 5 reviews the evaluation design and Section 6 focuses on implementation details. Section 7 presents the impact analysis and Section 8 discusses findings. Last, Section 9 touches on the main policy implications of the study.

2. Intervention, theory of change and research hypotheses

2.1 Intervention

2.1.1 Incentives experiment

Incentives were provided for each visit to an immunisation camp conducted by a front-line nurse (known as an auxiliary nurse midwife [ANM]) in the 70 incentive primary health centres (PHCs). After each visit, the caregiver automatically received mobile credit (talk time) on the phone number they provided at the immunisation camp. The mobile credit recharge was triggered through an app on the ANM's tablet at each immunisation, and the caregiver also received a notification of the amount transferred.

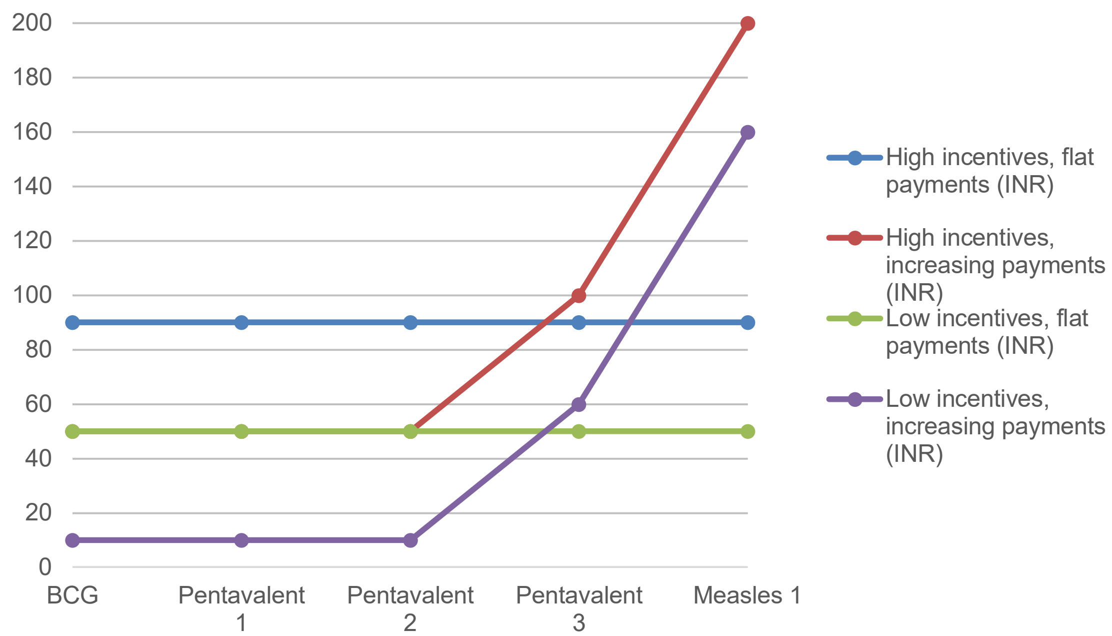
The incentive amounts varied at the health sub-centre (SC) level. Depending on the SC the caregiver fell under, (s)he would receive either:

- *A high-incentive, flat payment* of INR90 per immunisation (INR450 total);
- *A high-incentive, increasing (sloped) payment* of INR50 for the first three immunisations, 100 for the fourth and 200 for the fifth (INR450 total);
- *A low-incentive, flat payment* of INR50 per payment (INR250 total); or
- *A low-incentive, increasing (sloped) payment* of INR10 for the first three immunisations, 60 for the fourth and 160 for the fifth (INR250 total).

Figure 1 provides a graphic depiction of the incentive amounts. The incentive levels were chosen to be small and therefore implementable at scale, while still being a non-trivial amount for the households: INR50 corresponds to 100 minutes of talk time on average. Note that the provision of incentives was independent across vaccines in the treatment group: if a child missed a vaccine (e.g. the first dose of the pentavalent vaccine [penta-1]) and came to receive the next (in this case, measles), they still received an incentive for measles.

Information about the incentives was disseminated in several ways. ANMs and accredited social health activists (ASHAs) informed potential beneficiaries of the incentive structure and amount in the relevant villages. Every immunisation camp also displayed a poster describing the same information. ASHAs did not have any further involvement in the study, aside from awareness of the programme and having been asked by the ANM to disseminate information.

Figure 1: Schedule of incentives



2.1.2 Communication experiment

In each of the villages assigned to one of the non-random seed groups (gossip, trusted people, and trusted gossips), we selected seeds by asking 17 households randomly sampled from the census to nominate four individuals matching one of the descriptions below (see Section 5.3 for details on the nominations survey), depending on the village's treatment assignment.

Gossip seed
Gossip seeds are people expected to be good at diffusing information in their village. We used the script below to determine who they were.

'Who are the people in this village, who, when they share information, many people in the village get to know about it. For example, if they share information about a music festival, street play, fair in this village, or movie shooting, many people would learn about it. This is because they have a wide network of friends and contacts in the village and they can use that to actively spread information to many villagers. Could you name four such individuals, male or female, that live in the village (within OR outside your neighbourhood in the village) who, when they say something, many people get to know?'

Trust seed
Trust seeds are people relied upon in the village, and any information coming from them will be received with confidence. Information on health services, immunisation in this case, tends to be more sensitive than general gossip. We used the script below to determine who they were.

'Who are the people in this village that you and many villagers trust, both within and outside this neighbourhood? When I say trust, I mean that when they give advice on something, many people believe that it is correct, and tend to follow it. This could be advice on anything like choosing the right fertiliser for your crops or keeping your child healthy. Could you name four such individuals, male or female, who live in the village (within *or* outside your neighbourhood in the village) and are trusted?'

Trusted gossip seed
This person is an intersection of trust and gossip seeds, who is trusted in the village and effective at spreading information. We used the script below to determine who they were.

'Who are the people in this village, both within and outside this neighbourhood, who when they share information, many people in the village get to know about it? For example, if they share information about a music festival, street play, fair in this village, or movie shooting, many people would learn about it. This is because they have a wide network of friends and contacts in the village and can use that to actively spread information to many villagers.

Among these people, who are the people that you and many villagers trust? When I say trust, I mean that when they give advice on something, many people believe that it is correct and tend to follow it. This could be advice on anything like choosing the right fertiliser for your crops or keeping your child healthy. Could you name four such individuals, male or female, that live in the village (within *or* outside your neighbourhood in the village) who, when they say something, many people get to know, and they are trusted by you and other villagers?'

Random seed
In this treatment arm, randomly selected individuals from the village census are provided with the same information as in the other treatment arms.

Respondents were also asked to provide demographic characteristics and identifying information for each of their nominees. The nominations were matched and aggregated by village and the top six nominated individuals in each village were selected to be seeds.

Throughout the course of our intervention, seeds received monthly reminders and calls asking them to disseminate information about immunisation.

2.1.3 Reminders experiment

The reminders experiment was designed in two levels in order to identify spillovers. First, we randomised the fraction of the SC population receiving targeted reminders into three groups: no reminders, 33 per cent reminders, and 66 per cent reminders. Second, the targeted reminders treatment arms were generated at the individual level, according to the saturation level assigned to that SC in the first step.

The following text reminders were sent to beneficiaries eligible to receive reminders:

Reminders in incentive treatment PHCs
Hello! It is time to get the ______vaccine administered for your child_. Please visit your nearest immunisation camp to get this vaccine and protect your child from diseases. You will receive mobile credit worth __________ [RANGE FOR SLOPE/ FIXED AMOUNT FOR FLAT] as a reward for immunising your child.

Reminders in incentive control PHCs
Hello! It is time to get the ______vaccine administered for your child. Please visit your nearest immunisation camp to get this vaccine and protect your child from diseases.

The randomisation was done on a rolling basis, where the treatment status for each child visiting a session camp for the first time since the launch of the programme is assigned to receive or not to receive reminders as soon as their record is added to our database. This receipt of reminders is conditional upon visiting a session camp for the previous vaccine.

All interventions were implemented between December 2016–April 2018. However, due to a technical glitch, the implementation of the targeted reminders intervention was discontinued from November 2017. Thus, the analysis for the targeted reminders experiment is based on the data collected until this date.

Note that all participants of the study – those who attended a session camp in our sample – received a congratulatory text message on the registered mobile phone. For all text messages that were sent as part of the study, a corresponding voice call was also sent. This contained exactly the same information and was sent to ensure the inclusion of illiterate participants.

2.2 Theory of change

The theory of change describes the linkages from inputs to outputs and from outputs to outcomes for all three experiments in the study. For an illustration of the theory of change, please refer to Figure 2 for incentives, Figure 3 for communication experiment and Figure 4 for targeted reminders. For the theory of change to hold, there are a number of assumptions that must be met, as described in detail in each figure.

2.2.1 Incentives

This evaluation draws on a previous study the authors conducted in Udaipur, Rajasthan, which found that vaccination uptake drops drastically after penta-2, and that incentives had a large and significant impact on improving immunisation outcomes, especially for penta-3 and measles-1. These findings were the rationale used to conduct a follow-up evaluation at scale as an effectiveness trial.

Given that the baseline findings for this study showed the same drop-off pattern in vaccine uptake as Udaipur and beliefs and attitudes pose little resistance toward immunisation, we had a sufficient foundation to support our view that the same challenge underlies our theory of change, though in a slightly different context and at a larger scale.

It is important to note that while the Udaipur study had in-kind incentives, we administered quasi-cash incentives in the form of mobile recharges. This method was chosen in order to leverage the increasing acceptability of using Aadhaar numbers as an identification mechanism, the penetration of mobiles, and the acceptability of direct beneficiary transfers (though these were not in cash). We tested the feasibility, acceptability and perceived value of this approach through focus groups prior to launching the intervention.

2.2.2 Communications

This evaluation also draws on existing literature concerning leveraging networks and the formative research by the authors in Karnataka as described in the introduction (Section 1). The existing evidence shows that people who have a high degree of centrality in a network are able to spread information more widely in a community. The authors' formative work has suggested that these people can be identified relatively easily by directly asking community members to identify the best 'diffuser' in the area. An interesting question was whether the communication experiment in this study could achieve an impact when implemented at scale.

2.2.3 Reminders

This evaluation draws on the extensive body of evidence concerning reminders delivered to mobile phones, which shows that they increase the salience of numerous behaviours (such as savings or taking medication) across many different contexts. Again, the drop-off in immunisation rates found in our baseline, and the fact that attitudes and beliefs were not found to pose much resistance to vaccination, supported our theory that low salience leads to lower uptake.

Figure 2: Theory of change diagram – incentives experiment

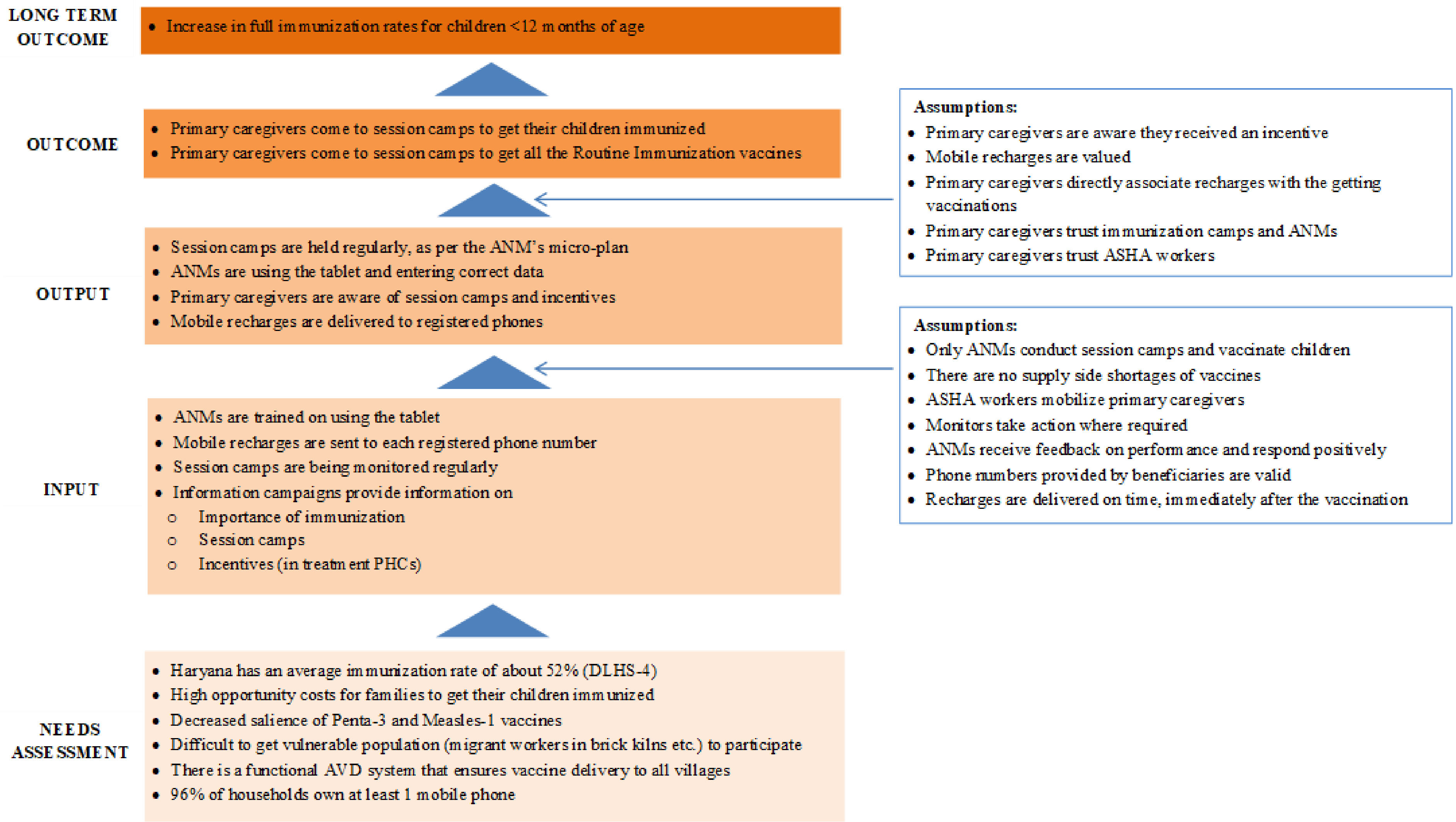


Figure 3: Theory of change diagram – communications experiment

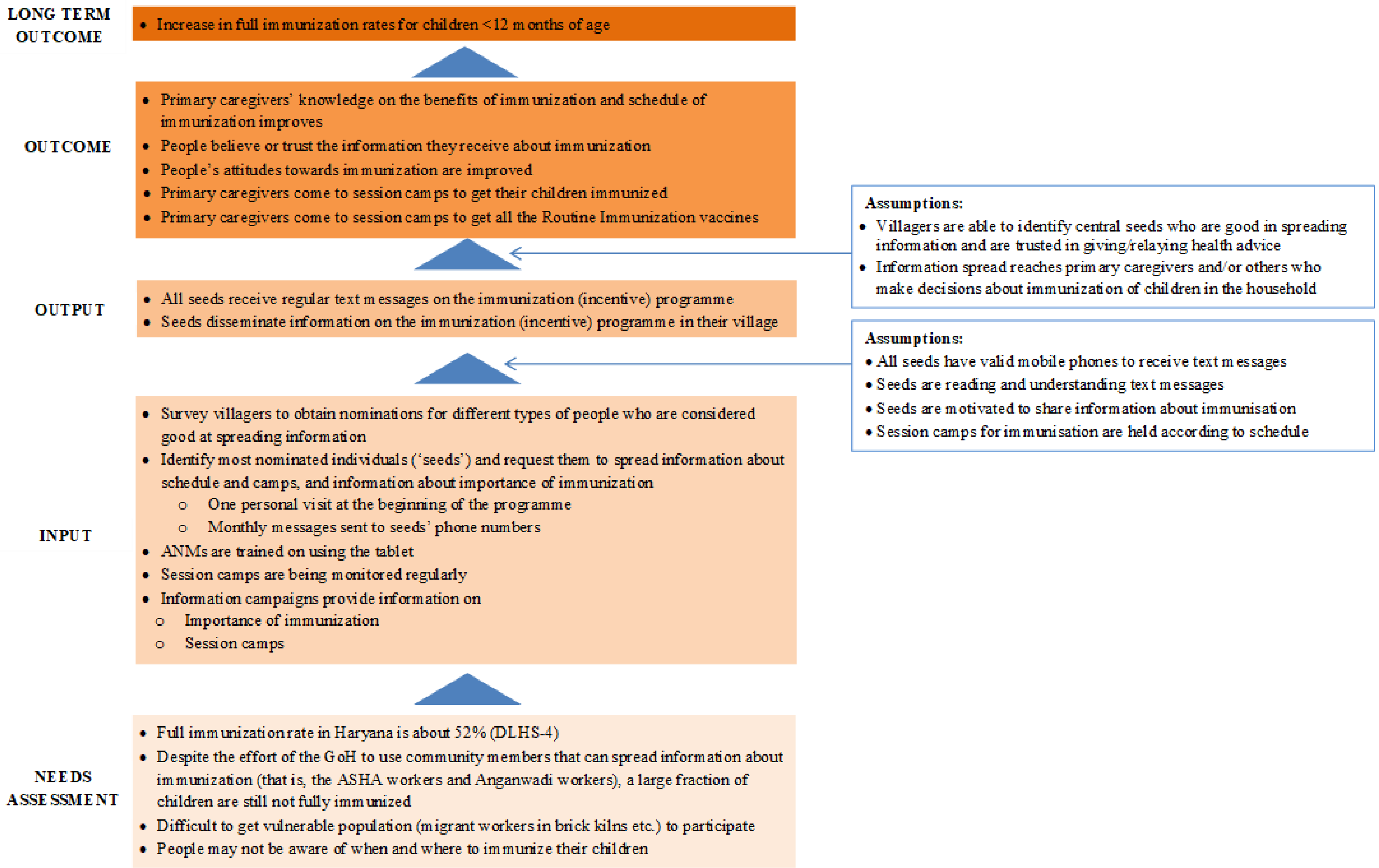
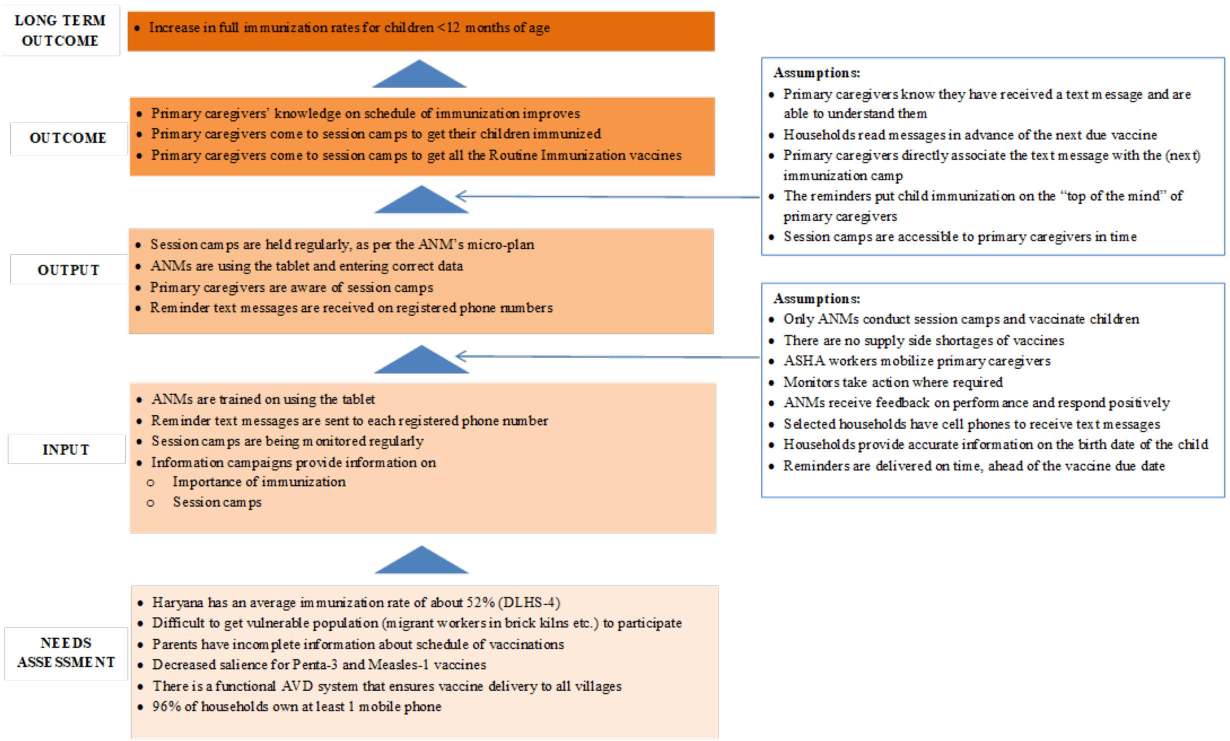


Figure 4: Theory of change diagram - targeted reminders experiment



Notes: we define 'primary caregiver' as the person who takes primary responsibility of, and knows most about, the child's health (self-defined). The theory of change assumes that the primary caregiver is a main stakeholder in the decision to take their child to the immunisation camp each time. It may be the case that other members of the family or the household also have decision-making power. The theory of change assumes that, while there might be differences in knowledge and beliefs around immunisation at an individual level, within households the ultimate decision to take the child to an immunisation camp is unified at the family level. In part, this will be affected by power structures within households, especially gendered power relations.

The mobile phone registered at the immunisation camp may not be that of the primary caregiver (or the person who took the child to the camp, if different). The theory of change assumes that, even if this is the case, the recharges or text message reminders can reach the caregiver or decision makers in the family.

2.3 Outcomes of interest

2.3.1 Primary outcomes

To assess the effect of the programme, we focus our analysis on the following three outcomes, similar to Banerjee and colleagues (2010):

1. Number of vaccines received per child;
2. Full immunisation status – the vaccine package administered in this study is the WHO/UNICEF Expanded Program on Immunization (EPI), which is the package provided by the Indian government. For children, the EPI includes 1 dose of the Bacillus Calmette–Guérin (BCG) vaccine, 3 doses of the penta vaccine, 3 doses of the oral polio vaccine, and 1 dose of the measles vaccine; and
3. Immunisation status for individual vaccines – penta doses 1, 2, 3 and measles.

2.3.2 Secondary outcomes

Secondary outcomes include changes in beliefs, attitudes and knowledge; prevalence of mother and child protection (MCP) vaccine record cards; adherence to a vaccine schedule (as defined by the percentage of prescribed vaccines received on time), and adherence to vaccines due later in the schedule.

2.3.3 Hypotheses

We hypothesised that the overall effect of incentives will be positive on all outcomes, with smaller effects on vaccines earlier in the schedule. For the communications experiment, the dissemination of information through any community-nominated seed will have a positive effect relative to having no seed or a randomly selected seed. For the targeted reminders experiment, we hypothesised that receiving reminders positively affects immunisation outcomes.

3. Context

This study was implemented across seven districts in Haryana where immunisation rates are particularly low, with full age-appropriate immunisation coverage ranging from 27.3–62.5% ((IIPS n.d.). From each of these districts, 140 PHCs were selected to be part of the study, covering a total of 2,360 villages. PHCs are health facilities that serve an average of 25 rural and semi-urban villages with approximately 500 households each.

Under each PHC are approximately four SCs. Each has at least one female ANM who is responsible for administering vaccines to all children in her coverage areas, either at her SC or in the villages in her catchment area every Wednesday (the routine immunisation day throughout the country). For each immunisation session, the ANM receives a supply of required vaccinations from her SC's umbrella PHC.

ANMs also supervise village-based ASHAs, who serve as a bridge between mothers and SCs. ASHAs are mobilised to spread awareness, provide information, and identify beneficiaries from their villages and channel them towards the health facility.

The public healthcare infrastructure in Haryana delivers services through a network of 60 hospitals, 124 community health centres, 500 PHCs and 2,630 SCs. The infant mortality rate in Haryana was 33 per 1,000 live births in 2016. (Department of Economic and Statistical Analysis, 2018) The DLHS-4 survey of 33,772 households in Haryana reported that 77 per cent of pregnant women had an institutional delivery during the reference period between DLHS-3 (2007–2008) and DLHS-4. Table 1 provides statistics from the DLHS-4 survey.

According to DLHS-4, full immunisation coverage[1] among children (aged 12–23 months) in the state was 52.1%, with 51% coverage for rural areas. There was a decline in coverage from DLHS-3, when full immunisation coverage among children was 60%. Only eight districts recorded full immunisation coverage over 60%. The highest coverage was 74% in the district of Kaithal, and lowest in the district of Mewat at 27.4%. The districts of Faridabad, Mewat, Panipat and Sonipat have full immunisation coverage lower than 40%. The percentage of children (aged 12–23 months) in rural areas who have not received any vaccination stood at 6.7%.

In India, a child is considered to be fully immunised if (s)he receives the following vaccinations in the first 12 months after birth: 1 dose of BCG, 4 doses of oral polio, 3 doses of penta, 3 doses of rotavirus, and 1 dose of measles. This is completed in 5 visits: BCG is typically given at birth, and then the child receives penta doses 1, 2 and 3 at 6 weeks, 10 weeks and 14 weeks, respectively. Finally, the first dose of the measles vaccine is given at 9 months. In this study, each of these visits was considered a 'shot' for which a primary caregiver was eligible to receive an incentive.

We also conducted a baseline survey in our seven sample districts (Section 5.3). Appendix B provides a summary of the demographic characteristics of the sample.

Table 1: Population characteristics of sample population

Indicators | Districts	Bhiwani	Jhajjar	Palwal	Panipat	Mewat	Rewari	Sonipat
Percentage of population literate age 7+ years	72.2	79.0	67.0	77.5	61.8	80.8	79.3
Sex ratio at birth (males per 100 females)	109	116	109	113	109	118	112
Mean age at marriage for boys (marriages that occurred during the reference period*)	19.7	20.6	19.6	20.2	18.7	20.9	20.0
Institutional delivery (%) (for live/stillbirth during reference period*)	75.1	90.2	42.2	52.9	40.3	87.5	70.3
Sampled number of children (children aged 12–23 months)	54	82	60	64	61	22	88
Percentage of children who received full vaccination**	49.0	48.6	35.2	50.0	20.8	30.0	32.9
Households surveyed	**687**	**1189**	**668**	**858**	**677**	**673**	**1137**

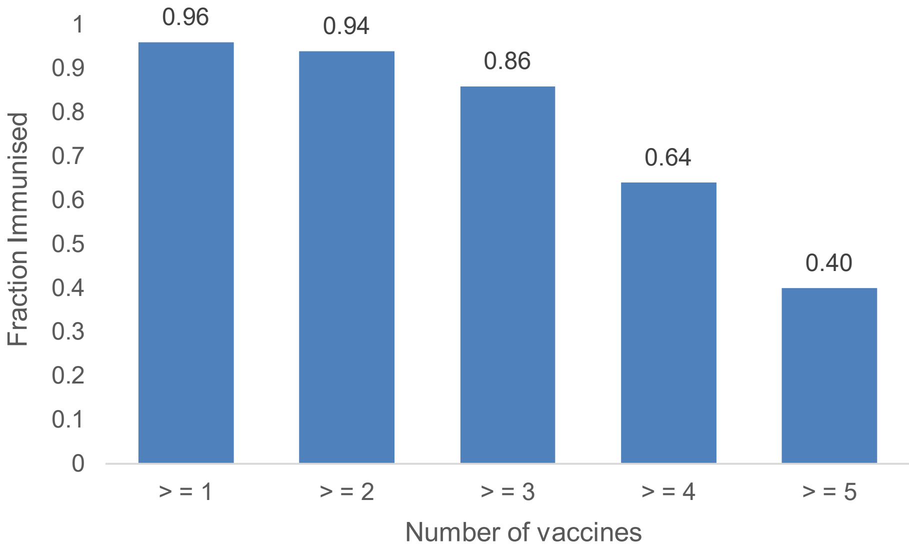
Notes: * the reference period is from January 2008 to survey date (survey in Haryana conducted between June and Dec 2013).**full vaccination is defined as having received 1 dose of BCG, 4 doses of the oral polio vaccine, 3 doses of penta, 3 doses of rotavirus and 1 dose of measles by the age of 12 months.
Source: (IIPS n.d.).

[1] Full immunisation coverage comprises BCG, three doses of diphtheria, tetanus and pertussis, and polio and measles vaccines.

In terms of immunisation rates, Figure 5 shows that the prevalence of vaccine shots is close to universal, as about 96 per cent of the children have had at least one vaccine. There is high attrition, however, as children progress through the immunisation course, particularly after the third and fourth vaccines. Approximately 86% of the children receive at least three vaccines, but the number drops to 64% for the fourth vaccine, and further down to 40% for the fifth vaccine.

This immunisation pattern is consistent with the view that beneficiaries do not have a fundamental opposition to vaccination, given that most beneficiaries have allowed their children to receive at least one vaccine. It is important to understand why parents fail to adhere to the immunisation schedule as their children age. Only 40 per cent of children in the sample receive all routine vaccines, although almost all of them receive at least one vaccine. In addition to this, only 19 per cent of the children receive the five vaccines as per the prescribed schedule, i.e. before 12 months of age.

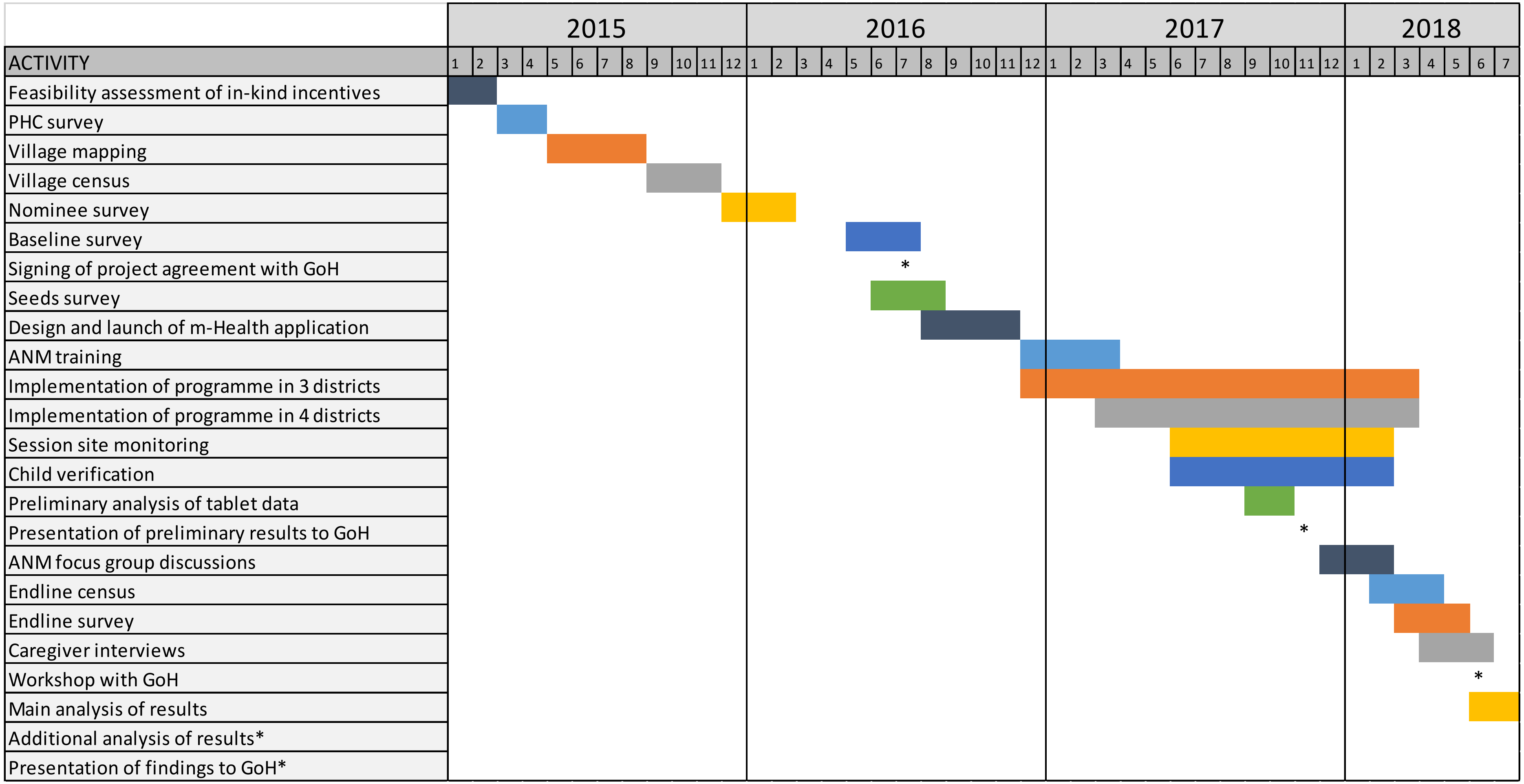
Figure 5: Prevalence of vaccines from baseline survey



4. Timeline

Initial conversations for this project with key stakeholders in the Haryana government began in 2012. However, it was not until January 2015 when the study activities began, facilitated by the USAID grant received on June 2014 and the 3ie grant received in April 2016. Figure 6 below shows the timeline of this project from the start of 2015.

Figure 6: Project timeline



Notes: GoH = Haryana government. * = still to be completed.

5. Evaluation design and methods

5.1 Treatment assignment

To estimate the impact of the incentives, communication and reminders programmes (and cross-programme effects), we used a randomised controlled trial design, where all interventions were cross-randomised. We selected the seven most poorly performing districts in terms of immunisation rates from the DLHS-4 survey. From these seven districts, 140 PHCs were selected for the programme based on the availability of a cold-chain facility.

The presence or absence of incentives was randomised at the PHC level, where 70 PHCs were randomly assigned to receive incentives and 70 PHCs were randomly assigned not to receive incentives. Randomisation was performed at the PHC level for administrative simplicity – ANMs are supervised by a doctor at the PHC, and collect vaccines and other supplies from PHCs – and to avoid spillovers between the treatment and the control groups. Each PHC has an average of 25 villages with about 500 households each, and PHCs tend to be separated by several kilometres.

In the treatment group, the level and slope of the incentives were varied at the PHC level to allow for identification of the most effective package of incentives. There was very limited scope for error or manipulation of the incentive type at the individual level, as the process was centrally administered and automatised.

The communications experiment was cross-randomised with the incentive experiment at the village level. We randomly selected 529 of our sample villages to receive one of four communications interventions (gossip, trust, trusted gossip, random) and randomised the sub-treatment arms to which they belonged.

The reminders experiment was similarly cross-randomised with the incentive treatment at the SC level. We randomised in two steps (stratified by treatment status) in order to identify spillovers. First, we randomised the fraction of the SC population into three groups: no reminders, 33% reminders and 66% reminders. In our study sample, 711 villages receive no reminders, in 886 villages 33% of individuals receive reminders, and in the remaining 793 villages 66% of individuals receive reminders.

Within each village, the targeted reminders treatment arms were randomly assigned at the individual level, according to the saturation level assigned to that village in the first step. Randomisation was performed on a rolling basis, wherein the treatment status for each child visiting a session camp for the first time since the programme launch is assigned to receive or not receive reminders as soon as their record is added to our database. Children only receive reminders on the condition that they visited a session camp for the previous vaccine.

For both the communication experiment and the reminders experiment, there were treatment arms without incentives as well as a pure control treatment arm (no incentives, no reminders, no seeds).

For all three experiments, the random assignment of treatments was conducted by the principal investigators and research associates in the office using Stata® version 12 before the start of the programme. The assignment of participants to receive or not receive individual-level reminders in the 33% and 66% treatment arms was done on a rolling basis as soon a child was registered on our database using a Python script.

Appendix C explains the evaluation design using a schematic diagram.[2]

5.2 Data collection

5.2.1 Survey data

Over the course of the project, we conducted multiple survey exercises. This section describes these activities and features common to all the exercises. Table 2 below summarises the data collection activities.

Table 2: Overview of survey data collection activities

Data collection activity	Survey sample	Timeline	Purpose
PHC survey	161 PHCs across 7 districts	March–April 2015	Sampling PHCs for the study
Census for baseline	328,058 households in 912 sample villages	September–November 2015	Identifying sampling frame for baseline survey
Baseline survey	17,000 children in 912 sample villages	May–July 2016	Collecting baseline data
Nominations survey	15,504 households in 912 sample villages	December 2015–February 2016	Identifying and sampling seeds
Seeds survey	2,117 nominated respondents	June–August 2016	Obtaining consent and information on seeds
New census	~120,000 households in 200 sample villages	February–April 2018	Identifying sampling frame for endline survey
Revised endline survey	4,000 children in 200 sample villages	May–June 2018	Collecting outcome data

PHC survey

This was a facility-based survey conducted to obtain information on staff and infrastructure availability, the current set-up of the vaccine delivery system and its monitoring, and the magnitude of population served by each PHC and associated SCs.

The PHC survey included 161 PHCs across the seven sample districts, selecting a final sample of 140 PHCs. There were a total of 154 PHCs on the list provided by the National Health Mission. Based on the reconciliation of the PHC list from the PHC survey and the list we received from the Mission, we conducted surveys in seven additional PHCs, bringing the total to 161 PHCs.

[2] Note that this diagram was created prior to the baseline and PHC survey. After these surveys were conducted, it transpired that some villages in our sample had moved PHCs and therefore out of our sample, and they were then dropped prior to the implementation of the programme.

A key criterion we used to determine the eligibility for sampling PHCs was that a PHC should not receive vaccines from a different PHC. This served as a proxy for sampling PHCs with an onsite cold storage capacity. In Haryana, only PHCs with cold storage units are considered as planning units. The delivery of vaccines, reporting and fund disbursal is restricted to such PHCs. PHCs that did not have cold storage units reported to the closest cold storage PHC, and all planning was also delegated to cold storage PHCs. (In essence, non-cold storage PHCs function more like SCs than autonomous PHCs.)

We found that 151 PHCs met this criterion. In addition, given the vast geographical spread of the PHCs in Bhiwani, 11 PHCs that were closest to the Rajasthan state border were dropped for logistical reasons, namely to reduce costs of data collection and implementation.

In order to sample villages for the survey, the first step was to assign *dhanis*[3] to their parent villages. Villages with fewer than 100 households were dropped, as they did not meet the eligibility criterion[4] for the household sampling. Villages with more than 1,000 households were split into multiple units to generate a weighted sample. Following this decision, 207 villages were split. In addition, villages with more than 3,000 households were dropped for logistical reasons.

Through this process, we arrived at a final list of villages for sampling. For the baseline survey, seven village units were randomly selected from each of the 140 PHCs. In all, 912 villages were sampled for the baseline survey. The number of village units in each district varies with the size of the district.

Census for baseline

A census survey was conducted to identify eligible households[5] (those with one or more children between 12–36 months of age[6]) in sample villages in order to sample them for the baseline household surveys. A detailed mapping exercise of all the villages in the sample was conducted to estimate the number of dwellings within the village boundaries and to help track households during the census. A total of 328,058 households were visited as a part of the census exercise over a period of three months. Out of these, 62,548 households met the eligibility criterion.

Baseline survey

The 62,548 households from the census that fit our eligibility criterion constituted our sampling frame, from which we sampled households for the baseline survey. Fifteen households[7] from each of the 970 village units (912 villages) were randomly selected for the baseline survey.[8]

[3] Settlements with a very small population, usually an appendix of a bigger village.

[4] Fifteen households with children between 12 to 36 months of age.

[5] A household comprises people who have lived in the same house for at least 30 days in the past year, eat food cooked from the same stove, and contribute to and share household income.

[6] This age range was chosen because these children had completed the immunisation cycle, and understanding their immunisation history provides a reasonable estimate for village-wise baseline immunisation rates.

[7] Fifteen households in each village unit were also randomly selected as replacements to be used if a household in the main sample could not be interviewed for any reason.

[8] In PHCs where fewer than seven village units were sampled, a higher number of households from each village were sampled to maintain the proportional sampling of household across all PHCs.

The baseline survey entailed an extensive questionnaire, which contained eight modules of various questions on vaccines administered to children and their mothers, knowledge and attitudes towards immunisation, the government's text message programme, and health practices for all eligible children in sampled households.

Responses about household assets, income, education and other demographic information were preferred to come from the head[9] of the household, who are best placed to answer such questions, whereas questions on vaccines and the immunisation status of the children were compulsorily answered by the child's primary caregiver.[10] In total, 14,670 households with 17,000 children were surveyed during the baseline exercise.

Nominations survey
A specific set of questions were used to collect nominations for the different types of seeds in the communications experiment. The questions were similar to those used in Banerjee and colleagues (2014) and other gossip studies; however, they were adapted to suit the local context of Haryana.

The survey was administered after a series of nine well-planned pilots, each with a new variation, were conducted in Haryana. The nominations from these pilots were carefully studied to arrive at the final questions, for which it was necessary to understand the problems and trade-offs faced by each variation of the instrument. For example, we learned that an earlier version of the gossip question suffered from 'formal bias', as people only nominated officials. To address this, the subsequent pilot added a variation to address the issue.

For the nominations survey, 17 households in every sample village were randomly selected from the census conducted in these villages. A short survey was conducted in these households, wherein the respondent was asked to nominate four persons they thought would best fit the description in the question asked. Additional information was collected to help us identify unique nominees and verify overlaps.

Seeds survey
The seeds survey was conducted for the following reasons: (1) to ask the nominated nodes for consent to be a part of the experiment; (2) to collect phone numbers of the recruited seeds to send them information via text messages and recorded phone calls; and (3) to collect other demographic information of the recruited seeds.

Both in-person and telephone surveys were conducted to ensure that we recruited as many seeds of total nominated nodes generated from the algorithm. Four days after the baseline surveys were completed in a particular district, the seeds survey began (following two days' rest and two days of refresher training). A linear decision rule could not be established because seeds are probabilistic, and their identifying information comes from third-person nomination. Thus, the enumerators were trained to use their discretion in cases where only a few characteristics matched, or in cases where there was more than one match. If two people were equally likely to be a seed, the

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[9] The head of household is the primary decision maker, mainly regarding finances. This is usually the eldest male member of the household.
[10] The person primarily responsible for taking care of the child and (in this case) who takes the child to be immunised. In our sample this was usually the mother or grandmother of the child.

enumerators tossed a coin and randomly chose one. There was no scope for replacements except when a seed did not exist in the village.

A total of 2,601 seeds (approximately 79%) consented to be part of the programme and receive information to disseminate in the village. Of these, 2,117 consented for a survey to collect information on their demographics, among other topics.

New census

Using the information of the child verification survey conducted during implementation monitoring, we found that the quality of the administrative data collected by ANMs on the tablets was very good. (Specifically, we were able to physically locate 91% of the children we sampled from the names ANMs entered into the platform.) Given the high quality and comprehensiveness of the administrative data (which surpassed what would be possible to collect through a household survey), we decided to use the tablet data as the primary source of data for analysis purposes.

However, one limitation of the tablet data is that they only capture children that came to the session camps, and not those that may have been immunised elsewhere (such as in an SC, PHC or vaccination camp outside of our sample). Hence, to establish a denominator for the fraction of all the children observed in the tablet data, we conducted a second door-to-door census in a subset of villages.

The census survey identified 18,963 eligible children (those below the age of 24 months, or all children 12 months or below when implementation began) across 200 villages in the seven sample districts. Of these, 18.8% (2,015) were from Bhiwani, 9.6% (1,811) from Jhajjar, 33.2% (6,298) from Mewat, 13.9% (2,635) from Palwal, 9.6% (1,811) from Panipat, 10.9% (2,065) from Rewari and 12.1% (2,292) from Sonipat.

Revised endline survey

While the administrative data are our primary outcome dataset, we wanted to determine the extent to which children not included in our tablet data were immunised, and if there are any significant differences between them and the children in our tablet data. Towards this goal, we first established a match rate with the tablet data using the information collected during the census. We matched children based on the barcode on their immunisation cards. In practice, a child found with a barcode would be 'in the system'. However, a third category emerged from those cases, wherein the households could not produce their immunisation card during the survey to verify the barcode status.

We identified that 47.2% of the children were matched (part of our tablet data), 21.1% were in the uncertain category (could not produce their immunisation card at the time of the survey), and 31.9% were cases that did not have a barcode on their immunisation cards and were considered 'outside our system'.

Of these 18,963 children identified during the census, we sampled 4,000 (accounting for a 25% replacement rate) by district and match status. In-person surveys were conducted for these 4,000 children, collecting immunisation history for unmatched children; we also included a module on caregivers' knowledge and attitude towards immunisation, including their sources of immunisation-related information. In villages that are part of the network experiment, we were able to match sources of information against our list of seeds.

Since the survey was conducted during the summer months and fell during Ramzan, we faced a high replacement rate, completing 3,200 surveys in total.

Digital data collection
We employed complete digital data collection as it has significant advantages over paper-based surveys. It makes the survey exercise much simpler, as complicated skip patterns become automated and there are multiple answer pre-filling options. It also provides better validations and checks, as responses can be constrained beforehand. Real-time checking of data – to provide feedback to survey teams and facilitate immediate reconciliation of errors – is particularly useful if the survey is being conducted on a large scale, like the present exercise.

Instruments
Please refer to Appendix D and sub-appendixes D1–D7 for the instruments used in each of these surveys.

Survey teams
Table 3 shows the distribution of field staff comprising field managers, monitors and survey teams across the seven sample districts. A total of 25 teams, each comprising four enumerators and one supervisor, were formed to complete our revised endline surveys. The supervisors were primarily responsible for troubleshooting issues that arose in the field, helping the enumerator to locate houses using the village map, and ensuring that the enumerators follow all the survey protocols.

Table 3: Distribution of survey teams

	Bhiwani	Jhajjar	Mewat	Palwal	Panipat	Rewari	Sonipat
Field managers	1	1	1	-	1	-	1
Monitors	5	4	2	3	2	3	3
Supervisors and surveyors	9	9	11	7	10	4	10

Compensation
No compensation was provided to the respondents for any of the surveys. When receiving consent from participants, the enumerator clearly explained that there is no direct benefit from being a respondent.

Training of enumerators
The enumerators were hired by the Abdul Latif Jameel Poverty Action Lab (J-PAL) South Asia directly through advertisements and word-of-mouth publicity. The selected enumerators were then trained by the research associates and the research operations team on administering the digital instrument and following protocol in the field. They were briefed on the organisation's sexual harassment policy and the road safety policy. This training was a mix of classroom lectures and field training in non-sample villages.

5.2.2 Administrative data
As a part of the intervention, ANMs were given tablets with a mobile app to track every subject vaccinated in the sample villages. Every patient visit was recorded by the mobile

app on the tablet and uploaded to a PostgreSQL database on the server. This information was downloaded and organised by assigning to each subject his or her corresponding treatment (reminders, incentives, both, none). The app then scheduled the reminders or recharges to be sent based on subjects' treatments. The server and the app could only be accessed through a secure link, to which only two researchers had access.

For our main analysis, we exploited these data as they were found to be of high quality and much more expansive than any household survey we could have conducted. Section 6.2 describes the process and its monitoring in greater detail.

5.2.3 Qualitative data collection

ANM focus groups

Qualitative data were collected through focus groups with ANMs conducted between November 2017 and February 2018. The rationale for the study was twofold: (1) to better understand some of the issues emerging from the monitoring data on ANM performance, such as ANMs not regularly using tablets, recording valid phone numbers, or informing beneficiaries about the incentives; and (2) to better understand the perceptions ANMs have around the acceptability, feasibility and perceived impact of the programme – given the importance of their support as the primary implementers of the programme.

We used focus groups to give ANMs the opportunity to share their perspectives, brainstorm and spark in-depth discussion. In addition, sitting with a group of colleagues likely gave ANMs confidence to share their issues and personal experiences.

The focus groups were designed to represent the study design, so we composed groups across the different incentive treatment arms within each district – at least one group for each incentive arm, and two groups for the control arm. Each focus group included a minimum of four and a maximum of six ANMs. Each group only included ANMs from the same PHC (the level of randomisation for the incentives experiment) to avoid the possibility of contamination during the discussion.

Aside from these parameters, participants were selected randomly from a master list of ANMs. Some effort was made to recruit participants within the same focus group that were of different ages, had different populations (migrants versus non-migrants) and were daily versus contractual employees. This was done to introduce heterogeneity within groups to help spark discussion.

A total of 18 focus groups were conducted with ANMs across four programme districts. The basic characteristics of the respondents are shown in Table 4 and suggest that we were able to achieve a reasonable heterogeneity in respondents across focus groups.

Table 4: Summary characteristics of focus group discussions

Characteristics of participant ANMs										
District	**Number of focus groups**		**Number of ANMs**	**Number of ANMs with types of population**			**Number of ANMs (type of employment)**			**Average age**
				Settled	**Migratory**	**Mixed**	**Regular**	**Contractual**	**Daily wage**	
Jhajjar (pilot)	C1, HS, HF, LS, LF, C2	6	32	21	2	9	17	15	N/A	44
Mewat	C, S, HF, LF	4	18	13	5	0	3	9	6	36
Palwal	C, S, HF, LF	4	21	12	1	8	6	11	4	38
Panipat	C, S, HF, LF	4	24	11	6	7	11	13	N/A	43

Notes: C = control; HS = high slope; LS = low slope; HF = high flat; LF = low flat; S = slope (combined).

ANMs were recruited by the research team by phone, using the mobile numbers provided at the pre-implementation training. In addition, notifications were sent to their supervisors requesting them to allow the ANMs to participate in the focus groups. Given that ANMs were now research subjects, the standard consent process was followed. Focus groups were recorded for analysis purposes. See Appendix M1 for the topic guide that was used to conduct the focus groups.

Caregiver interviews

A second qualitative data collection activity was conducted with primary caregivers in the form of semi-structured, in-depth interviews. The primary objective was to supplement and contextualise the quantitative findings from the impact evaluation; specifically, to support our process evaluation, validate our theory of change, and identify contextual factors that may have affected our findings beyond our theory of change.

The caregiver interviews sought to answer three research questions:

1. How well was the programme delivered, from the perspective of primary caregivers?
2. What factors influence a primary caregiver’s decision whether or not to immunise their child?
3. In what way was the programme able to influence primary caregivers to immunise their children, and why are certain caregivers in our programme still not bringing their children to immunisation camps?

The purposeful selection of participants was done for primary caregivers who were part of our programme, as well as for those who were not. This was done based on demographic details (e.g. religion, caste, education level, household characteristics) and responses from the endline survey on primary caregivers’ knowledge, experiences and attitudes around immunisation. We also identified villages from ANM and field staff feedback with interesting characteristics (e.g. villages with migrants, Muslims, daily wage workers, brick kiln workers and dwellers in village outskirts). Where selection was based on the responses of the endline survey, only primary caregivers who indicated they were interested in the programme following up with them were included in the selection pool.

A total of 20 respondents were interviewed. Characteristics of these respondents are summarised in Table 5.

Table 5: Summary characteristics of caregiver interviews

Demographics		Number of respondents
District	Mewat	9
	Panipat	5
	Sonipat	6
Incentive treatment arm	Control	5
	Flat	6
	Slope	9
Mother literacy level	Illiterate	5
	Never attended school/did not complete class 1	2
	Below class 7	4
	Class 8–12	6
	Graduate and above	3
	N/A	1
Religion	Hinduism	9
	Islam	10
	N/A	1
Immunis-ation status	Never immunised	8
	Partially immunised	6
	Fully immunised	6
Programme status	Part of the programme	13
	Not part of the programme	7

After obtaining consent, all in-depth interviews were audio-recorded and later transcribed and translated into English by a consultant. The maximum duration was set at no longer than one and a half hours to avoid respondent fatigue; however, most interviews took between 30–60 minutes.

Please see Appendix M2a and M2b for the interview guides that were used to conduct the in-depth interviews. Note that we focused primarily on the incentive treatment in the caregiver interviews. The communications experiment was not something that we could explicitly explore with respondents (as they would have limited awareness of it) and the implementation of the reminders treatment was stopped four months before we conducted the caregiver interviews.

5.3 Quality control measures

Providing incentives for immunisation can have positive spillover effects in villages without session camps. In fact, Banerjee and colleagues (2010) found large spillover effects in villages neighbouring the treatment villages with immunisation incentives. In the context of this project, the unit of randomisation of incentives was the PHC, containing, on average, twenty villages. PHCs are far from each other and it is unlikely in this context that the average outcome would be affected by spillover across villages in different PHCs.

For the reminders experiment, randomised at the village level, the information sent to parents were specific to each child. We are interested in the spillover effects on immunisation of children whose caregivers do not receive a text message.

The communication interventions are intended to measure the changes in immunisation behaviour through the channel of information diffusion. Therefore, the unit of randomisation is a village, and spillover between individuals is the precise channel by which we expect a change in outcome. In the intervention, the seeds were instructed to spread information on immunisation in their villages and it is unlikely that they will spread information to villages in which they do not reside.

While the programme was implemented on a large scale, our revised endline survey took place in only 200 villages as we used non-survey administrative data as our main data source. This would have minimised Hawthorne effects. Additionally, our survey procedure was light and non-invasive. Since our analysis relied on repeated cross-sectional samples of households, those that were surveyed in the endline were different from the baseline households and hence were not being observed by us during the intervention.

The fact that the PHCs are distant from each other would have minimised the John Henry effect, as, from an individual point of view (for most beneficiaries) their neighbours received the same treatment as they did. For the communication experiment, the intervention happened at the village level and the chosen seeds were not the households in the survey.

J-PAL South Asia has strict protocols on data quality control and these were adhered to over the course of the project. In addition to managing the survey logistics in their respective districts, the field managers and monitors were primarily responsible for ensuring data quality during the data collection. This was achieved by a high rate of partial and full accompaniments wherein they would silently observe the enumerator conducting the survey, in part or full, and then give them feedback. The high rate of accompaniments also ensured that the survey protocol was followed by all the survey teams.

A total of 20 per cent of households interviewed in the revised endline survey were also administered a subset of the main questionnaire. The objective of this back-check survey was to check the quality of the data collected during the revised endline survey and strengthen the monitoring activity in the field. The back-check data was matched with the main data to check for inconsistencies and enumerator error trends.

At the beginning of each day, the project associates and field manager would reiterate best practices to follow in the field, based on any inconsistencies seen the previous day. Additional support in the field and retraining, if necessary, was given to staff who did not perform well in the field. At the end of the day, a short debrief session was held, wherein problems were discussed and solutions proposed. These problems were shared with the research associate who would help to troubleshoot them.

A similar process was followed for all the other surveys conducted over the course of the project.

Once the data from the field were uploaded on the server, the research associate would run high-frequency checks to ensure their quality. This would entail checking for incorrect identifiers, duration of surveys and invalid 1responses. The errors thus generated were then forwarded to the project associates and field managers who would provide the research associate with the required corrections.

5.4 Ethical assurance

The authors sought and received institutional review board approval from the Institute for Financial Management and Research, which hosts J-PAL in South Asia, as well as from the Massachusetts Institute of Technology, the host institution of the lead researcher. Every modification made to the study design and change of personnel obtaining consent was shared with the institutional review board as an amendment.

Field staff was duly trained on the importance of informed consent and how to administer consent information before surveying respondents. Consents forms were read out loud to respondents before every survey, and a paper copy was left with the respondents, giving contact information should they wish to ask questions or make a complaint later on. Consent forms were written in Hindi, the local language understood by respondents in Haryana. Additionally, field monitors and field managers were trained to check for consent administration during fieldwork.

The study was registered with the American Economic Association trial registry on 6 October 2016, before the implementation of the intervention began. The pre-analysis plan was made public on 9 July 2018. The registration can be accessed here.

5.5 Sample size and sampling design

5.5.1 Power calculations

We originally estimated the required sample size for our indicators using one primary indicator – rate of full immunisation. The original power and sample size calculations from 2014 are presented in Appendix E of this report. We conducted our baseline survey in 2016, which gave us more precise estimates of intra-cluster correlation and immunisation rates. These calculations are also described in Appendix E.

The updated calculations suggest that we can detect a minimum effect size of 0.043 (or four percentage points) between the incentive treatment and incentive control arms at a significance level of 95% and a power of 90%. However, if we use a two-level clustering design (villages within PHCs and households within villages), we can detect an effect of seven percentage points with a power of 80%. For the communications experiment, we are powered to detect a minimum effect between 0.064 and 0.081 between two treatment arms. For the reminders experiment, we are powered to detect a minimum effect of 0.043 at a significance level of 95% and a power of 90%.

However, since the final incentive and reminders assignment was conducted at the SC level, and we decided to use our tablet data as our primary data source (which greatly increased the number of villages within each cluster and the households within each village), the above-mentioned calculations are a very conservative estimate of the minimum detectable effect in each of our experiments. We are powered to detect much smaller effects in each of the three experiments. Appendix E also presents a conservative example case, assuming one village per SC for varying levels of intra-cluster correlation. With an intra-cluster correlation of 0.12 (from the baseline), we can detect (against incentive control) a minimum effect of 0.045 for high slope, 0.033 for low flat/slope and 0.038 for high flat at 80 per cent power.

5.5.2 Sample size

A total of 62,548 households from the census conducted by the project fit our eligibility criterion and constituted our sampling frame, from which we sampled households for the baseline survey. Fifteen households[11] from each of the 912 villages were randomly selected to be administered the baseline survey. In PHCs where fewer than seven village units were sampled, a higher number of households from each village were sampled to maintain the proportional sampling of household across all PHCs.

No sampling was required for our outcome data as we decided to use the administrative data collected by the nurses on the tablets as our primary source of data for impact analysis. This comprised information about approximately 293,000 children from every village in our sample where the intervention was delivered. To complement this analysis, we redesigned a household survey to determine if there are alternative sources of immunisation to the camps (as opposed to a true increase in immunisation rates), and to generate a denominator for the fraction of children immunised.

First, we conducted a door-to-door census exercise in 200 randomly selected village units.[12]

The 200 village units were randomly sampled in two stages: in the first stage, we sampled one village unit per SC, resulting in 755 village units. In the second stage, we randomly selected 200 of those village units, stratifying by district and incentive sub-treatment arm.

We identified 18,963 eligible children during this census. For these children, we also collected information on whether a given eligible child has an MCP card, in addition to the following key identifying information: the child's name, date of birth, gender, parents' names, barcode, and the child's Aadhaar number.

Using a simple gated algorithm, we classified each child into one of three categories:

1. Matched (47.2%): This group contains the set of children that are successfully matched via barcode or the child's Aadhaar number, regardless of whether they have an MCP card;
2. Unmatched (31.85%): These children either: (a) don't have an MCP, and we were unable to match using their Aadhaar number; or (b) have an MCP, but no barcode and we were unable to match using their Aadhaar number; or
3. Uncertain (21.13%): These children have an MCP card, but their primary caregivers are not able to present it at the time of the survey.

[11] Fifteen households in each village unit were also randomly selected as replacements to be used if a household in the main sample cannot be interviewed for any reason.

[12] Due to the large variation in the size of village populations, we divided villages into roughly equal-sized 'village units' of 600 households, to ensure uniform sampling.

Next, we randomly selected approximately 3,000 children from the full population of eligible children identified through the census to complete the revised endline survey. Depending on the child's match status, we collect their full immunisation history in addition to administering the instrument. In addition, to complement the tablet data, the instrument varies by the child's match status: to avoid collecting redundant information, we only collect the full immunisation history for unmatched children.

Given that we had already collected information on the subset of matched children through our rolling tablet-data audit, we oversampled the unmatched and uncertain categories such that the final sample is composed 40% by unmatched children, 30% by uncertain children and 30% matched. In addition, due to large variation in the match rate across districts, the sampling was stratified by district.

The details of this are described in Appendix F1 (pre-analysis plan).

6. Implementation in practice

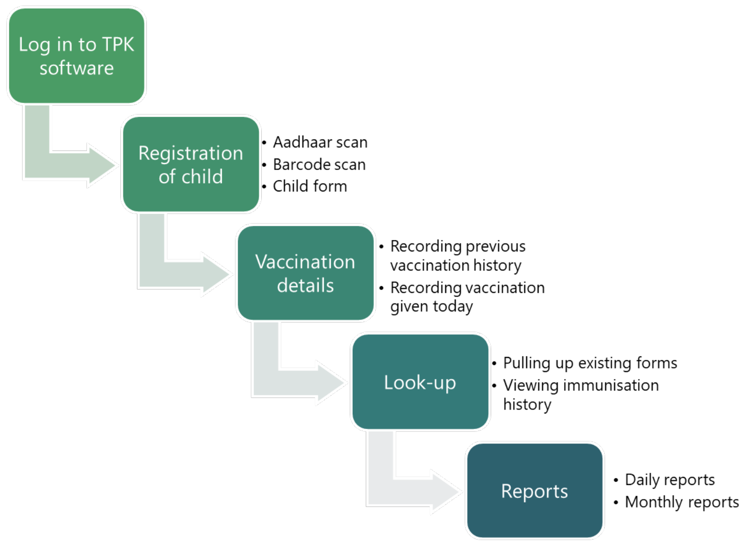
6.1 Key programme elements

6.1.1 m-Health app

The first key implementation activity the team undertook was the development of an m-Health platform, a beneficiary tracking app which was used by the front-line health workers (ANMs) to record administrative data at immunisation sessions. This app was developed as an innovative solution to overcome issues in the accuracy and availability of data, which are often faced in public health systems. It was created in collaboration with Sana, an organisation specialising in the development of m-Health platforms based at the Computer Science and Artificial Intelligence Laboratory at the Massachusetts Institute of Technology.

The app (also known in Hindi as *Teekakaran Protsahan Karyakram* [TPK]), which roughly translates to Immunisation Encouragement Programme) uses an Aadhaar card scan of the child and the caregiver as an identifier during visits to the session site. A summary of all of the functionalities of the app is shown in Figure 7.

Figure 7: m-Health app functionality



The app was very basic, with functionalities limited to immunisation tracking. However, this did not hamper the implementation of the programme; indeed, the simplicity facilitated it. It was not the intention of the programme to create an m-Health app that the Haryana government would fully adopt. The government is intending to roll out its own tablet app for ANMs in 2018 called auxiliary nurse midwife online (ANMOL), which has been co-created by UNICEF and sponsored by the Indian government. A future implementation of this programme could be merged with ANMOL.

6.1.2 Training ANMs

ANMs – the primary implementers of the programme – were trained using a master trainer (or training of trainers) model. This was the only feasible and effective method to train almost 1,400 ANMs with a very small programme team. Two-day, state-level training and orientation was conducted by J-PAL project staff between the end of December 2016 and February 2017 for all seven sample districts: Palwal, Mewat, Rewari, Sonipat, Panipat, Bhiwani and Jhajjar. The participants included district immunisation officers (DIOs), district training officers, medical officers (MOs) and several ANMs. These participants became the master trainers for subsequent district-level trainings.

A separate meeting was also conducted within these two days for the DIOs to orient the MOs and ASHA workers at the district level. The ANM trainings in all sample districts were conducted in batches from December 2016 to March 2017 by the master trainers supported by J-PAL staff.

The ANMs were aware that they were participating in the programme and were not blinded. As the ANMs from different PHCs and SCs may speak to each other, we decided to explain to them the presence and absence of incentives so to minimise the chance of confusion or resistance at a later stage. However, we ensured that the ANMs in treatment PHCs were trained separately from those in control PHCs to prevent any resistance before the launch of the programme.

To prevent any training effects on the quality of the implementation, master trainers and J-PAL staff members were administered trainings in both treatment and control groups. The same training material and protocols applied to both the groups, with the only notable difference being the presence of incentives. The training material was designed by the team at J-PAL with inputs from National Health Mission, Haryana.

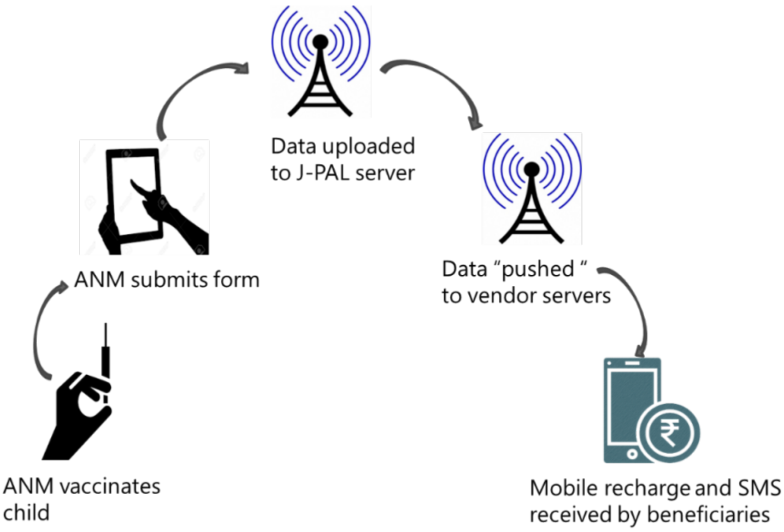
6.1.3 Intervention delivery

The delivery of the mobile phone recharges was managed by the project team using an algorithm scripted on the server. Every time a child under the age of 12 months attended an immunisation camp and received one of the five eligible shots (BCG, three doses of penta, measles-1), their form was uploaded to the server and an automatic process was triggered, whereby the server sent a request to the server of the relevant mobile phone provider to send a mobile recharge.[13] Figure 8 depicts the data flow in the project and how the interventions were delivered.

[13] Note that for returning children, each visit required the phone number to be re-entered, to which the system would send the recharge or reminder. This was to ensure that the system always had the most up to date phone number.

While the process of delivering recharges was largely automatic, there were times when some manual fixes had to be made, and those were managed by our team. A particular challenge was the fact that beneficiaries often port their phone numbers – that is, they switch from one mobile phone provider to another. This meant that the system needed to first search for the right provider before it could make a request for a recharge or a text message to be sent.

Figure 8: Data flow during intervention delivery



6.1.4 Implementation support

ANM hotline and issue log

A hotline was created for ANMs to call into if they face any issues implementing the programme in the field. This hotline used a toll-free number and was managed by J-PAL project field staff. Where possible, field staff resolved queries over the phone. Where necessary, they travelled to meet the ANM in question to resolve the issue in person.

In order to track issues that were raised via the hotline and otherwise, an issue log was created. This was reviewed on a weekly basis by J-PAL project staff to verify that the field staff took the appropriate measures to resolve issues and to address any unresolved issues.

Government reports

Throughout implementation, we shared progress reports with the government on a regular basis. This was important to maintain buy-in as well as to ensure successful implementation of the programme. ANMs work within the government system, and the main avenue to support them and correct implementation practice was through their supervisors – MOs and DIOs. We reported to the government in the following ways:

Monthly PHC-level, district-level and state-level reports

Since April 2017, when the programme was fully launched in all seven districts, we produced monthly PHC-level reports for all of the 140 PHCs that participated in our programme, which were sent to MOs at the start of every month. In addition, we produced monthly district-level reports for DIOs, which aggregated data of all PHCs within the districts, and state-level reports for the deputy director of child health and the mission director of the National Health Mission, which aggregated data from all seven districts.

Attending monthly district meetings
Every month, in conjunction with the PHC-level and district-level reports, members of the J-PAL project team attended district meetings. They presented the latest data for the districts and delivered key messages of improvements that needed to be made. This forum was also an opportunity for MOs and DIOs to ask questions, request clarifications and provide explanations for deviating data.

Attending quarterly state meetings
At the state level, members of the J-PAL project team attended meetings with the deputy director of child health, the mission director (National Health Mission), and the principal secretary for health. On average, these occurred every 2–3 months and involved providing an update on the progress of the project, and a discussion concerning scale-up and further collaboration in the future.

Overall, MOs and DIOs responded positively to the reports and meetings, and, over time, increasingly engaged with the presented data and took actions necessary to improve implementation. This was evident from the reduction in the number of implementation issues reported by ANMs (Section 6.2).

6.2 Process monitoring

6.2.1 Monitoring activities
An extensive system of monitoring activities was set up to assess the quality of programme delivery by ANMs and the quality of the data collected through the tablets, as outlined below.[14]

Server checks
This entailed using the data on the server, checking ANMs' data entry and ensuring that session camps are occurring and children are attending. On a daily basis, the number of uploaded forms was checked on the server, using Tableau®, a software that effectively and easily visualises data. There were multiple occasions wherein this daily monitoring revealed that the server had run into a problem and forms were not being uploaded, which was remedied immediately with the software engineers.

Session site monitoring
This activity assessed how ANMs are performing in the field. J-PAL project field staff observed session camps and recorded the presence (or absence) of key aspects of programme implementation (e.g. whether the tablet was being used, whether the information banners were present and visible). The sampling was done in such a way that at least one ANM in each of the 755 SCs was visited during each round of monitoring.

Enumerators would record the performance of the ANMs in their tablets. Two rounds of session site monitoring were conducted. Round two was conducted after all SCs were monitored once in round one. The ANMs who were monitored in round one were removed from the sampling frame for round two. See Appendix L1 for the instrument used.

[14] Note that some monitoring activities that were originally planned, such as the SC-level and PHC-level surveys, were not implemented. When looking at the monitoring plan as a whole, it was determined that these particular activities would not provide much added value. Overall, the suite of monitoring activities already set up were able to adequately capture all key aspects of implementation.

Child verification
A household survey was conducted to monitor programme implementation at the child level. This was performed in order to check whether the record entered into the tablet corresponded to an actual child, and whether the data entered for this child were correct. This novel child verification exercise involved J-PAL field staff going to villages to find the households of a set of randomly selected children which, according to the tablet data, visited a session camp in the four weeks prior.

Child verification was continuous throughout programme implementation, and the findings indicate high accuracy of the tablet data. We sampled children every week to ensure no additional vaccine is administered in the lag between the session camp and the monitoring team visiting them. See Appendix L2 for the instrument used.

ANM issue log
Data from the ANM issue log (described above) were monitored on a monthly basis to determine whether the number and type of issues were changing. A reduction in issues was used as a proxy for better implementation.

In addition, daily checks were run on the server data to determine whether mobile recharges were being sent and delivered. This was made possible because the J-PAL server directly communicated with the vendor server through an application programming interface, receiving real-time delivery status updates on recharges. A script was written onto the server which produced an automatic daily report sent to the project team summarising the recharge success rates. Every month, the project team would run their own analysis using the monthly spending reports from the vendor to double-check the server's data.

6.2.2 Overall quality of implementation
Overall, the implementation of the programme was highly successful, especially given the scale at which implementation took place. Some of the implementation highlights include:

- ANMs used the tablets – monitored tablets are present in over 98 per cent of session camps, and on average 90 per cent of ANMs upload data to the server every month;
- ANMs became more proficient in using the tablets and delivering the programme over time – the incidence of issues reported by ANMs gradually reduced over time across all districts, as did the issues of data entry and data quality that were monitored on the server on a monthly basis;
- Data entered into the tablets were generally of high quality – there were nearly no incidences of fake child records, and the child's name and date of birth were accurate over 80 per cent of the time. For 71 per cent of children, the vaccines overlapped completely (for all main vaccines under the age of 12 months). Vaccine-wise, on average, 88 per cent of cases had matching immunisation records. Errors seem genuine, rather than coming from fraudulence; they show no systematic pattern of inclusion or exclusion and are no different in any of the treatment groups;
- The delivery rate of recharges was high – the recharge success rate was above 90 per cent on average, with only four months dropping to 85 per cent; and
- The delivery rate of text message reminders was as expected – the success rate was around 80 per cent on average, with some fluctuation over time.

Nevertheless, there were a number of implementation issues, as demonstrated by our monitoring data. While these were addressed to the extent that was possible, the following issues persisted:

- For only 61 per cent of sessions monitored, a TPK information banner was present, and in 52 per cent of monitored incentive immunisation camps, ANMs were not telling beneficiaries about the recharges. This needs to be interpreted with caution, as surveyors were not able to check the age of children attending the immunisation camp (in order to minimise observation effect), and only those older than 12 months were eligible for recharges. Nevertheless, it is likely that the ANM skipped the messaging about incentives for some eligible children.[15] Both indicators mean that in a couple of cases, caregivers may not have necessarily associated the incentives with receiving one of the five key vaccines;
- The number of valid phone numbers was much lower in the no-incentives (control) group that in any of the incentive groups (75% versus 96%, respectively), presumably because ANMs thought collecting this information was less important as caregivers would not receive a mobile credit recharge; and
- While always above 90 per cent, the recharge success rate fluctuated by week and also by sub-group, due to issues beyond the programme's control (e.g. the introduction of the goods and services tax, mergers of mobile phone providers, changes in the accepted denominations by service providers).

A detailed report of the monitoring results is presented in Appendix L3.

6.2.3 Acceptability and feasibility of the TPK programme

From both the ANM focus groups (95 ANMs) and the caregiver interviews (20 primary caregivers), we collected data on the perceived acceptability, feasibility and fidelity of the programme. A full summary of the qualitative findings can be found in Appendix I (for the focus groups) and Appendix K (for the caregiver interviews). In this section, we focus on findings related to implementation. We describe the impact of the programme (from both the ANM and caregiver perspective) in Section 7 after we discuss the main findings.

ANM focus groups

Overall, ANMs judged the programme as acceptable and were generally enthusiastic about using the tablet to record data. While ANMs acknowledged that double record maintenance (tablet and register) increased their workload, they liked the time-saving automation (as opposed to manual and tedious processes like report generation), the easy accessibility of data and the proof of work completed.

Nevertheless, most ANMs listed technical and practical limitations in using the tablets, such as: inability to use the same barcode for children who received vaccines outside the programme area; not being able to go back in the app to rectify mistakenly selected vaccines; having to enter Aadhaar card numbers manually because of the sensitivity of the scanner to laminated or photocopied cards (an issue for all Aadhaar-based applications in India).

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[15] We know from our tablet data that approximately 78 per cent of visits registered at the session camp were for children 12 months or younger for one of the five main vaccines. As a rough approximation, therefore, we would expect ANMs to only deliver the message of incentives approximately 78 per cent of the time. This means that ANMs did not deliver the messages about incentives when they should have approximately 26 per cent of the time.

In addition, across all districts, ANMs mentioned facing complaints of recharge failures in incentive treatment arms, with some primary caregivers threatening not to return to the immunisation camp. While some ANMs worried about this, others dismissed it as part of the job of dealing with primary caregivers more generally. These findings help to contextualise some of the data entry issues that were found in the tablet data and the recharge failures we observed during certain months, although the extent of both of these issues was limited.

Not many concrete answers were given when ANMs were specifically asked to comment on hypothetical situations of ANMs not entering phone numbers in the tablet, or entering their own phone numbers (two core issues identified through the monthly reporting to government). Most concluded that this must be because the primary caregivers were not bringing valid phone numbers to the camp; generally, they mentioned problems with primary caregivers bringing documentation and other information for the programme.

With regard to the reason why an ANM would enter her own phone number, most respondents did not think this would be a case of fraud, but more that these children either lived in the ANM's household, or that the primary caregiver insisted the ANM enter her own number. Of course, this question is highly subjected to social desirability bias.

ANMs in Mewat appeared to be less enthusiastic about tablets. They were particularly frustrated due to delays associated with primary caregivers not bringing documentation and the time taken to conduct dual data entry during busier session camps. This, combined with their belief that the tablets failed to convince primary caregivers to come to session camps, fell within the context of general resistance to immunisation in Mewat.

Interestingly, while this was not an original part of the programme's theory of change, the tablets seemed to have raised self-esteem among many ANMs, who were excited to learn something new, become confident in their potential to harness technology, and impress both caregivers and their own families.

Caregiver interviews

Overall, primary caregivers had little spontaneous awareness of the TPK programme,[16] suggesting that it was not very salient to them. In a number of cases, respondents did not know what the components of the programme were (i.e. what was different from before); even those who were aware did not necessarily understand the purpose of the programme components (e.g. use of the tablet, the need to bring the Aadhaar card). This is not necessarily unexpected, as the programme was integrated into an existing government initiative, and demarcating what has changed is perhaps not very evident or indeed important.

In addition, most government programmes now do require some form of documentation, so this would have not seemed out of the ordinary. One key underlying theme is that people don't think very actively about immunisation and immunisation camps, which may to some extent explain the lower than expected salience.

[16] The 'TPK programme' was defined by its core components: use of tablets; bringing an Aadhaar card; banners present at the session camp; receipt of congratulatory text messages; and, in the relevant treatment groups, the receipt of incentives, being told about incentives by the ANM, and receipt of reminder text messages.

Importantly, respondents who were in the incentives treatment group did generally have a higher degree of awareness, both unprompted and prompted. Indeed, incentives were mentioned spontaneously by some respondents, and acted as a trigger when prompted by the interviewer, suggesting that they seemed to have stood out for some and made the programme as a whole more salient (i.e. including tablets and messages).

When prompted explicitly, most primary caregivers were positive about the programme and camp in principle, but were indifferent in terms of their own experience of it. Perception of the programme seemed driven by a combination of implementer behaviour and primary caregiver expectations. ANMs are not accessible to primary caregivers, who are dependent on ASHAs to be recruited for immunisation camps.

Primary caregivers had varied assessments of the performance of the ASHA and/or ANM, which seemed dependent upon the individual. Interestingly, in some cases, primary caregivers blamed the side effects caused by vaccines on ANM performance, for example, that she administered it too 'harshly'. This suggests that the empathy shown by the ANM while vaccinating the child is sometimes an important factor in how primary caregivers feel about the experience.

Primary caregivers who were not part of the TPK programme seemed generally more negative than those in the programme. This could be because ANMs in the treatment group had more key messages to convey and provided information about incentives.

Some primary caregivers clearly reported cases of non-fidelity by the ANM (as determined by the researchers), but this did not seem to be systematic – for each example of non-fidelity, there was an example of fidelity. A few primary caregivers who should have received the incentives said they had never received them, did not receive them for every vaccine, or did not receive the full amount. This was either revealed by them or surmised by the interviewer.

The degree to which this upset primary caregivers varied – from simply being curious about not receiving the incentive to feeling loss and even anger. This again seemed to be mediated by the primary caregiver's relationship with the ANM, where anger coincided withholding negative views about the ANM.

6.2.4 Changes in implementation

In March 2017, all immunisation and supplement programmes in Mewat district were stalled, as rumours of impotency tablets and infertility injections created a panic among the population. A WhatsApp message spread a rumour that children were being given drugs that would render them impotent, which prompted anxious parents to bring their children studying in government schools back home, forcing schools to shut abruptly. In fact, the children were being administered pills and supplements as part of a state-wide deworming and nutrition plan. But a rumour spread that it was a drive to check the growth of the Muslim population. In some areas, ANMs were even held captive when they went to conduct sessions. Due to this, no immunisation sessions were held until mid-March 2017.

Apart from this, there was one unanticipated change in an intervention during implementation. Due to a technical glitch, the implementation of the targeted reminders intervention was discontinued starting November 2017. Thus, the analysis for the targeted reminders experiment is based on data collected until this date.

6.2.5 Implementation quality and the theory of change

From our extensive monitoring activities, we have collected data for most of the links in the chain of our theory of change (Section 2.2). Our findings indicate that most of the assumptions hold. ANMs were trained successfully, session camps were mostly held as planned (and there were no reports of supply side issues), ANMs were almost always using the tablets, and by and large, the data ANMs entered were reasonably accurate. The one major issue identified was that ANMs entered substantially more invalid phone numbers for the incentive control group.

Our findings from monitoring activities are less conclusive about whether the assumptions hold in terms of outputs. While our data show that on average over 90 per cent of beneficiaries were successfully sent mobile recharges and over 80 per cent were successfully sent text message reminders, we have limited data to determine whether the intended beneficiaries *actually* received them and remembered them.

The recall data from the child verification exercise on text messages demonstrate that only half the beneficiaries remember receiving anything. However, we should be wary of relying on recall data, especially given that in some cases, messages will have gone to the phone number of someone else in the household. From our caregiver interviews, we found a number of respondents who stated they did not receive the recharges consistently. Again, we need to keep in mind that these might have been delivered to another phone in the household and that we did expect some cases of failed delivery.

We also cannot rule out that respondents were hesitant to tell us even if they did receive recharges; our qualitative findings suggest there may be social desirability bias around admitting that incentives are a motivating factor (Section 7.3.2). In any case, we find no evidence, from any of our many data collection activities, of primary caregivers refusing to attend a session camp because they did not receive an incentive.

With regard to the communication experiment, our process monitoring has only been able to capture limited process data pertaining to the theory of change. While we know we have been able to send text messages to seeds successfully over 80 per cent of the time, we were unable to directly track systematically whether seeds actually received them or disseminated them through their networks.

There are two important reasons for this: first, we could not carry out a monitoring activity with the seeds, as that would have likely introduced measurement effect, especially given how light-touch our intervention was. Any interaction with seeds themselves, for example asking them if they remember receiving messages on their phone, or if they disseminated immunisation-related information to the community, would have constituted subsequent messaging and would have changed the nature of the intervention.

Second, a monitoring activity with the beneficiaries would not have been able to tell us very much about whether seeds actually disseminated information. The theory assumes that information is diffused through networks, so it is very difficult to ascertain whether and at what points seeds were involved in the dissemination of information. Indeed, our qualitative data collection with primary caregivers and seeds, which were conducted after the implementation was complete, showed that even when we tried to explicitly explore this, it was difficult to capture.

The data that we do have from the endline survey suggest that very few of our respondents in seeds villages are able to identify who they receive information from (other than the ASHA or ANM). However, to the extent they were able, few received information directly from the seeds.

Our qualitative data collection with primary caregivers further supported this, and interviews with seeds showed that they too have difficulty identifying specific people to whom they have spread information. This corresponds to our theory of change: the idea was not that the seed would directly inform everyone but would talk to their close friends, who would then talk to their close friends. Our theory suggests that if this is the way the process of diffusion actually works, gossips would be the most effective form of seeds in a social process.

In any case, the 'intervention', as defined by what could be replicated by a government in another context, was implemented faithfully, since the messages were sent. The proof that it worked as we intended will be revealed by the final results.

Please see Appendix G for an overview of our process monitoring outcomes.

7. Impact analysis and results of the key evaluation questions

7.1 Analysis

Based on the findings from our monitoring data, which showed that the tablet data were of high quality, we decided to use the administrative data as our primary data source for the main outcome analysis. The sample is much larger and the data more precise than what households can tell us in surveys. We conducted a revised endline survey to complement the interpretation of the tablet data, including testing for a substitution effect, and analysis of the impact on knowledge and attitudes (data that are not available in the tablet). For a detailed explanation of the analysis, please refer to the pre-analysis plan (Appendix F1).

7.1.1 Tablet analysis

Incentives

For the incentives experiment, we estimated the following specification using the tablet data aggregated at the SC-month level, since the incentives experiment was randomised at the SC level. In particular, we estimated the following main specification:

$$Y_{st} = \alpha + \sum_j \beta_j T_s^j + \mu_d + \mu_t + \epsilon_{s,t}$$

where T_s^j denotes the type of incentive scheme SC s is assigned to: j belongs to {high slope, low slope, high flat, low flat}. u_d denotes a set of district fixed effects and u_t denotes a set of month fixed effects. Y_{st} represents a given outcome for SC *s* in month *t*, for the set of outcomes specified above. Standard errors are clustered at the SC level.

Communications experiment

For the communications experiment, we focused on the subset of 980 villages within which we randomly assigned villages to a communication sub-treatment, the set 'at risk for seeds'. Given the level of randomisation, we aggregated the data at the village-month level, and estimated the following specification:

$$Y_{vt} = \alpha + \sum_{k} \delta_k Seed_v^k + \sum_{j} \beta_j T_s^j + \mu_p + \mu_t + \epsilon_{v,t}$$

where $Seed^k_v$ is an indicator equal to one if village v has been assigned to seed type k, k belongs to {gossip, trusted, trusted gossip, random}. To maximise precision and reduce noise as much as possible, we included a full set of controls T_s^j for the incentives experiment and a full set of PHC indicators μ_p and time fixed effects μ_t; however, we omitted one of the treatment dummies since the other is otherwise collinear with PHC-level fixed effects. Y_{vt} represents a given outcome for village v in month t, for the set of outcomes specified above. We estimated cluster-robust standard errors, clustered at the village level.

We furthermore weighted these village-level regressions by village population. We did this because of the variance in village population sizes at this level, which is not as relevant at the SC level. Weighting standardises our analysis for better comparability to the incentives and reminders experiments.

Targeted reminders experiment
SC-level effect of reminders: Since the fraction of individuals receiving a reminder was randomised at the SC level, we analysed the data at the SC-month level, using the following specification:

$$\mathrm{Y_{st}} = \alpha + \emptyset_1 Q_s^{33} + \emptyset_2 Q_s^{66} + \mu_\mathrm{d} + \mu_\mathrm{t} + \epsilon_{\mathrm{s,t}}$$

where Q_s^{π} is an SC-level binary variable indicating that SC s belongs assigned to the group where π% of individuals receive a reminder.

Individual effect of reminders: In addition to estimating the SC-level effect of assigning a fraction of individuals to receive reminders, we estimated the individual effect of reminders. This is well identified, since we randomly assigned children to receive reminders conditional on having received the previous vaccine, based on the fraction π%. To this end, we used the following as our base specification on individual-level data, restricting to SCs assigned to a non-zero-fraction reminders treatment group:

$$Y_{is} = \alpha + \theta R_i + \mu_d + \epsilon_i$$

Here, the outcomes Y_{is} for child i in SC s are the same outcomes defined above, but here the outcome is recorded as '1' for a specified vaccine if the child received it at *any* time they were eligible for it, and '0' only if they were both eligible for it and *never* received it. The intent-to-treat effect is found by regressing outcome Y_{is} on an indicator R_i for whether child i has been assigned to receive reminders. We included district fixed effects. Since we restricted to SCs where the probability of receiving a reminder is non-zero, we report heteroskedasticity robust standard errors.

Spillover effects of reminders: Finally, to estimate the spillover effect of reminders, we estimated a version of the specification above on the entire individual-level data for the implementation period of the reminders experiment. In particular, we estimated the following specification:

$$\mathrm{Y_{ist}} = \alpha + \gamma_1 R_i * Q_s^{33} + \gamma_1 R_i * Q_s^{66} + \emptyset_1 Q_s^{33} + \emptyset_2 Q_s^{66} + \mu_\mathrm{d} + \mu_\mathrm{t} + \epsilon_{\mathrm{s,t}}$$

where all the variables are defined as before.

7.1.2 Endline analysis

Substitution between camps and other immunisation places

To address the question of substitution between camps as a source of immunisation from other sources, we estimated the effect of the intervention on immunisation outcomes in the set of children who are not in the tablet data – what we call 'unmatched' children. If the treatment effect observed in the tablet data reflects a substitution as opposed to a true treatment effect, we expect to observe lower immunisation rates for unmatched children in treated villages. To this end, we estimated the following specification:

$$Y_{iv} = \alpha + \sum_{j} \beta_j T^j_{[v]} + \mu_d + \epsilon_{iv}$$

where $T^j_{[v]}$ denotes the treatments received by village v among the incentive treatments, the communication treatments and the reminder treatments. We report standard errors clustered at the village level to account for possible correlation between children in the same village. Y_{iv} denotes the immunisation outcomes of child i in village v, specifically a set of indicators for each of the key vaccines and an indicator for successful completion of the entire schedule.

Log versus levels

We estimate treatment effects for outcomes in both logs and levels. Where possible we use log; however, where there are too many zeros at a specific level of analysis (e.g. because there was no child who received a particular vaccine in the village in a particular time period), we present specification in levels.

Note that we have used abbreviated names for outcomes in the regression tables so as to not squash the full descriptions of seven outcomes in the tables. In every outcome there is an implicit 'number of' as part of the description of the outcome. 'Log vaccine given' means the log of the number of vaccines given in the camp where we only consider the vaccines among those of interest in our intervention. 'Log children' means the log of the number of children that attended the camp (where they may have received a vaccine beyond our vaccines of interest).

Dealing with missing data

For log outcomes, we replaced zeros with '1' prior to taking logs conditional on a session being held (there are few zeros at this level of aggregation). For control variables that do not take on the value 0, we replaced missing values with zeros and included an indicator for when the variable is missing at baseline.

7.1.3 Qualitative analysis

The data collected via focus groups with ANMs and the caregiver interviews were analysed using thematic analysis. Main themes were identified, using the interview or topic guide as a framework along with the number of respondents who expressed or agreed with this theme, to provide insight into the degree of agreement. In order to assess the consistency of the coding, a second analyser independently coded focus group discussions at the beginning of the analysis. Disagreements between the two coders were resolved by a third coder who was aware of the exercise, but who was not part of the data collection and therefore had no prior assumptions. There were very few instances of disagreement between coders.

7.2 Main results

7.2.1 Tablet data

This section reports the results from our main outcome analysis using the tablet data. Note that we only present the tables for the main specifications. For all additional tables based on the analyses explained in the pre-analysis plan, please refer to Appendix F1.

Incentives experiment

Overall, we find that incentives have a positive significant effect on immunisation outcomes (Table 6).

The effect of the incentive depended on its profile. We find no effect of the flat incentive profile. The sloped incentive schemes (increasing across the immunisation schedule) have a significant positive effect at the extensive margin. We find an 11.8 per cent increase in the number of children who are fully immunised. Looking at the results separately by vaccine, we see that this reflects an increase in number for each of the five vaccines in the EPI package, but particularly for measles-1, where we see a 14.2 per cent increase (Table 7).

In contrast with the profile of incentive, the *level* of incentive does not seem to make a large difference. Looking at the effects separately by each of the sub-treatment arms, we find that these effects are driven by both the high and low slope treatment arms (Table 8). The difference between the low slope and high slope impact estimates is not statistically significant.

Figure 9 uses the baseline full immunisation rates to transform these coefficients into a percentage point increase in the full immunisation rate in low slope and high slope treatment arms vis-à-vis the control group.

We are unable to reject the null hypothesis for flat incentives (both high and low flat incentives), and therefore find no evidence that flat incentives have an impact on immunisation outcomes.

Table 6: Incentives experiment – overall effect

	Dependent variable						
	Log vaccine given	Log fully immun-ised	Log children	Log shot penta1	Log shot penta2	Log shot penta3	Log shot measles1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Incentive	0.082** (0.035)	0.050 (0.040)	0.058* (0.035)	0.082** (0.036)	0.069* (0.036)	0.072* (0.037)	0.078** (0.037)
Control Mean	44	8	45	11	10	9	9
Total obs.	10,214	10,220	10,220	10,214	10,214	10,214	10,214
Zeros replaced	2	413	0	141	138	159	235

Notes: *p < 0.1; **p < 0.05; ***p < 0.01. Obs. = observations. All specifications include district-time fixed effects. For outcomes expressed in logs, -Inf replaced with 0. Control mean shown in levels. Standard errors clustered at the SC level.

Table 7: Incentives experiment – slope versus flat

	Dependent variable						
	Log vaccine given	Log fully immunised	Log children	Log shot penta1	Log shot penta2	Log shot penta3	Log shot measles1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Slope	0.132***	0.118***	0.108***	0.134***	0.121***	0.119***	0.142***
	(0.039)	(0.045)	(0.039)	(0.041)	(0.041)	(0.042)	(0.042)
Flat	-0.002	-0.066	-0.027	-0.005	-0.017	-0.009	-0.030
	(0.046)	(0.055)	(0.046)	(0.047)	(0.048)	(0.048)	(0.049)
Control mean	44	8	45	11	10	9	9
Total obs.	10,214	10,220	10,220	10,214	10,214	10,214	10,214
Zeros replaced	2	413	0	141	138	159	235

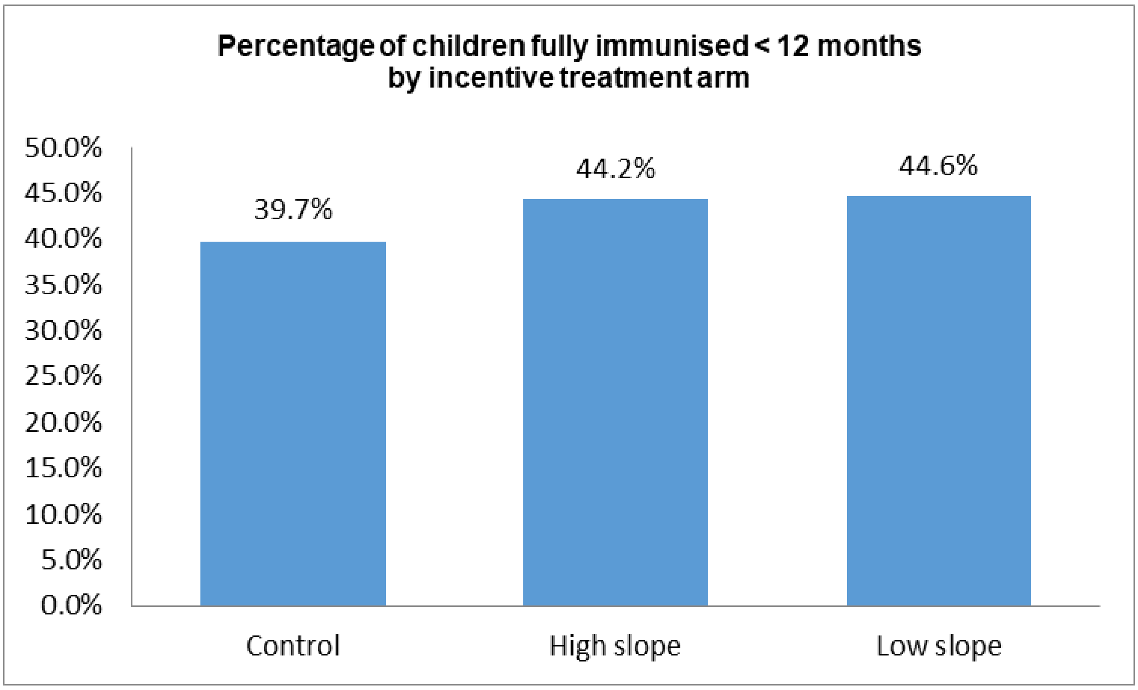
Notes: *p < 0.1; **p < 0.05; ***p < 0.01. Obs. = observations. All specifications include district-time fixed effects. For outcomes expressed in logs, -Inf replaced with 0. Control mean shown in levels. Standard errors clustered at the SC level.

Table 8: Incentives experiment – overall

	Dependent variable						
	Log vaccine given	Log fully immunised	Log children	Log shot penta1	Log shot penta2	Log shot penta3	Log shot measles1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High slope	0.115**	0.114**	0.087*	0.115**	0.094*	0.101**	0.131***
	(0.045)	(0.053)	(0.046)	(0.047)	(0.049)	(0.050)	(0.050)
High flat	0.015	-0.048	-0.009	0.011	-0.007	-0.014	-0.034
	(0.059)	(0.068)	(0.059)	(0.062)	(0.062)	(0.061)	(0.063)
Low slope	0.150***	0.122**	0.129**	0.153***	0.149***	0.138**	0.154***
	(0.053)	(0.061)	(0.053)	(0.055)	(0.055)	(0.057)	(0.056)
Low flat	-0.020	-0.085	-0.045	-0.021	-0.027	-0.003	-0.026
	(0.061)	(0.078)	(0.063)	(0.062)	(0.065)	(0.066)	(0.066)
Control mean	44	8	45	11	10	9	9
Total obs.	10,214	10,220	10,220	10,214	10,214	10,214	10,214
Zeros Replaced	2	413	0	141	138	159	235

Notes: *p < 0.1; **p < 0.05; ***p < 0.01. Obs. = observations. All specifications include district-time fixed effects. For outcomes expressed in logs, -Inf replaced with 0. Control mean shown in levels. Standard errors clustered at the SC level.

Figure 9: Percentage of children who are fully immunised



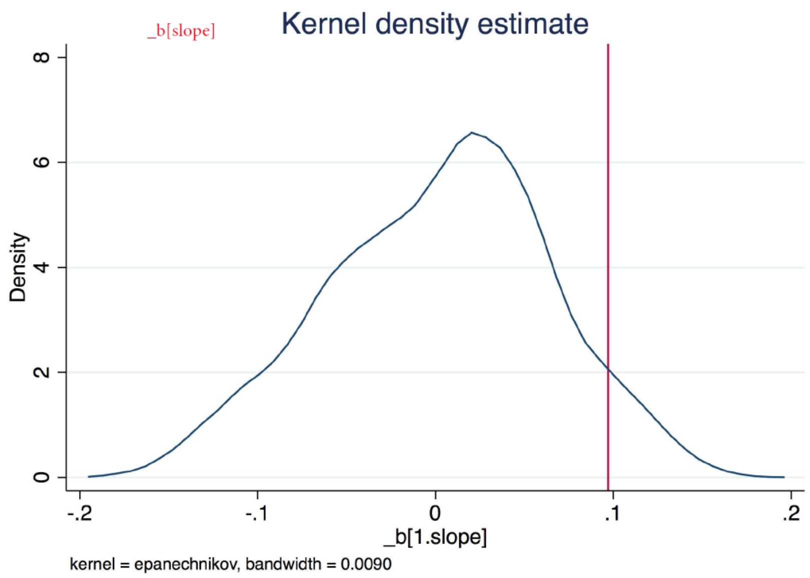
Note: Control rate is based on baseline full immunisation rate.

Randomisation inference

As discussed in the pre-analysis plan, the type of incentive (high flat, low flat, high slope, or low slope) was randomised at the SC level but the provision or omission of incentives was randomised at the PHC level. Clustering at the SC-level potentially underestimates the standard errors. Since there is no straightforward analytical solution to this problem, we address this concern by conducting a randomisation inference test for the same specification.

Using a randomisation inference test, we can reject the one-sided sharp null for (any) slope treatment at the 10 per cent level. Figure 10 presents a graphical depiction of this.

Figure 10: Randomisation inference test



Communication experiment

To analyse the communication experiment, we shift to village month-level specifications (since the experiment was randomised at the village level). Since there are many zeros in these regressions, we present specification in levels. Note that some outcomes (number of children and number of vaccines) remain in log because data are at a lower level and therefore do not have many zeros.

Consistent with the theory of Banerjee and colleagues (2014), we find that immunisation rates are higher when the person chosen to relay information in the village was a 'gossip' (someone villagers considered to be good at spreading information). More importantly, we find that leveraging the social network in this way leads to an increase in immunisation rates. In particular, we find that 1.321 additional children were fully immunised every month in gossip seed villages compared to pure control villages, and 1.373 additional children came for the measles-1 vaccine (Table 9).

The impact of gossip seeds appears to be larger than that of any other type of seed. In gossip seed villages, 1.5 additional children each came for the penta-3 vaccine, and 1.6 additional children received the measles-1 vaccine and were fully immunised every month (Table 10).

Given the standard errors, we cannot reject the finding that for trusted seed and trusted gossip seed villages, there is no effect (compared to random seeding) or that the effect is as large as that for the gossip seeding. At best, this suggests that there was no gain from explicitly trying to identify trustworthy people, even for a decision that probably requires some trust. It is very encouraging that the simplest seed identification is effective.

When we restrict our sample to villages that did not receive incentives, we find that gossip seeds have an even larger significant positive effect on full immunisation as well as separately for all five vaccines in the EPI package. Per month, 2.61, 2.47 and 2.24 additional children came for penta doses 1, 2 and 3, respectively, and 1.90 additional children came for the measles-1 vaccine.

Overall, 1.80 additional children were fully immunised every month in non-incentive gossip villages (Table 11). However, if we restrict our sample to the incentive villages, the effect of gossip disappears (Table 12) and trusted gossip has a negative treatment effect.

Table 9: Communication experiment – compared to control

	Dependent variable						
	Log vaccine given	Fully immunised	Log children	Shot penta-1	Shot penta-2	Shot penta-3	Shot measles-1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Random	-0.013	-0.306	-0.025	-0.033	-0.141	-0.272	-0.237
	(0.076)	(0.524)	(0.076)	(0.665)	(0.625)	(0.605)	(0.551)
Gossip	0.168**	1.321***	0.171**	1.448**	1.258**	1.220**	1.373***
	(0.068)	(0.327)	(0.069)	(0.626)	(0.579)	(0.532)	(0.510)
Trusted	-0.031	-0.278	-0.031	-0.638	-0.584	-0.376	-0.431
	(0.070)	(0.449)	(0.071)	(0.715)	(0.590)	(0.557)	(0.558)
Trusted gossip	0.083	-0.081	0.082	0.213	0.061	-0.034	-0.005
	(0.070)	(0.422)	(0.071)	(0.570)	(0.526)	(0.502)	(0.471)
Control mean	19	3	20	5	4	4	4
Total obs.	11,539	11,567	11,567	11,539	11,539	11,539	11,539
Zeros replaced	13	0	0	0	0	0	0

Notes: *p < 0.1; **p < 0.05; ***p < 0.01. Obs. = observations. All specifications include PHC-time fixed effects and a full set of controls for incentives. For outcomes expressed in logs, -Inf replaced with 0. Control mean shown in levels. Standard errors clustered at the village level.

Table 10: Communication experiment – compared to random

	Dependent variable						
	Log vaccine given	Fully immunised	Log children	Shot penta-1	Shot penta-2	Shot penta-3	Shot measles-1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Gossip	0.193**	1.624**	0.205**	1.319	1.476*	1.531**	1.601**
	(0.088)	(0.634)	(0.088)	(0.831)	(0.779)	(0.749)	(0.679)
Trusted	0.073	0.749	0.085	0.396	0.459	0.744	0.690
	(0.094)	(0.536)	(0.094)	(0.704)	(0.651)	(0.614)	(0.588)
Trusted gossip	0.107	0.327	0.119	0.411	0.465	0.378	0.333
	(0.088)	(0.495)	(0.088)	(0.670)	(0.618)	(0.591)	(0.544)
Control mean	17	3	18	4	4	4	4
Total obs.	6,697	6,712	6,712	6,697	6,697	6,697	6,697
Zeros replaced	10	0	0	0	0	0	0

Notes: *p < 0.1; **p < 0.05; ***p < 0.01. Obs. = observations. All specifications include PHC-time fixed effects and a full set of controls for incentives. For outcomes expressed in logs, -Inf replaced with 0. Control mean shown in levels. Standard errors clustered at the village level.

Table 11: Communication experiment – restricted to no incentive set

	Dependent variable						
	Log vaccine given	Fully immunised	Log children	Shot penta-1	Shot penta-2	Shot penta-3	Shot measles-1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Random	-0.044	0.223	-0.038	0.650	0.472	0.343	0.285
	(0.114)	(0.875)	(0.113)	(1.077)	(1.018)	(1.001)	(0.896)
Gossip	0.290***	1.817**	0.297***	2.607***	2.468***	2.236***	1.899**
	(0.095)	(0.731)	(0.097)	(0.970)	(0.909)	(0.824)	(0.786)
Trusted	-0.085	-0.174	-0.072	-0.389	-0.420	-0.127	-0.194
	(0.105)	(0.706)	(0.107)	(0.855)	(0.808)	(0.760)	(0.785)
Trusted gossip	0.169*	0.834	0.188*	1.596**	1.273*	1.035	0.953
	(0.100)	(0.603)	(0.101)	(0.807)	(0.719)	(0.699)	(0.673)
Control mean	17	3	18	4	4	4	4
Total obs.	5,930	5,943	5,943	5,930	5,930	5,930	5,930
Zeros replaced	4	0	0	0	0	0	0

Notes: *p < 0.1; **p < 0.05; ***p < 0.01. Obs. = observations. All specifications include PHC-time fixed effects. For outcomes expressed in logs, -Inf replaced with 0. Control mean shown in levels. Standard errors clustered at the village level.

Table 12: Communication experiment – restricted to incentive set

	Dependent variable						
	Log vaccine given	Fully immunised	Log children	Shot penta-1	Shot penta-2	Shot penta-3	Shot measles-1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Random	0.032	-0.736	0.0002	-0.505	-0.682	-0.776	-0.647
	(0.098)	(0.526)	(0.099)	(0.718)	(0.668)	(0.620)	(0.588)
Gossip	0.026	0.852	0.023	0.142	-0.112	0.134	0.870
	(0.099)	(0.587)	(0.101)	(0.761)	(0.675)	(0.638)	(0.653)
Trusted	-0.001	-0.465	-0.012	-0.999	-0.836	-0.680	-0.755
	(0.095)	(0.590)	(0.095)	(1.125)	(0.854)	(0.814)	(0.810)
Trusted gossip	-0.038	-1.144*	-0.062	-1.411*	-1.387*	-1.320*	-1.132
	(0.104)	(0.615)	(0.106)	(0.830)	(0.805)	(0.757)	(0.689)
Control mean	20	4	21	5	5	4	4
Total obs.	5,609	5,624	5,624	5,609	5,609	5,609	5,609
Zeros replaced	9	0	0	0	0	0	0

Notes: *p < 0.1; **p < 0.05; ***p < 0.01. Obs. = observations. All specifications include PHC-time fixed effects. For outcomes expressed in logs, -Inf replaced with 0. Control mean shown in levels. Standard errors clustered at the village level.

Reminders experiment

Overall, the reminders experiment is disappointing, partly because (by design) it affected only children who had attended the camp at least once. At an aggregated SC level, we see a tendency toward a positive effect overall (Table 13) and the sub-treatment analysis shows that this is attributable to the 33 per cent treated villages for measles and fully immunised (Table 14).

However, the more pertinent analysis, given the design of this experiment, is at the child level. Here, we don't see any significant effect of being reminded (Table 15), and only a weak effect in the isolated sample of 33 per cent treated villages (with regard to receiving the measles shot in particular, which entails a special reminder [Table 16]). Furthermore, we don't see any significant spillover effects.

Table 13: Reminders experiment – pooled effect at SC-month level

	Dependent variable						
	Log vaccine given	Fully immunised	Log children	Log shot penta-1	Log shot penta-2	Log shot penta3	Shot measles-1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reminders	0.047	0.338*	0.050	0.056	0.069*	0.060	0.393*
	(0.038)	(0.185)	(0.038)	(0.038)	(0.040)	(0.041)	(0.203)
Control mean	37	3	36	10	9	8	4
Total obs.	6,515	6,517	6,517	6,515	6,515	6,515	6,515
Zeros replaced	0	0	0	95	93	117	0

Notes: *p < 0.1; **p < 0.05; ***p < 0.01. Obs. = observations. All specifications include district-time fixed effect and. a full set of controls for incentives. For outcomes expressed in logs, -Inf replaced with 0. Control mean shown in levels. Standard errors clustered at the SC level.

Table 14: Reminders experiment – treatment-wise effect at SC-month level

	Dependent variable						
	Log vaccine given	Fully immunised	Log children	Log shot penta-1	Log shot penta-2	Log shot penta-3	Shot measles-1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
33% first impl.	0.055	0.511**	0.059	0.062	0.076	0.071	0.596**
	(0.044)	(0.229)	(0.044)	(0.045)	(0.046)	(0.047)	(0.248)
66% first impl.	0.038	0.160	0.041	0.049	0.062	0.048	0.184
	(0.043)	(0.217)	(0.044)	(0.044)	(0.046)	(0.047)	(0.236)
Control mean	37	3	36	10	9	8	4
Total obs.	6,515	6,517	6,517	6,515	6,515	6,515	6,515
Zeros replaced	0	0	0	95	93	117	0

Notes: *p < 0.1; **p < 0.05; ***p < 0.01. Obs. = observations; first impl. = first implementation. All specifications include district-time fixed effects. All specifications include a full set of controls for incentives. For outcomes expressed in logs, -Inf replaced with 0. Control mean shown in levels. Standard errors clustered at the SC level.

Table 15: Reminders experiment – individual-level including spillover effects

Dependent variable						
	Vaccine given	Fully immunised	Shot penta-1	Shot penta-2	Shot penta-3	Shot measles-1
	(1)	(2)	(3)	(4)	(5)	(6)
Reminders	0.026 (0.027)	-0.002 (0.016)	0.005 (0.006)	0.011 (0.010)	0.008 (0.013)	0.006 (0.017)
Reminders *ind. treat.	0.001 (0.010)	0.006 (0.006)	-0.002 (0.004)	-0.005 (0.004)	0.002 (0.005)	0.007 (0.006)
Control mean	2.1	0.42	0.59	0.57	0.58	0.48
Total obs.	121026	55229	117875	111155	101498	55229
Zeros replaced	0	0	0	0	0	0

Notes: *p < 0.1; **p < 0.05; ***p < 0.01. Obs. = observations; ind. treat. = individual treatment. All specifications include PHC fixed effects. For outcomes expressed in logs, -Inf replaced with 0. Control mean shown in levels. Standard errors clustered at the SC level.

Table 16: Reminders experiment – treatment arm including spillover effects

			Dependent variable			
	Vaccine given	Fully immunised	Shot penta-1	Shot penta-2	Shot penta-3	Shot measles-1
	(1)	(2)	(3)	(4)	(5)	(6)
33% first impl.	0.023 (0.032)	-0.0004 (0.018)	0.004 (0.007)	0.010 (0.012)	0.010 (0.015)	0.006 (0.019)
66% first impl.	0.030 (0.030)	-0.004 (0.018)	0.005 (0.007)	0.012 (0.011)	0.005 (0.015)	0.007 (0.019)
33% * ind. treat.	-0.001 (0.010)	0.012* (0.007)	-0.003 (0.005)	-0.007 (0.005)	0.008 (0.005)	0.013** (0.007)
66% * ind. treat.	0.001 (0.011)	0.005 (0.008)	-0.002 (0.005)	-0.005 (0.005)	0.0001 (0.005)	0.003 (0.008)
Control mean	2.09	0.41	0.6	0.56	0.57	0.47
Total obs.	121,026	55,229	117,875	111,155	101,498	55,229
Zeros replaced	0	0	0	0	0	0

Notes: *p < 0.1; **p < 0.05; ***p < 0.01. First impl. = first implementation; Ind. treat. = individual treatment; obs. = observations. All specifications include PHC fixed effects. For outcomes expressed in logs, -Inf replaced with 0. Control mean shown in levels. Standard errors clustered at the SC level.

Drop-out rate

Tables 17 and 18 are individual-level regressions for the incentives and communication treatments, respectively, conditional on receiving the penta-1 vaccine. They capture the effect of treatment on vaccine continuation from penta-2 through measles-1 conditional on receiving penta-1.

Table 17: Continuation rate for incentive experiment (child received vaccine X conditional on getting penta-1)

	Dependent variable		
	Shot penta-2	Shot penta-3	Shot measles-1
	(1)	(2)	(3)
High slope	-0.002	-0.003	-0.034*
	(0.016)	(0.017)	(0.018)
High flat	-0.048**	-0.048***	-0.056**
	(0.019)	(0.018)	(0.022)
Low slope	-0.029**	-0.040**	-0.039**
	(0.015)	(0.016)	(0.019)
Low flat	-0.002	0.003	-0.009
	(0.016)	(0.018)	(0.020)
Control mean	0.69	0.56	0.5

Note: *p < 0.1; **p < 0.05; ***p < 0.01.

Table 18: Continuation rate for communication experiment

	Dependent variable		
	Shot penta-2	Shot penta-3	Shot measles-1
	(1)	(2)	(3)
Random	-0.010	-0.020	0.001
	(0.010)	(0.013)	(0.016)
Gossip	0.007	0.009	0.033**
	(0.013)	(0.014)	(0.017)
Trusted	0.013	0.016	0.031
	(0.014)	(0.015)	(0.020)
Trusted gossip	-0.015	-0.021*	-0.005
	(0.012)	(0.012)	(0.016)
Control mean	0.7	0.59	0.52

Note: *p < 0.1; **p < 0.05; ***p < 0.01.

The key finding here is that that continuation rates are *lower* in the incentive group. This reflects a combination of treatment and selection. The selection effect stems from the fact that the incentives drew more people to receive at least one vaccine (penta-1). The selection effect is likely to be negative, since the people drawn by the incentives are those less interested in immunisation to start with.

The treatment effect is that, for any given person, the incentive could have encouraged them or discouraged them to receive each shot. It is likely to be positive. It could have been increasing or decreasing at each shot. The net effect here is negative, suggesting that the selection effect dominates. This differs from what we had found in Udaipur, namely a strongly positive effect on continuation rates. But the policy implication should be based on the total effect, not these coefficients.

7.2.2 Endline data

Substitution effect

The substitution effect analysis involves comparing immunisation rates for unmatched children – children not in the tablet system – between our treatment arms and the control arm, using endline data. We define 'immunisation rates' for different immunisation outcomes – from 'at least one' to 'at least seven' vaccines (which may not be a perfectly calibrated measure between children[17]), as well as a measles-1 outcome (which functions as a particularly well-calibrated proxy to detect full immunisation since it's the last relevant vaccination in the sequence).

Lower immunisation rates in unmatched children in the treatment groups could be evidence of a substitution effect between camps as a source for immunisation and other sources (e.g. private hospitals), if we observe that children who are not immunised in the camps are less likely to be immunised elsewhere.

On average, 60 per cent of unmatched children in the control group declared that they had received at least two vaccinations, which suggest that other sources of immunisation are important. A total of 39 per cent have received the measles shot.

With regard to the incentives experiment, when considering aggregated slope versus flat incentives (Table 19) we do not generally find evidence for a substitution effect (the sole exception is in the immunisation rates of 'at least six vaccines' given slope treatments). If we consider sub-treatments, we do find evidence of a substitution effect across multiple outcomes for one of the treatment arms – high slope incentives (Table 20). This is one more reason to aggregate the analysis of the experiment by 'slope' versus 'flat'.

In the communications experiment, when considering sub-treatments (Table 21) we don't generally find evidence of a substitution effect (with the exception of 'at least two vaccines' given random seed treatments and measles for trusted gossip seeds).

The reminders experiments (Table 22) does not show evidence of a significant substitution effect (the sole exception is in the immunisation rates of 'at least seven' vaccines given reminders at a 33 per cent level).

The fact that the substitution effect is only apparent in one sub-treatment arm – the high slope incentive – is a sign that substitution does not generally account for tablet data treatment effects.

[17] The reason why these immunisation rates may not be very accurate is twofold: (1) recall of the number of vaccines is not always accurate, as other studies have shown; and (2) just prior to implementation, two doses of inactivated polio vaccine were introduced across Haryana. However, these were not always consistently implemented in our programme areas. Where we do not have MCP card data, we therefore do not know whether fully immunised is 'at least five' or 'at least seven'.

Table 19: Incentives experiment, aggregated sub-treatments – unmatched children

	Dependent variable						
	At least 2	At least 3	At least 4	At least 5	At least 6	At least 7	measles-1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Slope	-0.045	0.051	0.012	-0.121	-0.148*	-0.057	-0.083
	(0.055)	(0.059)	(0.074)	(0.083)	(0.078)	(0.077)	(0.084)
Flat	-0.022	0.013	-0.061	-0.121	-0.047	0.002	0.008
	(0.060)	(0.063)	(0.080)	(0.124)	(0.118)	(0.127)	(0.110)
Control mean	0.69	0.55	0.4	0.31	0.17	0.11	0.39
Total obs.	1,199	1,185	1,185	1,061	1,061	718	615
Zeros replaced	0	0	0	0	0	0	0

Notes: *p < 0.1; **p < 0.05; ***p < 0.01. Obs. = observations. All specifications include district fixed effects. All specifications include a full set of controls for communication and reminders. For outcomes expressed in logs, -Inf replaced with 0. Control mean shown in levels. Standard errors clustered at the village (SC) level.

Table 20: Incentives experiment, all sub-treatments – unmatched children

	Dependent variable						
	At least 2	At least 3	At least 4	At least 5	At least 6	At least 7	measles-1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High slope	-0.163***	-0.058	-0.075	-0.193*	-0.171*	-0.028	-0.148
	(0.062)	(0.071)	(0.091)	(0.104)	(0.099)	(0.101)	(0.103)
High flat	-0.026	-0.030	-0.099	-0.075	-0.040	0.106	0.182
	(0.087)	(0.061)	(0.076)	(0.154)	(0.155)	(0.165)	(0.142)
Low slope	0.087	0.171***	0.109	-0.015	-0.117	-0.070	0.044
	(0.062)	(0.059)	(0.084)	(0.097)	(0.080)	(0.077)	(0.098)
Low flat	0.005	0.069	-0.013	-0.138	-0.046	-0.090	-0.141
	(0.074)	(0.093)	(0.119)	(0.179)	(0.173)	(0.168)	(0.132)
Control mean	0.69	0.55	0.4	0.31	0.17	0.11	0.39
Total obs.	1,199	1,185	1,185	1,061	1,061	718	625
Zeros replaced	0	0	0	0	0	0	0

Notes: *p < 0.1; **p < 0.05; ***p < 0.01. Obs. = observations. All specifications include district fixed effects. All specifications include a full set of controls for communication and reminders. For outcomes expressed in logs, -Inf replaced with 0. Control mean shown in levels. Standard errors clustered at the village (SC) level.

Table 21: Communications experiment, all sub-treatments – unmatched children

	Dependent variable						
	At least 2	At least 3	At least 4	At least 5	At least 6	At least 7	measles-1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Random	-0.097*	-0.039	0.006	-0.141	0.002	0.068	-0.003
	(0.057)	(0.065)	(0.086)	(0.122)	(0.103)	(0.103)	(0.099)
	-0.035	-0.116	-0.032	-0.111	-0.003	-0.081	-0.060
	(0.081)	(0.078)	(0.110)	(0.133)	(0.123)	(0.077)	(0.116)
	0.031	-0.035	0.097	0.028	0.123	0.007	0.136
	(0.069)	(0.070)	(0.096)	(0.126)	(0.118)	(0.107)	(0.103)
	-0.097	-0.076	-0.036	-0.208*	-0.113	-0.045	-0.316***
	(0.075)	(0.078)	(0.097)	(0.111)	(0.085)	(0.083)	(0.094)
Control mean	0.78	0.67	0.51	0.59	0.31	0.22	0.61
Total obs.	477	469	469	397	397	255	235
Zeros replaced	0	0	0	0	0	0	0

Notes: *p < 0.1; **p < 0.05; ***p < 0.01. Obs. = observations. All specifications include district fixed effects. All specifications include a full set of controls for communication and reminders. For outcomes expressed in logs, -Inf replaced with 0. Control mean shown in levels. Standard errors clustered at the village (SC) level.

Table 22: Reminders experiment, all sub-treatments – unmatched children

	Dependent variable						
	At least 2	At least 3	At least 4	At least 5	At least 6	At least 7	measles-1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
33% first impl.	0.078	0.093	0.073	0.035	0.012	-0.134*	0.005
	(0.057)	(0.064)	(0.083)	(0.101)	(0.092)	(0.078)	(0.084)
66% first impl.	0.034	0.099*	0.048	-0.077	-0.074	-0.048	-0.103
	(0.052)	(0.055)	(0.068)	(0.092)	(0.080)	(0.068)	(0.083)
Control mean	0.64	0.48	0.34	0.28	0.19	0.13	0.45
Total obs.	1,199	1,185	1,185	1,061	1,061	718	625
Zeros replaced	0	0	0	0	0	0	0

Notes: *p < 0.1; **p < 0.05; ***p < 0.01. First impl. = first implementation; obs. = observations. All specifications include district fixed effects. All specifications include a full set of controls for incentives and communication. For outcomes expressed in logs, -Inf replaced with 0. Control mean shown in levels. Standard errors clustered at the village (SC) level.

Comparison between immunised and non-immunised children

We ran an analysis of the immunisation outcomes over individual demographic features of the endline population to understand how these characteristics compare between immunised and non-immunised children. In this analysis, we controlled for all incentive, communication and reminder treatments.

The following characteristics are linked to children having significantly higher immunisation outcomes:

- A father or mother with education above class 10;
- Being Hindu;
- The availability of electricity;
- Having a greater number of rooms in the house; and
- The child's family owning their house.

The following characteristics are linked to children having significantly lower immunisation outcomes:

- Being Muslim;
- Being from Mewat;
- Having an Aadhaar card; and
- The household reporting that they cut back on food.

Please refer to Appendix H3 for all outcome tables for this analysis.

Treatment effects on knowledge and attitudes

We ran an analysis to see whether there were any treatment effects on knowledge and attitude outcomes, using data collected through the endline survey. For the communications and reminders experiment, which involve information transfer, we might expect a positive impact on the knowledge of primary caregivers regarding immunisation. In addition, for the communications experiment, we might expect a positive impact on attitudes toward immunisation – people may more readily believe information coming from people in the community.

For the incentives and reminders experiments, we might expect that people have better knowledge of the immunisation schedule. We would not, however, necessarily expect them to have a more positive attitude about immunisation, as these interventions did not seek to change attitudes. We ran an analysis of aggregated treatments effects on three sets of general outcomes: (1) knowledge of immunisation camps; (2) knowledge of immunisation; and (3) attitudes towards immunisation. We present the main findings below. Please refer to Appendix H3 for all outcome tables for this analysis.

Knowledge of immunisation camps

This includes the following outcomes: being aware of camps, accurately assessing camp frequency, and informing others about the camps. We found no significant results in these knowledge outcomes from treatment in any of the experiments.

Knowledge of immunisation

This includes the following outcomes: relying on personal experience to understand vaccination, accurately assessing the number of recommended vaccines, and sharing the same view of vaccines as at least some other proximate individuals (friends, neighbours, fellow villagers).

In the incentives experiment, we found that the incentive treatment group is less driven by personal experience, more accurate in their assessment of the number of recommended vaccines, and more to likely share the same views towards vaccination as some proximate individuals.

In the communications experiment, we found some evidence that the random seed treated group is less accurate in their assessment of the number of recommended vaccines, and that the nominated seed treated group relies less on personal experience to understand vaccination.

In the reminders experiment, we found that the treated group relies less on personal experience to understand vaccination.

Attitudes towards immunisation
These include the outcomes of believing vaccines are on the whole beneficial, or that vaccines are on the whole harmful. (Note that these outcomes are not logical inverses of each other; for example, one can indicate that they don't believe vaccines are beneficial while also separately indicating that they don't believe they are harmful.)

In the incentives experiment, we didn't find any significant differences in attitude outcomes.

In the communications experiment, the nominated seed treated group is more likely to believe that vaccines are overall beneficial. The random seed treated group is both more likely to believe that vaccines are beneficial and that vaccines are harmful (possible again because these outcomes are separate and not logically opposite).

In the reminders experiment, we didn't find any significant differences in attitude outcomes.

7.2.3 Heterogeneity of impacts
We tested the differential impact of treatment across several factors. To establish these factors for the relevant subpopulations (village and SC), we often needed to avail ourselves of baseline and 2011 national census data.

Each heterogeneous factor (except high risk area, gender and religion) was analysed by comparing the relative treatment effects from two separate analyses – one for subpopulations associated with the 'low' level of that factor, and one for subpopulations associated with the 'high' level of the factor, where 'low' and 'high' are defined according to the median of the factor in question. Comparing these effects helps us to discover if treatment impact differed according to the factor.

Overall, there are many different results emerging from the heterogeneity analysis, none of which stand out particularly. There is some suggestion that areas with higher prevalence of immunisation cards and those with higher previous vaccines or injections respond more positively to incentives. However, these findings should be investigated further. We are planning to do a fuller analysis using machine learning in the future.

Please refer to Appendix H4 for the complete set of tables from the heterogeneity analysis.

7.3 Qualitative results

7.3.1 ANM focus groups
Here we report the key findings from the ANM focus groups in terms of ANMs' perceptions of the programme's impact (focusing specifically on incentives, given that ANMs were aware of this treatment) in order to offer some qualitative explanation of the

pattern of results described above. Note that the findings are perceptions of one type of stakeholder, which needs to be triangulated with the perceptions of others, such as primary caregivers themselves. (For a fuller set of findings, please refer to Appendix I.)

Overall, ANMs feel the programme has been able to attract more primary caregivers to immunisation camps and has made them obtain vaccinations on time. Most people were reported to be aware of the importance of immunisation prior to the programme, but ANMs believe that the promise of a reward at the end of the process has helped to motivate the traditionally resistant, and neutralise complaints of long wait times and fever from vaccines.

Interestingly, some ANMs attribute part of the success of the programme to the tablets themselves, as primary caregivers now feel the information is legitimised. These findings support our results from the outcome analysis and our theory of change: that there is no need to change attitudes, but rather to ‘push’ people over the hump. This would also help to explain why gossip seeds work best, and why trusted sources of information are perhaps unnecessary.

ANMs do feel, however, that there remain difficult-to-reach populations such as migrant workers and daily wagers. The perception is that for these minority groups the programme (specifically incentives) was not able to provide a sufficient push. Some ANMs suggest that concerns about fever remain paramount, and for daily wagers, the concern is the loss of money.

A slightly different minority population are Muslims. Especially in Mewat, some ANMs state that primary caregivers from Muslim-majority areas remain unmoved due to religious reasons. This confirms the finding from the endline that children in Muslim households are less likely to be immunised and suggests resistance due to beliefs and attitudes. However, some ANMs believe that the incentives may have had some effect if not for the sterilisation rumours that spread periodically.

7.3.2 Caregiver interviews

Here we present the main findings from the caregiver interviews, focusing on knowledge, attitudes and behaviour around immunisation, and linking this back to the theory of change of the three interventions this study aimed to evaluate. (For a fuller set of findings, please refer to Appendix K.)

Knowledge, attitudes and behaviour

Overall, it seems that the decision whether to immunise a child is determined by a combination of different types of barriers coming together (or not), many of which are not necessarily obvious to the primary caregiver because they are not related to knowledge and attitudes. The topic of immunisation is uncomplicated for many primary caregivers; they don’t think about it very consciously and don’t have strong opinions on the subject. They attend camps because they are there, because they are recruited by ASHAs to come, and because ‘it must be good’.

These findings generally support the underlying assumptions in our theory of change. Except for a few cases, we find no strong attitudinal resistance to immunisation. Yet, there is disagreement in terms of how important immunisation really is. In a number of cases, there is clear evidence of long-term benefits not outweighing the short-term costs and inconveniences, which are almost always related to side effects.

The concern around side effects – held even by those who value immunisation – acts as an almost destabilising factor, with subsequent other environmental and socio-demographic factors 'tipping' the scales to decide whether to immunise their child. We find evidence that for some primary caregivers, opportunity costs tip their decision toward not immunising their child, either in terms of time or money or sometimes related directly to the consequences of dealing with vaccine-related side effects.

We also find evidence that for some primary caregivers, others' beliefs and behaviours tips the scale, especially if these are people that the primary caregivers trust or their beliefs and/or behaviours are widely shared by the community.

Incentives
Most primary caregivers expressed positive opinions about recharges. Some admitted to finding them motivating or influencing their decision to immunise their child (whether or not they actually received them). This was linked to generally liking free things ('What's not to like?'), their self-assessed poverty level, or their trust in the government 'investing in them'. One primary caregiver explicitly stated that the incentives made her overcome her hesitation toward immunisation due to the fear of side effects. This provides qualitative evidence that the nudging effect of incentives, as outlined in the theory of change, does indeed occur.

Most, however, did not admit that they felt incentives to be a motivating factor for themselves, stating that it is not about the money but the benefits of immunisation. A similar moral argument was made with respect to others: that only those who are unaware or uneducated, or 'greedy' would respond to incentives, because they don't understand the real value of immunisation. It is not a stretch to assume that it is socially desirable to deny that incentives would have an effect. It is further possible that, as a result of responding to incentives, primary caregivers have justified their motivation for attending immunisation camps.

There were primary caregivers unmoved by incentives in the treatment group, but they either missed vaccines or didn't immunise their children at all. The barriers that they cited were similar to the general barriers identified for the entire set of respondents, although these primary caregivers did seem to have at least some degree of underlying concern about side effects. Only two respondents were overwhelmingly positive about immunisation, and they stated that they missed a vaccine due to lack of time and the ASHA not coming to remind them.

While it was difficult to establish a clear causal pathway for the incentives on immunisation behaviour from the caregiver interviews, there is evidence that incentives are liked in principle, and that they have had an impact on at least some primary caregivers (as assumed in our theory of change).

It seems that incentives won't work where there are either strong negative views around immunisation (usually linked to side effects), or some degree of underlying concern around side effects **plus** indifference to the importance of immunisation **and/or** an environmental or socio-demographic barrier. This again aligns with our theory of change: we expect that incentives, acting as a nudge, will have an impact on those uncertain about immunisation, or who face either issues of inconvenience or salience. We do not expect them to have an impact on primary caregivers who hold strong, entrenched negative attitudes or those constrained by wider socio-demographic issues.

Communications
A key finding from the caregiver interviews is that what other people say or do is an important source of primary caregivers' knowledge, attitudes and behaviour around immunisation. A range of people is cited by primary caregivers as influential. ASHAs and ANMs are clearly important in terms of creating awareness and knowledge of immunisation and immunisation camps, and many primary caregivers are heavily dependent on them.

For some, the ANM is also important in shaping their attitude towards immunisation, which is linked to trust in either the ANM herself and/or the authority of the government. Other members of the household are influential, with clear cases of others either pushing for or stopping the decision to immunise the child, especially elder family members and family by marriage, such as sisters-in-law and mothers-in-law.

With respect to the wider community, primary caregivers seem influenced especially by what other mothers are saying and doing, as well as people with positions of power or respect in the community, such as religious leaders or the village *sarpanch*. There is evidence that there are sometimes key mobilisers in the community – people that are identified by primary caregivers by name and position.

However, there is also evidence that the influence of the community is not entirely clear or direct. There is a sense that if everyone says it is important, or if everyone is going to the immunisation camp, then it must be good. In most cases, knowledge is disseminated by a vaguely defined collective or majority, and this affects attitudes and behaviour. In two cases, primary caregivers reported widespread discussion of incentives in the community, citing this as a factor that affected their own opinion and decision around immunisation.

These findings support the underlying assumptions of our theory of change for the communications experiment, namely that information is spread through social networks in a diffused manner, rather than a direct, targeted manner. It is a matter of stirring up conversation. We would therefore not necessarily expect primary caregivers to be able to name specific people as sources of information about immunisation.

Similarly, we would not expect people who disseminate information to remember to whom they spoke. This was confirmed by the very brief interviews we conducted with nominated gossip seeds: where they remembered receiving information about immunisation, they could not name specific people to whom they had passed information, and stated they just mentioned it to people 'around the village'.

7.4 Cost-effectiveness analysis

For this programme, we conducted a cost-effectiveness analysis (CEA): a comparison of the incremental cost per unit of effectiveness (or outcome) across different treatment arms.[18] As such, we used the CEA methodology as defined and developed by J-PAL. This analysis was only conducted for the treatment arms, and interventions showed a statistically significant effect from the impact analysis. However, costs were collected and organised for all treatment arms. Please refer to Appendix J1 for a description of the CEA methodology.

It is important to note that while we have reported all costs incurred over the course of implementation of the interventions, we have excluded those pertaining to operating the m-Health platform (software development, tablets, SIM cards, server costs) in the subsequent CEA. This is because such infrastructure would presumably exist when governments are considering successful interventions for scale-up. Depending on the context and purpose, cost packages can be varied to generate more appropriate cost-effectiveness estimates from our programme (Appendix J).

7.4.1 Cost of the programme

Two sets of costs were included in the calculation of the total cost of the programme: (1) the actual costs incurred in implementing the programme by J-PAL; and (2) the estimated costs that the government incurred through its routine and intensified immunisation programmes in our sample areas. Despite our efforts, we were unable to obtain data on direct spending from the government (Section 7.4.4).

As such, we imputed the marginal costs the government incurs based on assumptions we have about the unit costs: the cost of the vaccines, the alternative vaccine delivery and the ASHA incentives. These costs vary almost proportionately to the number of children attending immunisation camps and constitute the major costs the government incurs.

Since April 2016, **USD805,790** was spent on implementing just those interventions being evaluated. When including costs incurred by the government during the implementation period, this increases to a total of **USD1,887,450**. Table 23 shows a detailed breakdown of the costs incurred using the J-PAL standard ingredients method – both for the programme overall and each treatment arm that was found to have a significant impact.

The main differences between the costs of the different treatment arms in Table 23 are driven by the fact that the treatment arms are of different sizes. In addition, the incentives treatment arms include the cost of mobile recharges, which form 27 per cent of the total cost of the high slope incentive arm and 18 per cent of the total cost of the low slope incentive arm.

Please refer to Appendix J2 for the excel version of this table, with further descriptions.

[18] CEA is different to other cost analyses such as cost-benefit analysis. While the former shows the amount of 'effect' achieved by the intervention for one outcome measure at a given cost, the latter combines all the different benefits of an intervention or programme onto a monetary scale, showing the ratio of combined benefits to cost (Dhaliwal et al. 2013). Cost-benefit analysis allows an absolute judgement as to whether a programme is worth the investment, as is takes into account multiple outcomes on the same scale. However, it is also much more difficult to agree on the assumptions of the monetary value of benefits across different stakeholders (Ibid.).

Table 23: Total cost of the programme

Programme ingredient	Programme overall (755 SCs, 2,360 villages, 219,992 children)			High slope (120 SCs, 370 villages, 38,598 children)			Low slope (120 SCs, 424 villages, 39,778 children)			Gossip seed (130 villages, 17,253 children)		
	INR	USD		INR	USD		INR	USD		INR	USD	
Programme administration	***210,55,036***	***305,633***	***16.2%***	***3,346,496***	***48,577***	***12.8%***	***3,346,496***	***48,577***	***14.3%***	***1,160,226***	***16,842***	***15.0%***
Targeting	***894,627***	***12,986***	***0.7%***	***77,099***	***1,119***	***0.3%***	***77,099***	***1,119***	***0.3%***	***127,451***	***1,850***	***1.7%***
- IEC materials	484,252	7,029	0.4%	77,099	1,119	0.3%	77,099	1,119	0.3%	26,684	387	0.3%
- Nominations survey	410,375	5,957	0.3%	0	0	0.0%	0	0	0.0%	100,766	1,463	1.3%
Staff training	***2,329,812***	***33,819***	***1.8%***	***366,899***	***5,326***	***1.4%***	***380,241***	***5,520***	***1.6%***	***128,286***	***1,862***	***1.7%***
- J-PAL staff training	117,273	1,702	0.1%	18,468	268	0.1%	19,140	278	0.1%	6,457	94	0.1%
- Master training	275,022	3,992	0.2%	43,311	629	0.2%	44,885	652	0.2%	15,143	220	0.2%
- ANM training	1,937,517	28,125	1.5%	305,121	4,429	1.2%	316,216	4,590	1.4%	106,685	1,549	1.4%
User training												
Implementation costs	***30,930,415***	***448,983***	***23.8%***	***9,319,604***	***135,282***	***35.6%***	***6,411,955***	***93,075***	***27.5%***	***797,049***	***11,570***	***10.3%***
- Mobile recharges	16,712,390	242,595	12.9%	7,056,800	102,436	26.9%	4,147,620	60,206	17.8%	0	0	0.0%
- Congrats messages & IVRs	378,660	5,497	0.3%	65,539	951	0.3%	66,891	971	0.3%	29,272	425	0.4%
- ANM messages	31,405	456	0.0%	4,946	72	0.0%	5,126	74	0.0%	1,731	25	0.0%
- Seeds messages	14,623	212	0.0%	0	0	0.0%	0	0	0.0%	5,972	87	0.1%
- Tablets & SIM cards	10,695,593	155,256	8.2%	1,699,962	24,676	6.5%	1,699,962	24,676	7.3%	589,375	8,555	7.6%
- App development cost	2,030,900	29,480	1.6%	322,792	4,686	1.2%	322,792	4,686	1.4%	111,912	1,624	1.5%
- Server costs	515,133	7,478	0.4%	81,875	1,188	0.3%	81,875	1,188	0.4%	28,386	412	0.4%
- Other costs	551,711	8,009	0.4%	87,689	1,273	0.3%	87,689	1,273	0.4%	30,402	441	0.4%
User costs												
Averted costs												
Monitoring costs	***301,000***	***4,369***	***0.2%***	***47,841***	***694***	***0.2%***	***47,841***	***694***	***0.2%***	***16,586***	***241***	***0.2%***
Government costs	***74,515,518***	***1,081,659***	***57.3%***	***13,051,712***	***189,457***	***49.8%***	***13,091,516***	***190,035***	***56.1%***	***5,487,387***	***79,654***	***71.1%***
- ASHA incentives for mobilisation	6,808,700	98,834	5.2%	1,082,177	15,709	4.1%	1,082,177	15,709	4.6%	375,190	5,446	4.9%
- ASHA incentives for FIC	12,447,750	180,690	9.6%	2,250,450	32,667	8.6%	2,263,950	32,863	9.7%	1,017,300	14,767	13.2%
- Alternative vaccine delivery	5,106,525	74,126	3.9%	811,633	11,782	3.1%	811,633	11,782	3.5%	281,392	4,085	3.6%
- Vaccines	48,991,098	711,150	37.7%	8,699,237	126,277	33.2%	8,726,773	126,677	37.4%	3,726,595	54,095	48.3%
- Medical supplies	1,161,445	16,859	0.9%	208,215	3,022	0.8%	206,983	3,005	0.9%	86,910	1,262	1.1%
TOTAL (without government costs)	***55,510,890***	***805,790***		***13,157,939***	***190,999***		***10,263,633***	***148,986***		***2,229,598***	***32,365***	
TOTAL (with government costs)	***130,026,408***	***1,887,450***		***26,209,651***	***380,457***		***23,355,149***	***339,021***		***7,716,984***	***112,019***	

Notes: IVRs = incentive voice call reminders. FIC = Fully immunised children All costs have been converted from INR to USD, using an exchange rate of 6INR8.89 to USD1 or USD0.015 to INR1. Percentages are based on the programme plus total government costs. User training costs, user costs and averted costs were not judged to be incurred by the programme – see Appendix J2 for further detail.

Table 24: Total cost of the programme (incentives intervention)

Programme ingredient	Incentives control (369 SCs, 1,117 villages, 101,041 children)			High slope (120 SCs, 370 villages, 38,598 children)			Low slope (120 SCs, 424 villages, 39,778 children)		
	INR	**USD**		**INR**	**USD**		**INR**	**USD**	
Programme administration	10,290,475	149,375	19.2%	3,346,496	48,577	12.8%	3,346,496	48,577	14.3%
Targeting	235,747	3,422	0.4%	77,099	1,119	0.3%	77,099	1,119	0.3%
- IEC materials	235,747	3,422	0.4%	77,099	1,119	0.3%	77,099	1,119	0.3%
- Nominations survey	0	0	0.0%	0	0	0.0%	0	0	0.0%
Staff training	1,130,717	16,413	2.1%	366,899	5,326	1.4%	380,241	5,520	1.6%
- J-PAL staff training	56,916	826	0.1%	18,468	268	0.1%	19,140	278	0.1%
- Master training	133,475	1,938	0.2%	43,311	629	0.2%	44,885	652	0.2%
- ANM training	940,327	13,650	1.8%	305,121	4,429	1.2%	316,216	4,590	1.4%
User training									
Implementation costs	6,934,615	100,662	12.9%	9,319,604	135,282	35.6%	6,411,955	93,075	27.5%
- Mobile recharges	0	0	0.0%	7,056,800	102,436	26.9%	4,147,620	60,206	17.8%
- Congrats messages & IVRs	177,994	2,584	0.3%	65,539	951	0.3%	66,891	971	0.3%
- ANM messages	15,242	221	0.0%	4,946	72	0.0%	5,126	74	0.0%
- Seeds messages	0	0	0.0%	0	0	0.0%	0	0	0.0%
- Tablets & SIM cards	5,227,383	75,880	9.8%	1,699,962	24,676	6.5%	1,699,962	24,676	7.3%
- App development cost	992,586	14,408	1.9%	322,792	4,686	1.2%	322,792	4,686	1.4%
- Server costs	251,767	3,655	0.5%	81,875	1,188	0.3%	81,875	1,188	0.4%
- Other costs	269,644	3,914	0.5%	87,689	1,273	0.3%	87,689	1,273	0.4%
User costs									
Averted costs									
Monitoring costs	147,111	2,135	0.3%	47,841	694	0.2%	47,841	694	0.2%
Government costs	34,859,781	506,021	65.0%	13,051,712	189,457	49.8%	13,091,516	190,035	56.1%
- ASHA incentives for mobilisation	3,327,696	48,304	6.2%	1,082,177	15,709	4.1%	1,082,177	15,709	4.6%
- ASHA incentives for FIC	5,822,700	84,522	10.9%	2,250,450	32,667	8.6%	2,263,950	32,863	9.7%
- Alternative vaccine delivery	2,495,772	36,228	4.7%	811,633	11,782	3.1%	811,633	11,782	3.5%
- Vaccines	22,680,170	329,223	42.3%	8,699,237	126,277	33.2%	8,726,773	126,677	37.4%
- Medical supplies	533,444	7,743	1.0%	208,215	3,022	0.8%	206,983	3,005	0.9%
TOTAL (programme with government costs, without tablet costs)	**47,126,712**	684,086		**24,105,022**	349,906		**21,250,519**	308,470	

Note: IVRs = incentive voice call reminders. FIC = Fully immunised children

Table 25: Total cost of the programme (networks intervention)

Programme ingredient	Communication control (383 villages, 42,740 children)			Gossip seed (130 villages, 17,253 children)		
	INR	USD		INR	USD	
Programme administration	3,420,913	49,658	16.8%	1,160,226	16,842	15.0%
Targeting	78,679	1,142	0.4%	127,451	1,850	1.7%
- IEC materials	78,679	1,142	0.4%	26,684	387	0.3%
- Nominations survey	0	0	0.0%	100,766	1,463	1.3%
Staff training	378,316	5,492	1.9%	128,286	1,862	1.7%
- J-PAL staff training	19,043	276	0.1%	6,457	94	0.1%
- Master training	44,658	648	0.2%	15,143	220	0.2%
- ANM training	314,615	4,567	1.5%	106,685	1,549	1.4%
User training						
Implementation costs	2,325,235	33,753	11.4%	797,049	11,570	10.3%
- Mobile recharges	0	0	0.0%	0	0	0.0%
- Congrats messages & IVRs	79,063	1,148	0.4%	29,272	425	0.4%
- ANM messages	5,103	74	0.0%	1,731	25	0.0%
- Seeds messages	0	0	0.0%	5,972	87	0.1%
- Tablets & SIM cards	1,737,764	25,225	8.6%	58,9375	8,555	7.6%
- App development cost	329,970	4,790	1.6%	111,912	1,624	1.5%
- Server costs	83,696	1,215	0.4%	28,386	412	0.4%
- Other costs	89,639	1,301	0.4%	30,402	441	0.4%
User costs						
Averted costs						
Monitoring costs	48,905	710	0.2%	16,586	241	0.2%
Government costs	14,066,649	204,190	69.2%	5,487,387	79,654	71.1%
- ASHA incentives for mobilisation	1,106,242	16,058	5.4%	375,190	5,446	4.9%
- ASHA incentives for FIC	2,497,500	36,253	12.3%	1,017,300	14,767	13.2%
- Alternative vaccine delivery	829,682	12,044	4.1%	281,392	4,085	3.6%
- Vaccines	9,415,041	136,668	46.3%	3,726,595	54,095	48.3%
- Medical supplies	218,185	3,167	1.1%	86,910	1,262	1.1%
TOTAL (programme with government costs, without tablet costs)	**18,167,266**	263,714		**6,987,312**	101,427	

Note: IVRs = incentive voice call reminders. FIC = Fully immunised children

7.4.2 Impact data

As described previously, we only evaluated the cost-effectiveness of the treatment arms that have an impact. The results from the main analysis show that three interventions have a clear, positive significant impact on key immunisation outcomes, compared to control:

1. High slope incentives;
2. Low slope incentives; and
3. Gossip seeds.

While the outcome analysis was conducted on a number of immunisation outcomes, the CEA was limited to only three outcomes of most interest:

1. Number of children who have received diphtheria, pertussis and tetanus-3 or penta-3;
2. Number of children who received measles-1; and
3. Number of children fully immunised (who have received all five key antigens before 12 months of age – BCG, penta doses 1–3 and measles-1).

Tables 26 and 27 below show the impact estimates for each of the treatment arms that were found to have a significant effect, including an estimate of the number of additional children and shots that were given in the treatment versus the comparator group.

Table 26: Impact estimates for key outcomes – incentives

		High slope		**Low slope**	
Number of SCs		*120*		*120*	
	Control mean	Coefficient (log)	Number of additional children	Coefficient (log)	Number of additional children
Number of fully immunised children	8	0.114	1,538	0.123	1,653
Number of penta-3 shots	9	0.100	1,512	0.140	2,117
Number of measles-1 shots	9	0.130	1,966	0.156	2,359

Notes: Coefficients are in log. Control mean and coefficients are at the SC-month level. The number of additional children has been calculated for a 14-month period for the number of SCs in the treatment arm.[19]

[19] The programme was launched in a staggered manner, with half of the districts starting implementation in January, and the others in March. As such, we have assumed a full implementation period of 14 months on average (February 2017–March 2018).

Table 27: Impact estimates for key outcomes – communications experiment

		Gossips	
Number of villages		*130*	
	Control mean	Coefficient (level)	Number of additional children
Number of fully immunised children	3	1.321	2,404
Number of penta-3 shots	4	1.220	2,220
Number of measles-1 shots	4	1.373	2,499

Notes: Coefficients are in levels. Control mean and coefficients are at the village-month level. The number of additional children has been calculated for a 14-month period for the number of villages in the treatment arm.

7.4.3 Discussion on cost adjustments and final estimates

Cost allocation across treatment arms

As evidenced by the table above, there is a lot of variation in the size of the different treatment arms. To ensure comparability, it is important to 'allocate' costs in a reasonable manner across these arms. Costs were broadly one of two types: costs that varied by treatment arm (for example, ANM training costs) and costs that did not vary by treatment arm (for example, server costs). Variable costs were allocated in proportion to the item that led to its variability; for example, ANM training costs were allocated in proportion to the number of ANMs in each arm. Costs that were not variable were allocated according to size of the treatment arm (i.e. number of SCs). See Appendix J1 for more allocation units used.

Costs in control arms

In addition to average costs, we also present the incremental CEA. To estimate this, we need to consider costs incurred in the control group such as 'business as usual' implementation costs. Additionally, due to varying size of the treatment arms, we also need to 'adjust' the costs in the control group relative to the size of the treatment. See Appendix J1 for a detailed explanation and J2 for calculations.

User costs and averted costs

While it is advisable to include user costs and averted costs to estimate the CEA when feasible, we have not done so in this exercise. Based on findings from our baseline, monitoring and qualitative research, we concluded that user costs are typically zero or extremely trivial. Most people walk to the immunisation sites and spend less than 30 minutes accessing the services.

Averted costs, however, are important given the nature of the diseases prevented by the vaccination. While we would like to consider these, we are unable to do so at this point due to lack of information on such costs, and the fact that any assumptions we make will be extremely speculative.

Other adjustments

The costs were incurred between mid-2016 and mid-2018. Given the extremely short period, we did not undertake an inflation adjustment. The average exchange rate from August 2018 is used to convert costs in INR to USD.

7.4.4 Results

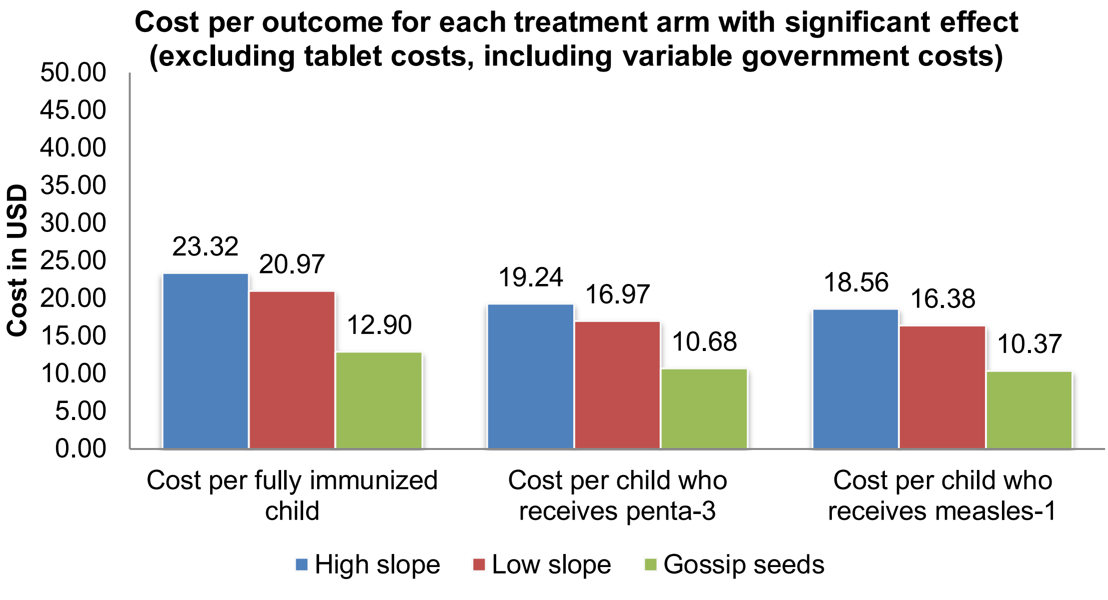
We conducted two analyses: (1) the average cost of each treatment, which indicates what was spent per unit of outcome assuming the current 'zero'-cost status quo context; and (2) the cost-effectiveness ratio, which indicates what the cost was per additional unit of outcome assuming the appropriate comparator group – in our case, the 'non-zero' cost control arm (since we invested in some costs such as ANM training and tablets).

For both analyses, we used a cost package that we believe is most representative of the purpose of this evaluation: working toward a scale-up of successful interventions. As such, we excluded the costs of app development, the tablets, and hosting the server developed and hosted for this evaluation. This was because these are all costs that the government would not need to incur if it was to scale-up any of the interventions; for example, Haryana has rolled out a tablet-based m-Health platform for immunisation. However, we included the estimated government costs, as this is what the government will continue to incur when they scale-up any of the interventions.

Average cost

Figure 11 below shows the average cost per treatment arm that was found to have a significant effect for each of the three main outcomes. The difference between the average cost of high slope and low slope incentives are not too large, while the average cost for gossip seeds is clearly lower.

Figure 11: Cost per outcome for all treatment arms



Note: For the average cost calculation we used the actual outcomes from our tablet data, rather than using our impact estimated to approximate total outcomes.

Cost-effectiveness ratio

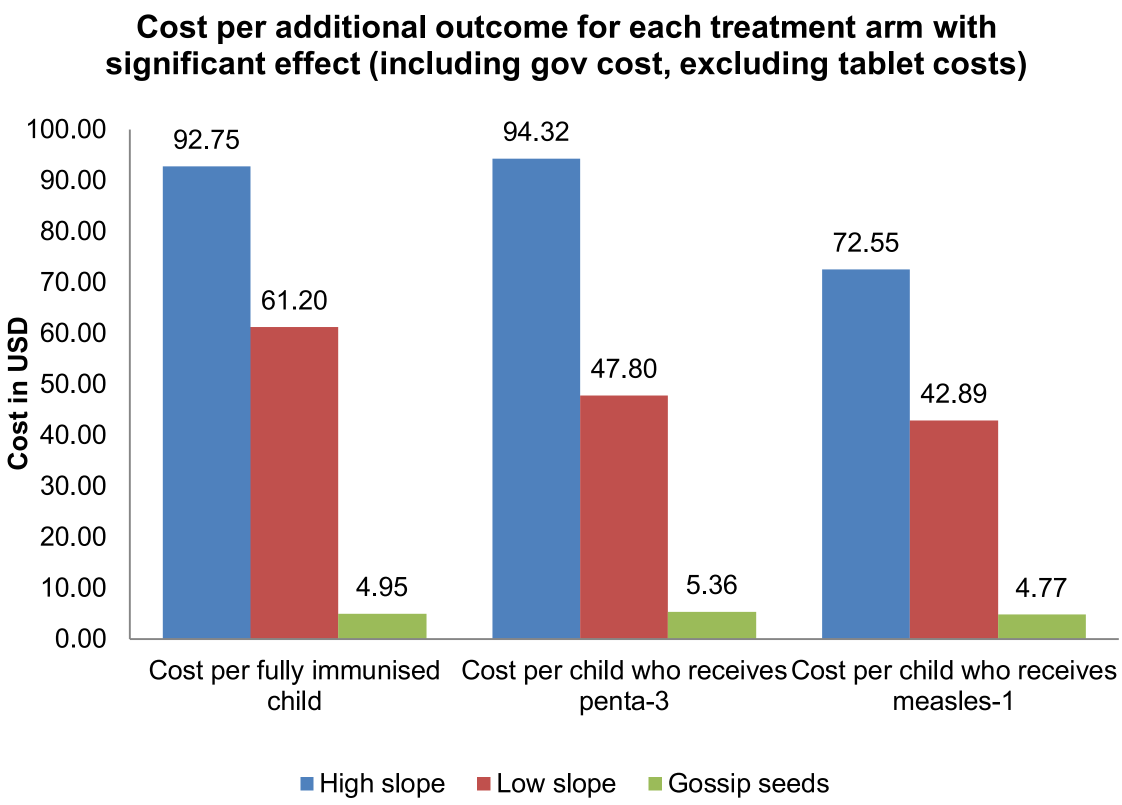
In order to evaluate the cost-effectiveness of the different treatments that showed a significant effect, we calculated the cost-effectiveness ratio: the additional cost that was spent on a particular treatment divided by the additional effect of the treatment. This is shown in the following equation, also known as the incremental cost-effectiveness ratio:

$$ICER = \frac{(C_1 - C_0)}{(E_1 - E_0)}$$

where C_1 and E_1 are the cost and effect in the intervention group and C_0 and E_0 are the cost and effect in the comparison group.

Figure 12 shows the cost-effectiveness ratio for each treatment arm, compared to control, in terms of additional cost per additional outcome. The findings clearly show that low slope incentives are more cost-effective than high slope incentives. High slope costs about twice as much as low slope per additional penta-3 shot, and about one-third more per additional measles shot and additional fully immunised child, compared to control. The ratio for gossip seeds is substantially lower for all significant outcomes due to its low implementation cost.

Figure 12: Cost effectiveness ratio



Note: For the cost-effectiveness ratio, we used our impact estimates to approximate the incremental change in outcomes compared to the control group for each treatment arm.

While incentives are a direct fiscal cost, people also value them. Currently, the assumption is that people value the recharge as much as the equivalent amount of cash. This would be true if the recharge amount is inframarginal. We are fairly confident that all of the recharge amounts are much lower than what households in our population would have typically consumed on their mobile phones, in the absence of incentives.

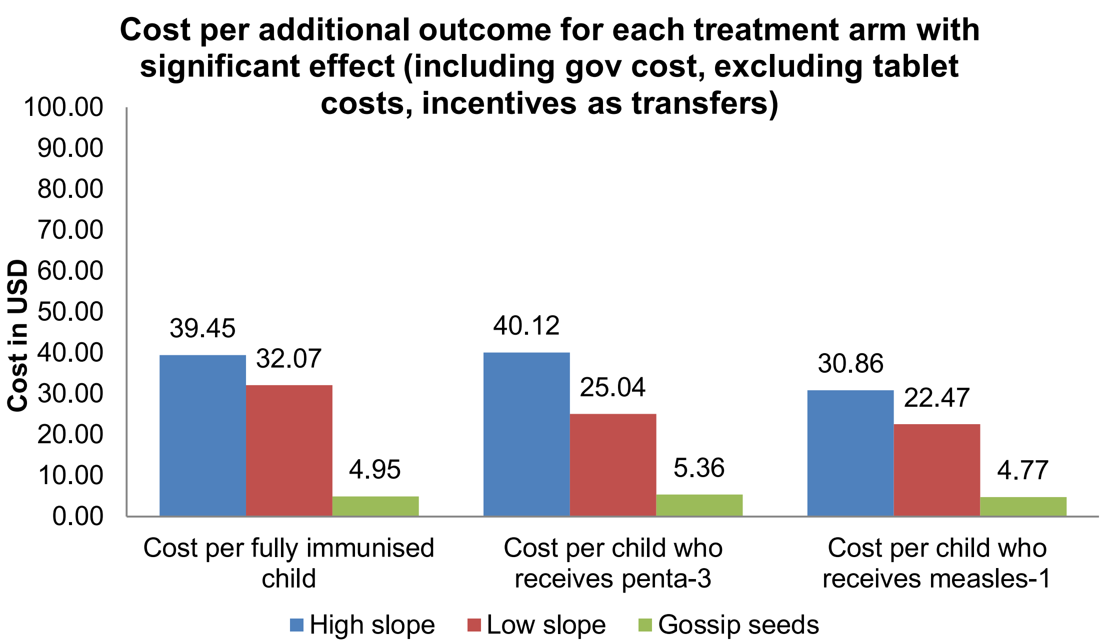
The population of rural India spends approximately 25 per cent of their monthly consumption expenditure on mobile phones, especially for access to the Internet.[20] Therefore, even higher recharge amounts in our programme (INR100, 160 and 200) are likely to be inframarginal, and to have a pure 'income' effect, as cash would have had. This is further supported by the qualitative findings, wherein primary caregivers stated that they valued the recharges, given their own and their families' mobile phone use, but that it was not a very high amount based on their usage.

Since people value incentives in the form of recharges, from the perspective of policymakers (who will consider investment from a larger societal perspective) it would therefore be more appropriate to treat the recharges as a transfer, not a cost. If it is treated as a transfer, then the recharges should be valued at marginal cost of public funds, which is typically 20 per cent.

Figure 13 shows the cost-effectiveness ratio for high slope and low slope incentives, assuming incentive costs as a transfer. This figure shows that the cost-effectiveness ratios have become smaller for both high and low slopes (therefore more cost-effective), although the difference between high slope and low slope has reduced substantially, especially for the measles shot and full immunisation.

This is because the proportion of the incentives costs compared to the total was much greater in the high slope incentives arm than in the low slope incentives arm, and an 80 per cent discount rate therefore results in a greater absolute reduction. Nevertheless, it is evident that gossip seeds remain the most cost-effective intervention.

Figure 13: Cost-effectiveness ratio – incentives as a transfer



Note: For the cost-effectiveness ratio we used our impact estimates to approximate the incremental change in outcomes compared to the control group for each treatment arm.

[20] See: https://www.indiatoday.in/mail-today/story/rural-india-technonology-mobile-phones-national-survey-328188-2016-07-09.

7.4.5 Estimating cost of existing government infrastructure

The CEA results presented in the previous section tried to estimate some of the government costs associated with delivering the programme; however, it did not include all the fixed costs, and did not take into account actual expenditure (which will likely vary).[21] Furthermore, it did not consider the fixed costs of the existing health infrastructure that the programme utilised (e.g. vaccine storage, *anganwadi* costs, ANM salaries), which are part of routine immunisation in India.

In order obtain a very rough sense of how much our programme costs in comparison to the government's existing expenditure on immunisation, we took a top-down approach to estimate the following key indicators:

- The cost of the existing immunisation infrastructure in the 140 PHCs across the seven programme districts; and
- The ratio of the programme costs to the estimated cost of the existing health infrastructure in the 140 PHCs across the seven programme districts.

Given that we were unable to obtain data on actual expenditure, the only data source we found that approximates the amount spent by the Haryana government on immunisation is the budget for 2017–2018. Fortunately, the budget overlaps with the timing of our programme's implementation, and therefore the estimated timelines match. This budget approved a sum of INR23.10 crores or approximately USD3.36 million to be spent on immunisation in the whole state of Haryana over the course of 12 months, excluding the costs of vaccines.

Using population estimates to crudely distribute this total cost across districts and PHCs, roughly INR5.78 crores or USD889,000 was allocated to the areas where our programme was implemented. Note that this does not take into account leakage in government public spending, which means that actual expenditure on immunisation is likely to have been lower.

The total cost of our programme is not too different from the total cost that the government was estimated to have spent on immunisation in the past year in our programme areas, at around 90 per cent. However, incentives alone account for roughly 30 per cent of our direct programme's expenditure – approximately USD243,000. In addition, given our extensive programme development and monitoring activities, our personnel costs accounted for roughly 38 per cent of total costs – approximately USD306,000.

The latter are costs that the government does not incur, and which would not be incurred if they scaled up the programme. The burden of their expenditure is on the Pulse Polio Programme operation costs (approximately 41%), mobilisation of children through ASHAs (approximately 35%) and alternative vaccine delivery (approximately 11%). (See Appendix J3 for the 2017–2018 approved budget for Haryana.)

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[21] Despite trying to obtain cost information directly from the government for the past 10 months, we received very limited data. We were only able to obtain some information related to salaries from certain districts, but there are simply too many missing costs for us to be able to include these. We are currently still trying to obtain data in a pre-specified format from district officials. If successful, we will include these in the final report.

8. Discussion

8.1 Summary of findings

Providing sloped incentives had a significant effect on all observed immunisation outcomes. This confirms our hypothesis that small, non-cash incentives would be effective when delivered in a slope schedule, where receiving a higher amount for later vaccines (irrespective of absolute monetary value) acts ‘nudge’ for primary caregivers to take their child to immunisation camps when salience is likely decreased and opportunity cost is likely to be higher.

We find an effect of sloped incentives at the extensive margin as well as the intensive margin, which we had hypothesised. We were not able to detect an impact of flat incentives on any of the immunisation outcomes. An explanation consistent with both results – the fact that sloped incentives work, but flat incentives did not – is that, in parents' perceptions, the salience or marginal benefit of later immunisations is lower. They do not realise that these are still needed, particularly for measles, which is a different disease altogether. This problem can be alleviated through a higher and more salient incentive for the final vaccinations.

Although the magnitude of the effect of incentives is smaller than expected based on the study conducted by the authors in Rajasthan (Banerjee et al. 2011), the baseline immunisation rates were much lower in Haryana, and the implementation of the interventions was on a much larger scale. Indeed, these results demonstrate that incentives work across a large aggregated population and in contexts with higher baseline immunisation rates, thereby lending strength to the generalisability of incentives for immunisation.

Our results confirm our hypotheses and the authors’ previous findings that when people identified as gossips disseminate information about immunization in a village, there is a significant effect on all immunisation outcomes. Indeed, as we hypothesised, gossips are particularly effective when compared to villages wherein random people were nominated to disseminate information about immunisation. This suggests that if social networks are to be leveraged, they should be nominated by the community, not selected randomly by an external entity (or the government).

It is important to note that the findings show that gossips only work when there are no incentives; in villages where there were both slope incentives and gossips, the effect of the gossip completely disappeared. This seems to indicate that these are not complementary strategies, but rather substitutes for each other, probably because they influence the same people: those for whom a nudge will work. We expect that both incentives and gossip influence the same people who have no resistant attitudes or beliefs toward immunisation, but rather they forget or perceive it as an opportunity cost.

Finally, our results for the targeted reminders are not very compelling. While we did not expect reminders to have a large impact, the current results are difficult to interpret. We find no effect on individuals overall, but do seem to find an effect for individuals in the 33 per cent villages. As such, we cannot confidently say that targeted reminders increase uptake of the measles vaccine. The findings may be a product of the study design – they were only sent to those who turned up to camps at least once – as well as the fact that we only have eight months' worth of tablet data to analyse.

The results from the endline survey show that while there are places besides the camps where children are being vaccinated, we do not observe a substitution effect of our programme and therefore conclude that our impact is genuine. As we found in the baseline, immunisation status is predicted by education level, religion and poverty level. This is supported qualitatively by findings from the caregiver interviews, which also suggest that migrant status is important.

We find limited treatment effects on knowledge and attitude outcomes. There is some indication that primary caregivers in the incentive treatment group are more accurate in their assessment of the number of recommended vaccines, which would support the theory that incentives lead to people being able to better track and remember the immunisation schedule.

There is also some indication that people in a nominated seed village perceive vaccines are more beneficial, which would support our theory of change that diffusion of information about immunisation and immunisation camps via social networks leads to improved knowledge on the benefits of vaccines. However, both these findings, as well as the results from the heterogeneity analysis, need to be investigated further.

The results from the CEA clearly show that due to their very low implementation costs, gossip seeds are extremely cost-effective at USD4.95 per additional fully immunised child. When considering incentives alone, low slope incentives are more cost-effective than high slope incentives. Whether low slope is considered cost-effective itself needs to be benchmarked and will depend on the threshold for willingness to pay for incentives.

In 2011, the EPIC study found that the cost per fully immunised child was about USD60 in Ghana, USD132 in Honduras and USD332 in Moldova.[22] This suggests that a cost-effectiveness ratio of USD61.20 per additional fully immunised child for low slope might be interesting to consider, even more so if we consider incentives as a transfer, resulting in a ratio of USD32.07.

8.2 Generalisability of findings

The findings from this study (having tested interventions at scale) lend themselves to a high degree of generalisability in Haryana state and India more broadly. The impact observed especially for incentives is predictive of what we would expect if the Haryana government scaled-up the intervention across the state, given that it was very closely integrated into the government's existing system of delivering routine immunisation.

Indeed, the programme lends itself to scalability and replication in other states, or even abroad. Most states in India have a similar routine immunisation programme, coordinated by the National Health Mission, with village-based immunisation camps led by ANMs and supported by ASHAs who mobilise primary caregivers in the community. From an implementation perspective, the only major change would be the introduction of tablet-

[22] https://www.immunizationfinancing.org/en/immunization-fundamentals/components-of-immunization-costs#!.

based data collection and setting up a server on the back end to deliver mobile recharges and text messages.

Our monitoring data show that, when implemented correctly, tabled-based recording of immunisation can be very successful, and our qualitative data shows that ANMs find it acceptable and feasible. Furthermore, as per the instruction of the Indian government, most states should be rolling out tablets as well as the ANMOL app for maternal and child healthcare. Thus, the implementation of the model the programme tested is already underway.

Nevertheless, if this programme was to be replicated in other contexts with the aim to observe similar impacts, a number of key assumptions would have to be met:

- A baseline immunisation rate where there is a steep decline or drop-off, particularly in the last five vaccines in the EPI package (pent-3 and measles-1), which is true in many contexts;
- Limited or no attitudinal resistance to immunisation;
- A high rate of mobile phone ownership at the household level;
- No shortages in the supply side of vaccines, and an existing supply system that is able to provide for an increase in demand, as a result of the impact of the programme; and
- High-quality data entry by programme implementers (i.e. the nurses), with an existing government reporting system that can reiterate messages regarding performance.

Finally, we have not yet found noteworthy results from our heterogeneity analysis to present a clear conclusion that any of the treatments have a differential impact on certain sub-groups of the population. This will be investigated further, but for the time being this suggests that both slope incentives and gossip seeds are potentially effective for the general population.

8.3 Contribution to the literature

The public health literature and systematic reviews by and large support the fact that demand-side interventions can lead to significant child vaccination coverage in low- and middle-income countries (e.g. Johri et al. 2015). However, there are still mixed conclusions as to whether incentives and/or text message reminders specifically have an impact on immunisation outcomes. While Johri and colleagues (2015) conclude that incentives are an effective strategy to increase vaccination coverage, a review published in *BMC Public Health* in 2013 found no effect of financial incentives on immunisation coverage (Bassani et al. 2013).

Similarly, a 2016 Cochrane review found that information, education and outreach efforts work, but that evidence for the impact of monetary incentives is limited (Oyo-Ita et al. 2016). A similarly mixed picture emerges for the impact of text message reminders on immunisation outcomes, and leads to the conclusion that there is limited high-quality evidence especially from low- and middle-income countries (Oliver-Williams et al. 2017) and a call for better-powered studies (Kazi et al. 2018).

Our results support the findings from smaller pilot studies that incentives – including quasi-cash incentives – can be effective in improving immunisation outcomes. Being a randomised evaluation at scale, our findings do not suffer from concerns of identification and power, and therefore constitute an important contribution to the evidence base in support of incentives for immunisation. Our results unfortunately do not add clarity to the existing evidence base around reminders to improve immunisation outcomes.

While this study is, to our knowledge, the first evaluation of reminders at this scale, and also the first evaluation of the spillover effects of reminders, our findings are inconclusive. We therefore urge future research projects to adapt our study design to a large scale in a similar context in order to test whether targeted reminders are effective.

The existing literature on diffusion of health information through seeds in social networks is growing. Most of the work to date, conducted by the authors, has been small-scale, proof-of-concept randomised controlled trials to better understand the network centrality of gossip seeds. As such, the evidence to date has had little policy relevance.

The findings from this study, however, validate the finding that gossips work, even when implemented at scale and in a policy-relevant context. In particular, this study proves that there is a non-costly way to identify the right gossip seeds. Existing literature on leveraging people who are central within networks uses measures that require extensive network information. However, in this study we simply asked villagers directly who would be the best at spreading certain types of information, and it was effective (see Banerjee et al. 2019) for a more detailed discussion on the implications of the findings from the communication experiment).

8.4 Limitations

There are a number of limitations of this study that are important to highlight when interpreting the findings. First, we would have ideally evaluated a treatment arm wherein 100 per cent of eligible children in a village receive targeted reminders. This would have offered us an interesting comparator group to see whether the very small individual-level impact of reminders observed in the 33 per cent treatment group would have persisted when everyone who is eligible receives a reminder.

Second, being a large-scale trial, this study did not aim to contribute to a deeper theoretical understanding of the theory of change. Yet our monitoring data and impact analysis raise some interesting questions. For example, we know that fathers are the primary recipients of the incentives and reminders – how does this translate into an observed impact of primary caregivers taking their children to immunisation camps? Our qualitative data has not been able to answer this question fully; while we find evidence that fathers relay the message about incentives, we also find evidence that they don't.

Some primary caregivers stated that people in their household pushed them to go to the camp, but it is unclear whether that was a result of any interventions (which the primary caregivers were unlikely to know themselves). Furthermore, we collected very little data on the pathway to impact for gossips in this context. While we know who the gossips are, and that they have a high degree of centrality in the community, it remains to be understood how and why they diffuse the information they receive, and how it travels along their networks to reach the primary caregivers now attending immunisation camps.

Our small qualitative data collection activity with seeds was not able to shed more clarity on this. Most seeds didn't remember what messages they received, and those who did could not articulate if, when, where, how and to whom they spread this information. While this supports the theory that information does not spread in a very conscious and targeted manner, it is unable to get to this nuance. (Note that the seed interviews were conducted two months after implementation was complete, and recall error may have played a significant factor.) We could encourage future studies to investigate some of these questions to better understand how the interventions lead to impact.

Third, there are a number of caveats with respect to the CEA presented in this report. The cost collation does not include all costs incurred by the government implementing its routine immunisation programme. As described previously, this makes it very difficult to estimate the true average cost of any of the interventions and benchmark the cost-effectiveness results.

In addition, the cost-effectiveness did not (yet) conduct any further sensitivity analyses, such as variations based on discount and inflation rates. Given that implementation of the programme took place only for a duration of 14 months, this is unlikely to have a very significant impact on the cost-effectiveness ratio estimates. However, in reality, if and when the programme is implemented at scale, it will be over a longer period of time, where inflation and discounting rates will potentially have a significant impact on the cost-effectiveness of the interventions.

8.5 Challenges and lessons

8.5.1 Challenges

Over the course of the study, we faced a number of key challenges, both on the implementation side (for which the study team was also responsible) and the evaluation side.

Implementation

Continuity of government buy-in: This study has faced various degrees of support from our primary government stakeholder since its inception, and this is the main reason why the timeline has been extended multiple times over the past seven years (when discussions with the Haryana government first began). We faced issues in continuity and buy-in especially when higher-ranking officers were transferred. Fortunately, since 2017 when we started training and subsequently implementing the interventions, we enjoyed the full support of the mission director.

Failure of text message delivery: From the beginning of the implementation period it has been difficult to accurately monitor the text message delivery status, because the vendor's database and our own database could not directly connect to each other. This contributed to a failure in identifying that due to a technical glitch, text messages were not delivered from November 2018 onward. By the time we discovered this, it was too late to rectify the situation.

Periodic problems in recharge delivery: We faced a number of issues over the implementation period with regard to recharge delivery. In the summer of 2017, we observed a much lower success rate, which was due to the implementation of the goods and services tax by the Indian government. In November 2017 we discovered the recharge success rate was dropping again, caused by the fact that certain denominations were no longer being accepted by mobile service providers, and we needed to bump up the amount to the nearest accepted denominations. This improved the recharge success rate from January 2018.

Issues with ANM performance: We continued to identify ANMs who were not meeting our performance metrics over the course of the programme. This includes ANMs entering their own phone numbers into the tablet and ANMs in the control group entering more invalid phone numbers than those in the treatment arms. Given that our study integrated into the existing government system, we were limited in how much we could push for improved performance, beyond the efforts that were already made (Section 6).

Evaluation

Managing multiple complex data collection activities and datasets: This study has seen a multitude of large and complex data collection activities, which required a high degree of coordination to complete. A key challenge was prioritising datasets and, in a limited timeframe, identify which variables across datasets are important to connect. While we had a team member dedicated to the data tasks, the large volume of data rendered this insufficient.

Administrative changes in villages: Over the course of the study, some villages underwent administrative changes – either name changes or changes in which SC or PHC they now belonged to. This not only posed a challenge for implementation, but also maintaining and safeguarding the village sample of the study and connecting them across waves of data collection.

Collecting costs from the government for the CEA*:* It was very difficult for the programme team to collect cost data from the government. Despite approaching the task from multiple strategies, including in-person visits to DIOs with a pre-designed cost collection form, very little data were actually provided to us. This made it difficult to estimate costs to include in the CEA and is a caveat to the interpretation of results from this analysis.

8.5.2 Lessons

Based on the challenges faced, there are several lessons learned which we would recommend for any future researchers and implementers who want to work in a similar context.

Stakeholder buy-in is key

Despite the delay at the start, we were able to conduct and complete the study successfully. This was made possible because of the buy-in from our key stakeholders; specifically: (1) the ANMs; (2) government officials (at all levels); and (3) our own field staff. We invested heavily in explaining the purpose and benefit of the programme to these stakeholders, and to solicit feedback from them in terms of feasibility and acceptability of the programme and the activities in which they were participating. Especially with the government, we ensured we met key officials regularly and made clear that this was a partnership. It also helped to have a champion inside the National Health Mission who was able to lobby senior officials and receive permission when required.

Leverage administrative data
This study was able to leverage administrative data for both real-time monitoring, as well as measuring outcomes. This is extremely valuable: implementation can be tracked closely and course-corrected almost immediately, and evaluation can be conducted with a much larger sample size without having to carry out expensive surveys. However, the condition is that administrative data are of high quality, which is not always the case.

There are two key reasons for the high quality of the administrative data collected in this study: first, the m-Health app was designed to be simple and focus only on immunisation, which we think made it easier for ANMs to use (and enter accurate data); and second (and most importantly), there was a very high degree of monitoring and scrutiny on the back end for which the study team was responsible. While we would not typically expect that administrative data collected routinely by the government, even digitally, would have the same high quality, we think it is possible to build this capacity.

Get the right implementation support
Given that J-PAL South Asia was also responsible for implementation, we recruited a dedicated team with relevant experience to do this. As a result, we were acutely aware of what was happening in the field, and able to identify and resolve issues quickly. This helped us to contextualise much of our monitoring activities, as well as flag any risk and mitigation activities a priori. While this may not be a common model, we would urge other evaluators to also think critically about whether they have, or are receiving, the right implementation support.

9. Policy implications and recommendations

9.1 Implications for policymakers

Since the programme was designed specifically to inform policy on a large scale, the findings and lessons from this study are very relevant for policymakers. They lead to a number of recommendations as to what policies to implement in order to improve immunisation outcomes.

Our programme demonstrates that tablet-based interventions can be acceptable and feasible to implement with front-line health workers, provided they are adequately supported in a context where m-Health interventions are new. This is particularly pertinent given that the Indian government is rolling out the ANMOL app; our findings are able to recommend what the minimum implementation support is in order to successfully do this (Section 9.2).

The findings from our evaluation show that incentives for immunisation, in the form of mobile recharges and administered in a slope schedule, improved all immunisation outcomes across different contexts and population groups. They are also feasible to administer – the distribution of mobile recharges happens automatically, as long as server functionality and monitoring systems are set in place.

Of course, incentives cost money, and the cost-effectiveness ratios calculated need to be benchmarked to establish whether they are indeed cost-effective and worth investing in, from the policymaker’s perspective. It is important to note that there are some costs that state governments will not need to incur if they decide to scale up incentives, such as the cost of the tablets and developing the m-Health app (with the advent of ANMOL).

This, along with the consideration that incentives themselves have value, means that low slope incentives specifically will incur minimum additional cost, and therefore may be an extremely attractive option. We would recommend that decision makers conduct a budgetary impact analysis to establish whether they are willing to scale-up low slope incentives.

Our findings also show that gossip seeds can improve immunisation outcomes at a lower cost than incentives. As such, gossip seeds are an extremely cost-effective option, much more so than incentives in the form of mobile recharges, and we would recommend that policymakers seriously consider adopting this strategy to try and improve immunisation outcomes.

Often, policymakers already use key informants to diffuse (new) information to a community, so there is at least willingness as well as a degree of cost involved in identifying this person. What our evaluation suggests is that policymakers should invest a little more time and money to identify the ‘best’ person to disseminate information, by relying on nominations from the community.

While this will require upfront investment in manpower (i.e. going into villages to find nominations), the return on investment is high. Indeed, gossips do not need to be used for immunisation alone – they can also be leveraged to spread information about other recommended behaviours and services the government wants to provide.

Based on our findings, we are unable to make a policy-relevant recommendation about targeted reminders. We first need to better understand and replicate our findings before we can determine if it would be cost-effective to implement targeted reminders at scale.

Finally, it is worth noting that the interventions we tested were targeted toward achieving behaviour change at scale; that is, finding a solution that works for most people in terms of improving immunisation outcomes. As the qualitative findings in particular show, incentives, for example, may not work for everyone: there are people who hold strong attitudinal resistance, face barriers in accessing immunisation services, or experience broader socio-demographic barriers. We would therefore recommend that policymakers consider other, targeted interventions for these specific groups, alongside a more broad-based solution for the majority of the population, such as incentives and gossip seeds.

9.2 Recommendations for implementation

If policymakers want to scale-up either the low slope incentives or gossip seed interventions based on the findings from this evaluation, there are a number of recommendations we would like to make around implementation.

First, as described in Section 8.2, policymakers should ensure that the interventions are both relevant and generalisable for their context – specifically, that there are no major supply-side issues and there is high coverage of households owning mobile phones.

Second, if the interventions are found to be relevant, there are a number of crucial implementation activities that should be adopted in order to ensure that the interventions are delivered successfully:

- Extensive training of front-line health workers, using the master trainer model;
- Continuous and on-going implementation support – either in the form of a separate hotline with an implementation support team, or dedicated nodal people for the programme at the district level to troubleshoot issues;
- Expertise to manage the recharge and text message delivery system through the server – specifically, ongoing support to troubleshoot any technological issues;
- Close monitoring (both remotely and in-field) of front-line health worker performance – specifically, monthly review of key indicators and reinforcing good performance norms when issues are found by supervisors (a risk is that without the extensive monitoring activities we undertook, the quality of data will decline, and for incentives there could be an emergence of fraud); and
- Regular meetings about the programme amongst district officials and state-level officials, so that issues can be acted upon quickly.

We would further recommend that an entity like J-PAL or Evidence Action offers support to policymakers to set up the intervention delivery and monitoring systems, and transfers the management of the interventions to the government after capacity building. This is to ensure that the activities as described above are actually put in place.

Appendix A: Field notes and other information from formative work

Sub-centre (SC)

Overall, every SC we visited had a schedule of immunisation days in all villages under the centre. There were records maintained by the ANMs as well as basic equipment. The SC location is usually central within a particular village (although in one case we saw that the location of the centre itself caused a lower number of mothers to come for immunisation, as a similar camp in another location usually receives a greater number).

People
ANM: In all the SCs we visited, there was at least one ANM; however, we heard of cases where there are no ANMs. There are also far fewer SCs than there should be (one for every 5,000 people). Therefore, the state appears to be pushing to create more SCs to meet the demand and subsequently there is a need for more ANMs. Another interesting thing about the ANMs is that most of them are either not from Haryana at all or at least not from the village. This means they usually live at the centre and hence are accessible most of the time.

Male and female worker: All SCs need at least one male and one female worker to assist the ANM. This of course is very rare. Out of the five SCs we visited, only one had both male and female workers.

Records
Eligible couple tracking record: This record is to track pregnancies and subsequent antenatal and prenatal care as well as immunisation.

Child immunisation tracking record: This record tracks every child's immunisation progress. We could use this record to obtain information on children vaccinated in a particular day.

Copy of the child's immunisation treatment card: This is maintained at the centre to track the child, though we didn't see it systematically kept in more than one SC.

Primary Health Centres *(PHCs)*

People
MOs: There should be at least one MO at every PHC. We met a couple of MOs and talked about their daily workload.

Lady health visitor: All PHCs have lady health visitors who are responsible for maintaining the PHC and monitoring SCs.

Health inspector: The health inspector is responsible for maintaining all reports and registers at the PHCs, and supervises the SC reports as well.

Others: Dental surgeon, staff nurse, lab technician, pharmacists

Records
PHCs have volumes of records submitted by SCs and collected at the PHC. Below are some relevant reports.

Monitoring formats: The MO and chief medical officer monitoring formats are filled out during their SC visits. Only hard copies of these reports are maintained.

ANM aggregated report: Every month the ANMs provide aggregate numbers on immunisation (e.g. total number of children, total number of children who received BCG vaccination). This is maintained in a register and some data are also entered electronically.

ANM monitoring: Currently the chief medical officer and MOs from the PHCs are assigned to conduct regular monitoring of the SCs. They have to fill in a very detailed questionnaire primarily focusing on the centres' infrastructure and physical assets. There are also details on the number of patients and immunisation. We managed to get a copy of the questionnaire at a PHC.

We also met an MO at an SC during his routine monitoring visit (though we didn't see him filling in the questionnaire). His main complaints concerned his workload and the expectation that he also maintains the quality of the SC while running the PHC.

A lady health visitor at each PHC is also supposed to visit the SCs regularly to monitor the ANMs' work. Her role is less organised and she does not have any performa to fill. She tends to oversee all the SC operations. This level of monitoring of course depends on her availability at the PHC.

We also spoke to a lady health visitor, and although we were not certain about her exact role, she mentioned her job is mostly limited to taking care of the PHC and making field visits.

A Health Inspector also makes visits but only looks at the technical and infrastructural aspects of the SC.

The current inspection is meant to consist of random and unannounced visits, but in practice it is rarely so. ANMs admit to knowing the approximate date of inspection. The paperwork to be filled out for one inspection is huge and doesn't seem to have been followed strictly in practice. It is possible that they complete it at home before they submit it to the PHC.

Also, ANMs seem completely optimistic when responding to inspectors. One could tell that they were explicitly dishonest with the inspector or us (thinking that we were monitors).

Frequency of immunisation sessions

From our observations, we understood that there are two broad models adapted by the ANMs in the districts we visited. Of the SCs we visited and observed, each covered 3–15 villages or hamlets. Covering more than four villages or hamlets is a challenge for the ANMs, as only one day per week is designated for immunisation. Every Wednesday is 'immunisation day' in Haryana, when every SC provides vaccinations to the children in the coverage area. If an SC oversees more than four villages or hamlets (which seems to be the usual case), the two strategies below are adopted to cover all the designated villages.

Covering multiple villages per Wednesday: The ANM divides her time between two or more villages (in one case the schedule had five in one day) every Wednesday. This way she can cover more than four villages per month and achieve her target. This of course is a less effective method, as she gives less time per village. It might even be unrealistic for her to travel so much every day. Whether ANMs actually conduct camps in more than one village is a different matter. Though they respond that they do have to finish, as there is no choice, it is hard to believe. According to the ANMs, they also cover the leftover mothers on Thursday if need be.

Conducting two 'immunisation days' per week: The ANMs cover two villages per week by going to one village on Wednesday and one on Thursday, thereby covering more than four villages per month and achieving their target. This seems to be the more efficient way of covering all villages, but this does affect ANMs' many other responsibilities, and allotting more than one day per week to immunisation is not always feasible.

ANM activities

ANMs have a lot of paperwork as well as various responsibilities apart from their basic role as healthcare providers. ANM workers provide vaccinations to children, prenatal and antenatal care and deliveries. They are also directly observed treatment providers and provide other medical assistance.

Other than their health service responsibilities, they maintain records for each of these activities separately, which is an onerous activity. Every Thursday is also allotted as 'village health nutrition day', when they visit villages and disseminate information on, for example, general health, malaria and tuberculosis. They are also responsible for going to the PHC and collecting vaccines for the immunisation day every week (the districts we visited do not yet have in place an alternate vaccine delivery mechanism), as well as reporting to the PHC one designated day of the week. Other than this, monthly reporting at the PHC also takes place to aggregate all the weekly figures for immunisation and other activities.

Incentives

We also had informal conversations with young mothers, couples, health workers, ANMs and MOs on incentives and preferences. Some of the (non-food) items purchased in households are cradles, toys, baby oil and clothes.

Baby oil: This is the one item that all mothers seem to use to massage their babies. Though some use domestic mustard oil for massage, the use of bottled oil is increasing and could be attractive to parents. Providing this as an incentive looks feasible since each bottle of oil is in the price range of INR40 (market retail price). Each bottle is small and wouldn't typically last for more than a month. This could be relatively easy to store and if we procure plastic bottles, wastage could be reduced.

Baby clothes: Clothes were another item that mothers often bought. Children under two years typically wear unisex clothes – t-shirts and shorts. However, parents typically do not buy clothes every month; therefore, providing clothes as a regular incentive might not work as well. Also, child sizes vary considerably for small children and might present issues in sorting. One way would be to provide a set of clothes upon full completion. Children should be of comparable age at this stage (say 10–15 months old) and size would not be a big issue. Also, a set of clothes (t-shirt and shorts) could be procured for the price range that we are targeting (around INR100).

Toys: This suggestion came up at one of the SCs. It seems that toys were used as direct incentives to attract children during the Pulse Polio campaign. They used plastic balls and whistles as incentives. People seem to think that these incentives were effective in attracting the crowd for immunisation. However, it might not be as attractive to parents when we give them out every month (with each shot). One solution would be to rotate the type of toys we hand out every month.

The downside of this incentive is that parents might not value giving children toys as much, and this might be an issue when we are handing them out every month. The upside of this incentive would be that this could be considerably cheaper than the alternatives (depending upon what we purchase). We could possibly rely on this if funding becomes an issue.

Immunisation practices

We also talked to mothers about their children's immunisation. These mothers were already coming to the sessions to immunise their children, so didn't represent typical mothers; however even their knowledge about immunisation and service was quite low.

Most mothers didn't know what the shots do, except that they were 'good'. Very few could name a disease that these vaccinations prevent. Mothers rarely even knew which shot their child would be getting on that particular day. Most mothers did have an immunisation card (which is almost entirely a selection issue), but some cards were very poorly kept. Mothers could barely remember the number of shots their child had without looking at the cards.

Some mothers didn't know of the scheduled day of the immunisation sessions in their villages. Few were told by children or mothers in their neighbourhood who went to the *baalwadis* (preschools).

Conclusion

Baby oil may be used for regular incentives and clothes are viewed as the ultimate prize. Toys are still under consideration particularly because of budget issues.

There is a need to conduct an informational campaign explaining the incentives and immunisation sessions. Mobilising ASHAs is the current way of disseminating information, but that might not be as effective. Informational campaigns could potentially be conducted in schools and *baalwadis* to explain immunisation sessions and incentives (where applicable). Pamphlets could also be used.

The current monitoring is not being carried out well, and current checks are rarely random. Separate and additional monitoring should be conducted with truly random visits.

One issue to be aware of in the monitoring is the question of which village should be visited if the scheduled session is supposed to be held in three villages on a particular day. Should the monitor visit all villages and verify that the session is taking place in at least one of them?

ANMs currently keep records of all the children. There is a need to verify the rosters with actual children to ensure ANMs aren't inflating and falsely reporting the numbers. ANMs currently have more than one record book wherein they record information about who was immunised in a particular day. We must be aware of this during monitoring and ensure that the list has no repetitions or omissions.

There is also a need to organise recordkeeping at the PHC level, at least for the treatment PHCs, to keep track of and audit incentive distribution.

Appendix B: Descriptive statistics

Seed characteristics by treatment

Table B1: Seed demographic information by treatment arm

	Random seed	Gossip seed	Trusted seed	Trusted gossip seed
Nomination statistics (per village)				
Number of nominations	-	19.915	20.313	19.993
	-	(8.585)	(8.670)	(11.351)
Nominations for top six individuals	-	11.217	10.560	10.769
	-	(4.576)	(4.265)	(5.575)
Seed characteristics				
Refused to participate	0.186	0.165	0.219	0.175
	(0.389)	(0.372)	(0.414)	(0.380)
Age	49.233	48.569	48.569	48.890
	(14.617)	(14.347)	(14.130)	(14.082)
Female	0.067	0.129	0.070	0.119
	(0.250)	(0.336)	(0.256)	(0.324)
Education (years)	6.980	8.499	8.116	8.753
	(4.280)	(3.966)	(4.073)	(3.930)
Own land	0.586	0.675	0.680	0.687
	(0.493)	(0.469)	(0.467)	(0.464)
Wealth index from assets	0.183	0.218	0.217	0.226
	(0.098)	(0.121)	(0.114)	(0.120)
Hindu	0.866	0.876	0.876	0.892
	(0.341)	(0.330)	(0.330)	(0.311)
Muslim	0.103	0.107	0.103	0.086
	(0.305)	(0.310)	(0.304)	(0.281)
Scheduled caste or tribe	0.231	0.200	0.173	0.200
	(0.422)	(0.400)	(0.378)	(0.400)
Other backwards caste	0.237	0.253	0.246	0.209
	(0.426)	(0.435)	(0.431)	(0.407)
Panchayat member	0.106	0.320	0.259	0.300
	(0.308)	(0.467)	(0.438)	(0.459)
Numberdaar or chaukidaar	0.112	0.353	0.261	0.326
	(0.316)	(0.478)	(0.439)	(0.469)
Interacts with others: very often	0.263	0.455	0.371	0.444
	(0.441)	(0.498)	(0.483)	(0.497)
Participates in community activities: very often	0.264	0.457	0.371	0.445
	(0.441)	(0.499)	(0.483)	(0.497)
Aware of immunisation camps	0.687	0.758	0.689	0.762
	(0.464)	(0.428)	(0.463)	(0.426)
Aware of ANMs	0.432	0.646	0.574	0.622
	(0.496)	(0.479)	(0.495)	(0.485)
Aware of ASHAs	0.605	0.794	0.706	0.780
	(0.489)	(0.404)	(0.456)	(0.415)
Observations	570	648	712	674

Note: The number of observations reported refers to the total number of nominees where a survey was attempted.

Demographic characteristics of our sample in Haryana

Table B2: Baseline demographic information 1

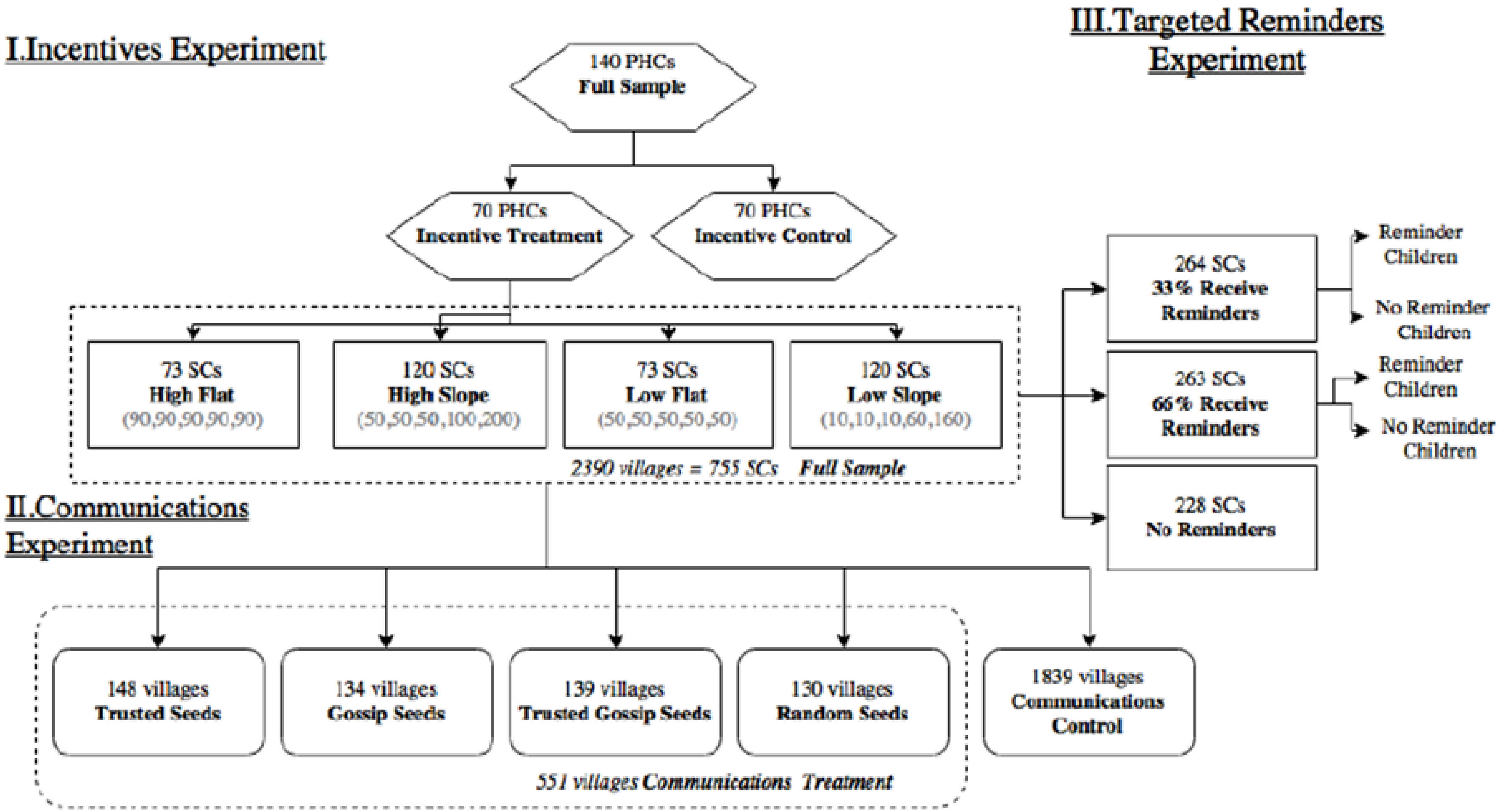
Characteristic	Mean	Std. dev.	Min.	Max.	Number
Male education (years)	7.88	4.46	0	13	35,383
Female education (years)	5.24	4.86	0	13	37,310
Household size	6.87	2.76	2	54	14,669

Note: Std. dev. = standard deviation.

Table B3: Baseline demographic information 2

Characteristic	Percentage	Number
Caste category		11,780
Scheduled caste or tribe	23.66	
Other backward caste	23.08	
Minority	0.63	
General	32.50	
Others	0.43	
Religion		14,340
Hinduism	86.33	
Islam	10.85	
Other	0.57	
Mobile phone ownership		14,655
Yes	97.35	
No	2.55	
Source of income		14,620
Self-employed	33.54	
(agriculture)	12.68	
Self-employed (non-	4.53	
agriculture)	37.05	
Agricultural labour	33.53	
Other labour	7.96	
Pension	1.44	
Regular wage/salary		
Other		

Appendix C: Sample design



Online appendix D: Survey instruments

All the survey instruments used in the project can be found as sub-appendixes:

Online appendix D1: PHC survey instrument

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-D1-PHC-Survey-Instrument.pdf

Online appendix D2: Baseline census_instrument_final

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-D2-Baseline-Census-Instrument-Final.pdf

Online appendix D3: Baseline survey instrument

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-D3-Baseline-Survey-Instrument.pdf

Online appendix D4: Nominations questionnaire_final

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-D4-Nominations-Questionnaire-Final.pdf

Online appendix D5: Seeds survey instrument

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-D5-Seeds-Survey-Instrument.pdf

Online appendix D6: New census instrument

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-D6-New-Census-Instrument.pdf

Online appendix D7: HIM endline instrument

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-D7-HIM-Endline-instrument.pdf

Appendix E: Sample size and power calculations

Original power calculations

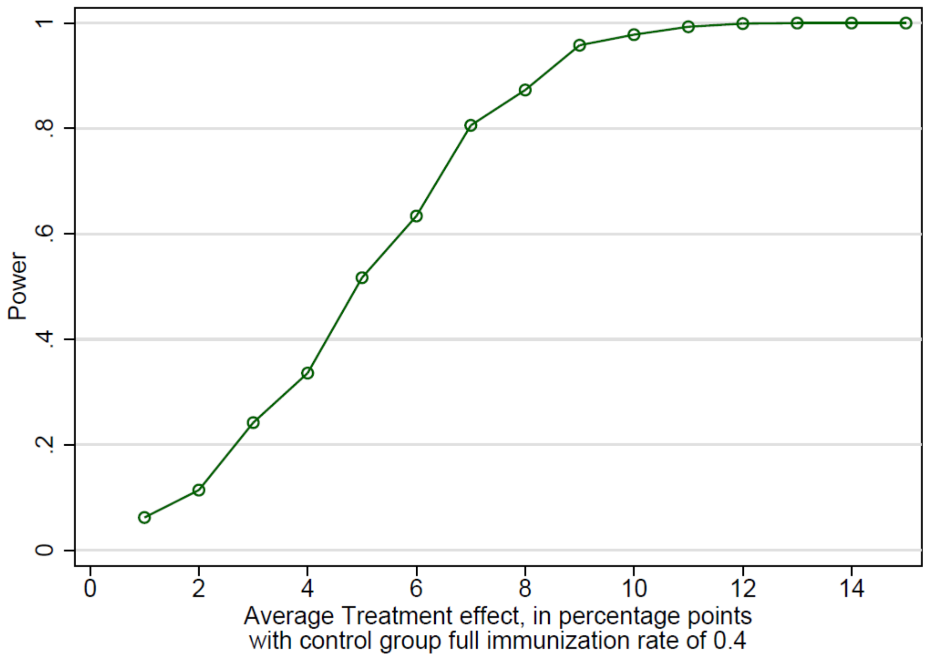
Online appendix E1 provides the power calculations that were conducted at the start of the project.

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-E1-Original-Power-Calculations.pdf

Updated after baseline

Appendix E2 provides the updated power calculations that were conducted after getting a precise estimate of intra-cluster correlation and baseline immunisation rate. If we use a two-level clustering design (villages within PHCs and households within villages), we can detect an effect of 7 percentage points with a power of 80 per cent. See Figure E1.

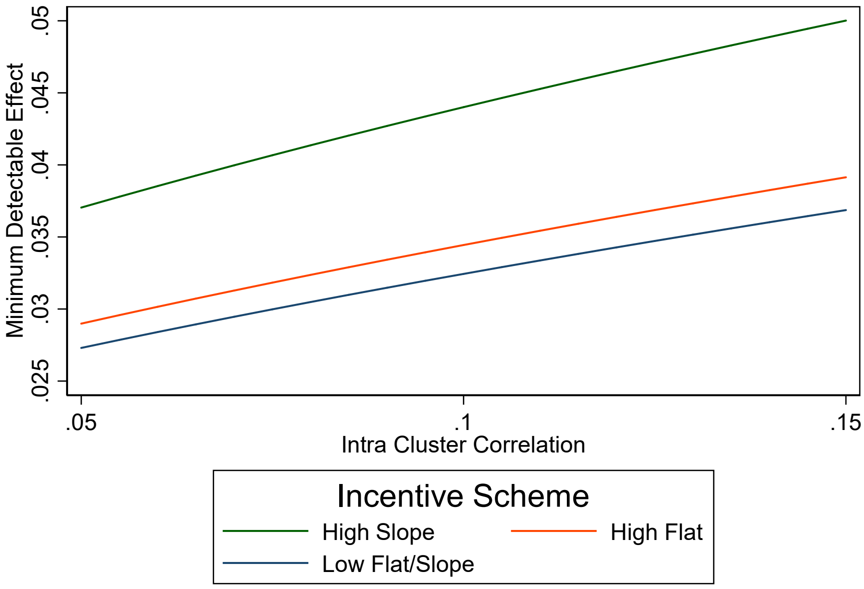
Figure E1: Power curve



With SC level incentive treatment

Note that the following graph is a conservative estimate, with an assumption of one village per SC. On average, we have data from three villages per SC. With an intra-cluster correlation of 0.12 (from the baseline), we can detect (against incentive control) a minimum effect of 0.045 for high slope, 0.033 for low flat and 0.038 for high flat at 80 per cent power.

Figure E2: Mean detectable effect



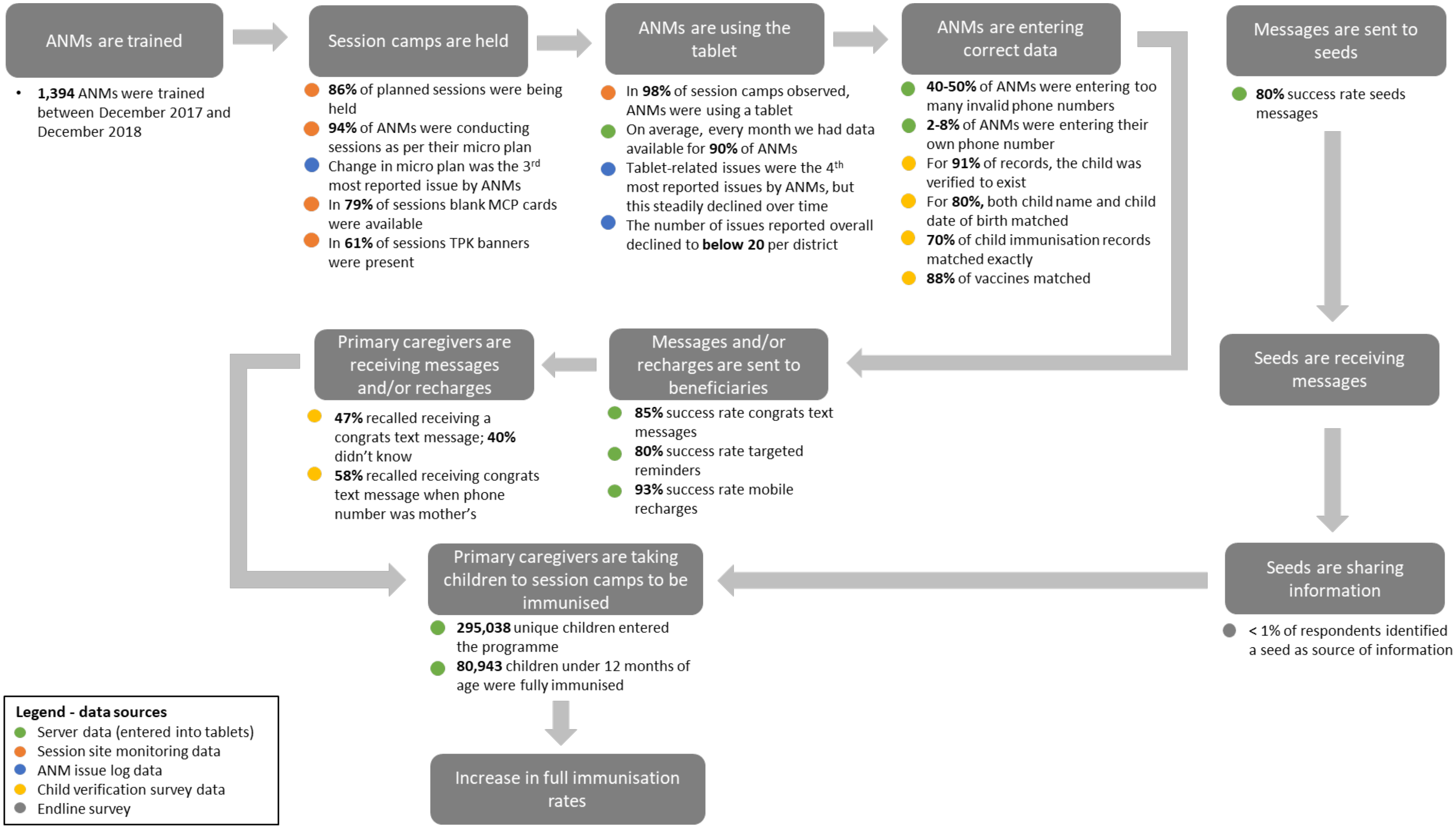
for various level of intra-village correlations and 80% power

Online appendix F: Pre-analysis plan

The updated pre-analysis plan is supplied as Appendix F.

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-F-HIM-PAP-June2018-vSUBMIT.pdf

Appendix G: Process monitoring indicators



Appendix H: Results

Balance test

A randomisation balance check was carried out to check if randomisation was conducted properly, to determine whether imbalance in baseline characteristics causes any chance bias, and whether the analysis requires any adjustment for one or more baseline variables. The exercise checks if any characteristic is significantly different between the incentive treatment and control group.

Table H1 reports the coefficients on a treatment indicator, from regressions of each of the baseline characteristics on the treatment indicator. For a given outcome, the coefficients can thus be interpreted as the difference in the mean of that outcome for the treatment and control groups. None of the 18 variables is statistically significant at the 5 per cent level. To be sure that this is no more than spurious correlation, we also report the p-value on the F-test for joint significance: 0.15. This is further confirmation that observable characteristics are jointly uncorrelated with treatment assignment.

Table H1: Balance test results

	Treatment mean	Control mean	$X_i = \alpha+\beta T_i+e_i$	Obs.
Self-reported financial status (scale of 1–10)	3.268	3.296	-0.029 (0.072)	14,665
Land owned	0.449	0.485	-0.036 (0.019)	14,669
Land area (acres)	6.146	6.942	-0.796 (0.575)	14,405
Wealth index	-0.054	0.052	-0.106 (0.129)	13,790
General	0.412	0.418	-0.006 (0.028)	11,720
Agricultural labour	0.030	0.023	0.007 (0.005)	14,447
Other labour	0.340	0.326	0.014 (0.021)	14,447
Maximum education	10.235	10.513	-0.279 (0.237)	14,668
Number of vaccines administered	3.785	3.833	-0.048 (0.108)	16,709
Full immunisation rate	0.401	0.396	0.005 (0.026)	16,709
Immunisation card present	0.848	0.866	-0.017 (0.023)	16,709
Immunisation card verified	0.496	0.492	0.004 (0.026)	16,709
Mother's age	26.045	26.061	-0.016 (0.139)	16,875
Mother's education	7.441	7.833	-0.392	16,833

	Treatment mean	Control mean	$X_i = \alpha+\beta T_i+e_i$	Obs.
			(0.396)	
Father's age	30.032	30.088	-0.056	16,735
			(0.142)	
Father's education	9.161	9.462	-0.301	16,669
			(0.261)	

Notes: ***p < 0.01, **p < 0.05, * p < 0.1. Obs. = observations. The p-value on the joint F-test is 0.1479. All standard errors are clustered at the PHC level.

Outcome tables

The complete outcome tables for the specifications mentioned in the pre-analysis plan are included as appendixes:

Online appendix H1: Tablet analysis

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-H1-Tablet-Data-Analysis-updated.pdf

Online appendix H2: Summary analysis from endline

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-H2-Summarized-Analysis.pdf

Online appendix H3: Demographic analysis

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-H3-Demographics-Analysis.pdf

Online appendix H4: Heterogeneity analysis

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-H4-Heretogeneity-Analysis.pdf

Appendix I: Key themes from ANM focus groups

Acceptability and feasibility of the tablet

General enthusiastic acceptance of the tablet in Jhajjar, Palwal and Panipat. Most ANMs acknowledged the increase in time taken (longer data entry) and workload (double record maintenance) in using the tablet. However, the time-saving automation of manual and tedious processes like report generation, the easy accessibility of data, the proof of work completed and the expectation of discontinuation of registers seemed to make up for this. Indeed, a recurring theme with most groups was the desire to expand the use of tablets in all other professional work (not just immunisation camps), provided improvements are made to the tablets' existing functional capacity.

Technical and practical limitations of tablets. While not decreasing the overall enthusiasm for the tablets, a number of technical and practical limitations were listed by ANMs:

- An inability to fulfil government-mandated immunisation-related tasks (like generating due lists);
- A lack of area reporting – i.e. an inability to mark vaccines (history, after the first visit) received by a child in an 'outside area' and having to assign multiple barcodes in this way, generating different records on different tablets;
- Navigation restrictions preventing the rectification of mistakenly selected vaccines;
- Tablet scanners' sensitivity to laminated, photocopied or worn Aadhaar cards, which is a core difficulty in many Aadhaar-based applications throughout India. We made sure we had an easy work around, namely manually typing the ID number, which was easy to do and a default for some ANMs; and
- An inability of caregivers to bring documents that were prerequisites for using the tablet.

In addition, some ANMs commented that there was an increase in waiting times at the immunisation camps caused by having to carefully record data in the tablet and the register, which made caregivers impatient. However, the reminder of the tablet entry was cited by some ANMs as a means of placating caregiver impatience in some cases.

Some instances of resistance to tablets in Mewat. For two out of the four ANM groups in Mewat district, there was an overall desire to see the tablets rolled back and resume traditional data entry[23] in registers. Tablets were labelled a 'headache' due to longer processes (made longer still because some primary caregivers did not bring documentation as required).

This was true even for ANMs who did not find it difficult to use tablets, but dismissed their convenience value and worth because they had failed to motivate caregivers and increased their workload. Older ANMs in these two groups were particularly strong in their preference of registers no matter the improvements to the tablet, since they believed tablets to be burdensome and they could not get used to them. Note that this also took place in a context of broad resistance to immunisation in Mewat.

[23] 'It [data entry on tablets] wastes 15 minutes.'
'When immunisation has not increased, then what is the benefit [of the tablet]?'

Tablets make ANMs feel more empowered. While it was not an original part of the programme's theory of change, the tablets seemed to have raised the self-esteem of many ANMs, who were excited to learn something new, to become confident about their potential to harness technology, and to impress both caregivers and their own families.

Acceptability and feasibility of the programme

General acceptance of the programme by both ANMs and primary caregivers. Overall, the ANMs had no issues with the programme itself (i.e. no resistance to the idea of providing incentives to primary caregivers) and considered it acceptable for primary caregivers as well, given that there are only benefits for them (e.g. monetary, health and integration within system, information). One of the control groups only judged the programme's acceptability based on the fact that people were able to adapt to its requirements for documentation, presumably because there was a lack of other benefits.

Most ANMs experienced some challenges in implementation, although this did not generally affect the perception of the programme's feasibility. The programme was generally considered feasible to implement by most ANMs, although there were a couple of key challenges they experienced, mainly revolving around dealing with difficult primary caregivers. Across all districts, ANMs mentioned facing complaints of recharge failures in incentive treatment arms – some primary caregivers would express their dissatisfaction with ANMs[24] and threaten not to return to the immunisation camp, as they viewed it as an empty promise.

While some ANMs worried about this, others dismissed it as part of the job of dealing with primary caregivers more generally. In Jhajjar, ANMs struggled particularly with primary caregivers not bringing their Aadhaar cards (especially among daily wage workers and some in the 'low flat' areas, who told ANMs that the incentive was too low to make the effort of having one made).

In Panipat, ANMs faced challenges with migrants and daily wage workers, who may not have a phone number, or who found the whole process of the programme too time-consuming and would rather have it limited to vaccine administration. In addition, the older primary caregivers do not always phone numbers.

Mixed views about adequate support given to ANMs. Despite being instructed to do so by district officials, ANMs reported receiving limited feedback from their supervisors on their performance during the programme. Where feedback was given, it was not experienced as constructive; rather, it was limited to reprimands of missing monthly targets and accusations of not using the tablet correctly (when ANMs thought they were).

There was general positive feedback for support from the J-PAL implementation team, specifically the quick troubleshooting of problems raised through the helplines. Some ANMs in Mewat felt that more support could have been provided in terms of aiding their efforts to convince resistant caregivers who were more likely to listen to an outside voice. The lack of regular (particularly positive) feedback on performance, was also expressed by many ANMs who desired more routine visits to see how they were coping.

[24] "If the recharge doesn't deliver, beneficiaries say, 'Madam, you are lying...the government is taking us for a ride'…so this can be tiresome."

Perceived impact of the programme

There is a general perception that the incentives have 'pushed' more primary caregivers to attend immunisation camps. Many ANMs expressed the view that they did not think their populations needed the programme to push them to come to the immunisation camps. Nevertheless, ANMs felt that, independent of how primary caregivers value immunisation, the incentives have been successful in reminding and motivating them to attend sessions on time.

Most people were reported to be aware of the importance of immunisation prior to the programme, but ANMs believe that the promise of reward at the end of the process has helped move previously traditionally resistant people, and neutralised the complaints of long waits and fevers from the vaccines. Some ANMs reported a slight increase in footfall because of the legitimising effect of the tablets and their online uploads.[25]

The perceived impact is positive in Jhajjar, Panipat and Palwal. In both Panipat and Palwal, ANMs expressed the view of a definite increase in number of children visiting camps, mainly because of the incentive, but also due to barcodes on immunisation cards, online data uploads and text message reminders.

In Jhajjar, ANMs were less unanimous in this view. A few ANMs reported an increase in the number of children visiting camps (especially among the daily-wage earning scheduled caste population) because of the legitimising effect of the tablets and online uploads. The rest, however, perceived their populations as aware of the importance of immunisation and not requiring the programme's push, though it has had the effect of allowing them to stick to a timely visiting schedule and take it more seriously.[26]

The perceived impact is mixed in Mewat. Only ANMs in the slope group reported a marginal increase in their coverage because of the incentives (and for some, exclusively for the reward[27]). For the rest of the groups, ANMs thought that either they had a majority population that never required the programme's push and a minority population that was immune to any motivating efforts, or a majority population that remains obstinate[28] due to sterilisation rumours, interpretation of holy forbiddance, complacency borne of faith in divine providence,[29] and time constraints of working mothers with many children.

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[25] 'It has all become valuable now, like first the villagers used to come, and we would just start giving the injections, now they feel, "She is doing something…[proper] on the phone [tablet]'… They say, 'Oh, our child's name and injection has gone up [online to the government]!"'
'So their faith has also increased, "These ANMs are doing good work…since she's entering all this information on it – name also, Aadhaar number also"…So some people's faith in us has increased, I feel our value has also increased.' 'The government is also taking immunisation seriously.'
[26] 'More people are beginning to participate and get immunisation done on time for fear and want to [get their child] into a good school, which will enable them to get good jobs.'
[27] 'People come but only for the incentive, [they say], "At least I will get a recharge", that's their only purpose.'
[28] 'I have eight such villages where even if you don't give one buck, give them no recharge, still all children will come, and some of my villages are such that even if you give the 500 instead of 90 bucks, they will still not come.'
[29] 'They say, "There is no point [of vaccines], only what God wills happens, there is no benefit of these, death will still come, even if you give this vaccine, death will still come"…I say, "I am Muslim too", I also explain, "This is not a bad medicine, there is no side effect, [your children] will have lots of children [referring to sterilisation rumour], there is no problem", but they don't understand.'
'You are not greater than God, lady.'

Across districts, there are ‘hard-to-convince’ populations who are not affected by incentives. Some ANMs listed specific populations groups they felt continued to be unaffected by the programme, and the incentives specifically, such as migrant workers, daily wage workers and the Muslim community. In Panipat, the top reason given by ANMs for resistance to immunisation by the few still unconvinced (migrants, Muslims) was the fever brought on as an after-effect by a certain vaccine (penta)[30] and direct costs involved for daily wage workers.[31]

Some grandparent caregivers are also reported as being sceptical of the value of vaccination, having raised their unimmunised children into disease-free adults. In Mewat, ANMs report that those who do not live in tightly knit neighbourhoods are harder to convince and motivate because of their relative geographic isolation and lack of normalised everyday conversation about immunisation.

Additionally, some Muslims (some of whom might take offence at seeming to be bribed[32]) have not been affected by the incentive. In Palwal, unlike Mewat, there was an acknowledgement of increasing buy-in among Muslim communities. The only people who were not participating were daily wage workers who cannot forgo a day’s wages to take their child to an immunisation camp.

The negative impact of sterilisation rumours in Muslim communities. For ANMs in Mewat it was very clear that sterilisation rumours spread across certain Muslim communities have mitigated any potential effect that the incentives or wider programme might have had with these populations. Indeed, some ANMs said that the tablets themselves have a dissuading effect on the Muslim community, as they make it easy to identify their children and therefore intensify fears of sterilisation.[33] ANMs reported that sterilisation rumours received traction in Panipat too, although seemingly only in the particular area of Sanoli Khurd.

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[30] ‘This vaccine will only cause pain and will produce no benefit.’

[31] ‘”If we don’t go for work, our wages will get cut, what will we eat”, so this way their children get left behind.’

“In the scheduled caste area when I told them to get [their children] immunised, they told me, ‘Keep my wages here, 350 bucks, I will bring [my child] to get vaccinated, give me 700 bucks at home for two days’ [worth of work] and then I will vaccinate [my child]’.”

[32] ‘They simply refuse the recharge and say, “I don’t need your 90 bucks”...they say, “Come I’ll put money in yours”, they snap back and say, “How much do you need, I will put money in your phone”.’

[33] ‘Modi can see everything sitting at home, doing everything online.’

Appendix J: Cost data for programme implementation

Please refer to Online appendix J1 for a description of the cost-effectiveness methodology used; it describes both the cost allocation across treatment arms as well as the steps in the calculation taken to produce the cost-effectiveness ratios.

Online appendix J1

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-J1-Cost-effectiveness-Methodology-vF.pdf

Please refer to Online appendix J2 for the actual algorithm used to calculate the average cost and cost-effectiveness ratios. This spreadsheet starts with the total cost table, and through the subsequent tabs shows the steps and results of the analyses. This template is editable: by varying the cost package, it is able to produce different cost-effectiveness results, and is a useful tool when applying the findings from this study to other contexts. Instructions for its use are summarised in the template itself.

Online appendix J2

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-J2-Cost-Effectiveness-Analysis-Algorithm-vF.xlsx

Please refer to Online appendix J3 for the detailed budget submitted by the Haryana government for health services, including immunisation.

Online appendix J3

https://www.3ieimpact.org/sites/default/files/TW10.1119-Online-appendix-J3-Haryana-ROP-17-18.zip

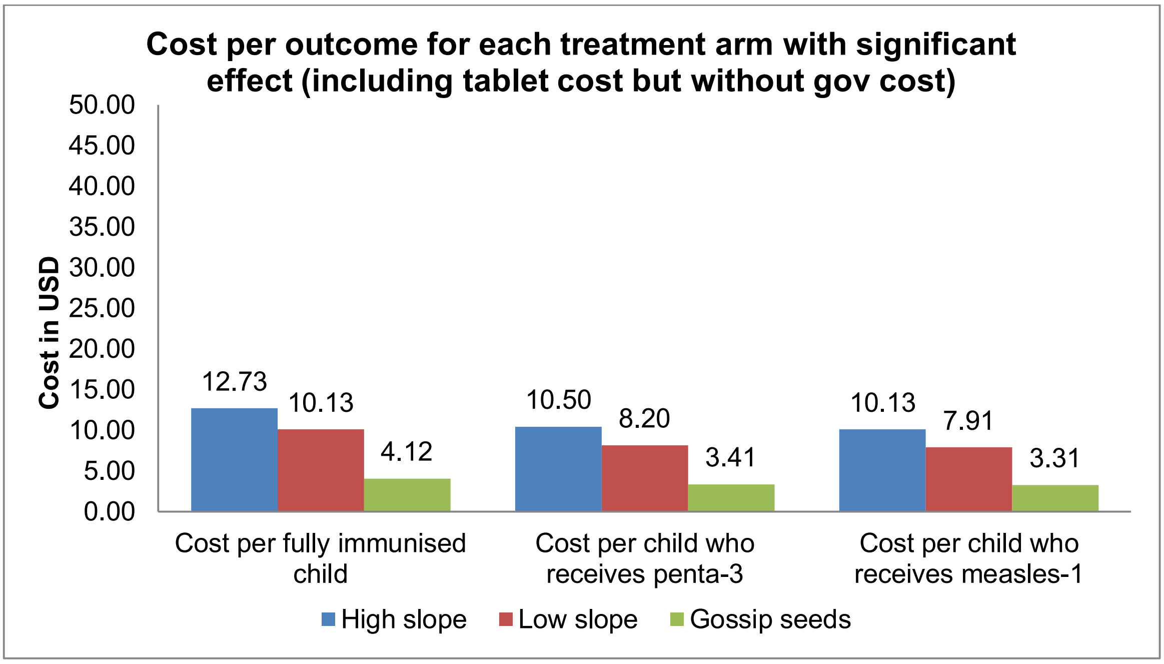
Variation on the main CEA

Our main CEA focused on a cost-effectiveness package that excluded the costs of the tablets and server, and included the estimated costs that the government would have incurred. However, this may not be very useful for a comparative CEA, depending on the context. As such, we present a variation of this analysis using a cost package that includes the costs of the tablets and server, and excludes the estimated costs of the government.

Average cost

The average cost for our programme as we implemented it is shown in Figure J1 below. This demonstrates the same finding as our main analysis – that the average cost for both high slope and low slope is higher than for gossip seeds. Compared to Figure 11 however, the average cost is about half that of the average cost when we include government costs, which confirms that the estimated government costs constitute a significant part of spending.

Figure J1: Average cost – variation of main analysis



Cost-effectiveness ratio

Figure J2 shows the cost-effectiveness ratio for each treatment, including tablet and server costs but excluding government costs. Again, we observe the same results as in Figure 12 in the main analysis: low slope incentives are more cost-effective than high slope incentives, but gossip seeds are the most cost-effective. The ratios are lower than in Figure 12 due to the exclusion of the government costs. Indeed, Figure J2 shows that the cost per additional fully immunised child in gossip seed villages is as low as USD0.65.

When considering incentives as transfers, we see that the cost-effectiveness ratio for high slope and low slope becomes substantially smaller and extremely attractive (Figure J3). Indeed, the reduction in the ratio between Figure J1 and Figure J2 is greater than the drop between Figure 12 and Figure 13 in the main analysis. This is because with a cost package that excludes estimated government costs, the cost of the incentives is a greater proportion of the variable costs and therefore carries more weight.

Figure J2: Cost-effectiveness ratio – variation of main analysis

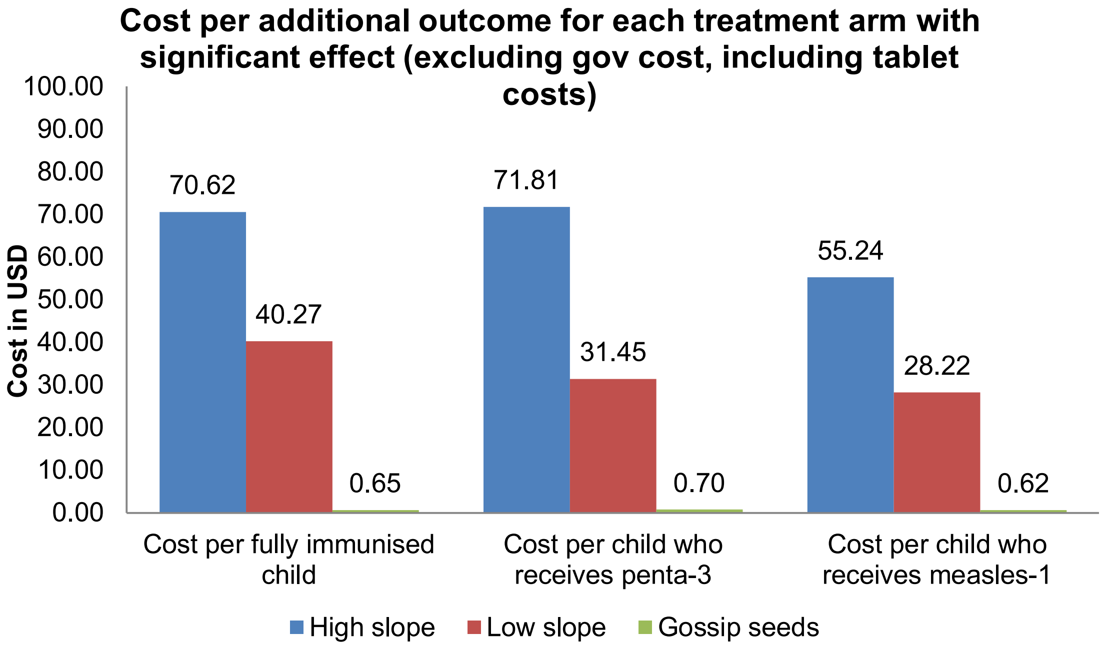
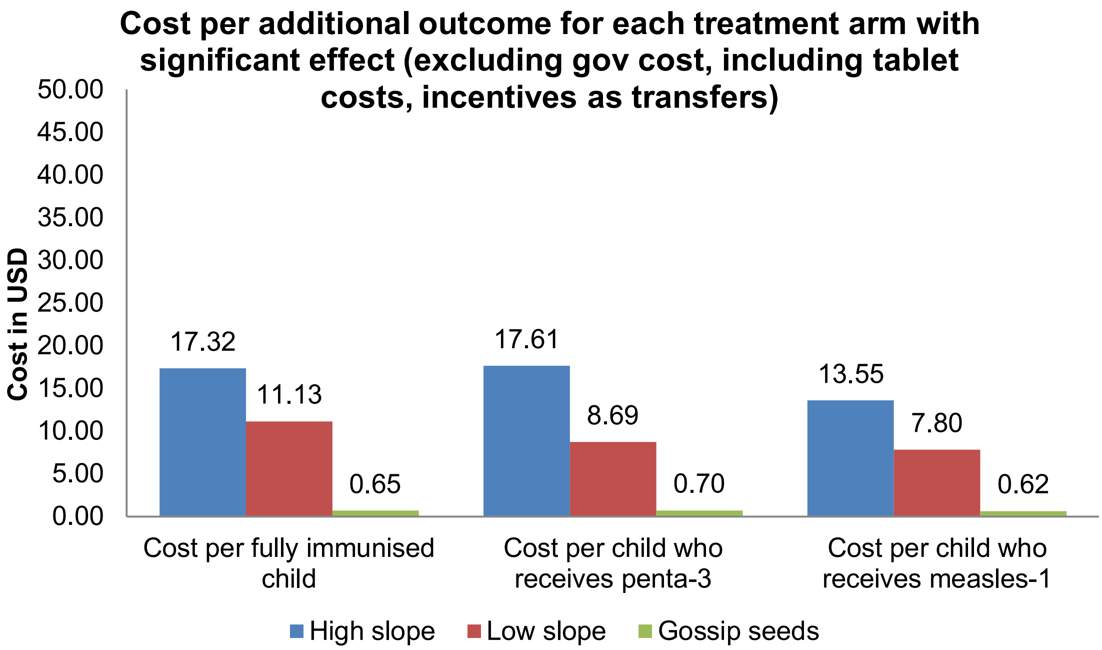


Figure J3: Cost-effectiveness ratio (transfers) – variation of main analysis



Online appendix K: Findings from caregiver interviews

Please refer to Online appendix K for detailed findings from the caregiver interviews.

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-K-Qualitative-report-from-HIMM.docx

Online appendix L

Online appendix L1: Session site monitoring instrument

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-L1-Session-Site-Monitoring-Instrument.pdf

Online appendix L2: Child verification instrument

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-L2-Child-Verification-Instrument.pdf

Online appendix L3: Process monitoring report

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-L3-Process-Monitoring-Report.pdf

Online appendix M

Online appendix M1: ANM focus group discussion guide

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-M1-ANM-Focus-Group-Discussion-Guide.pdf

Online appendix M2a: Interview guide for participating PCs_ENG

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-M2a-Inteview-Guide-for-Participating-PCs-ENG.docx

Online appendix M2b: Interview guide for non-participating PCs_ENG

https://www.3ieimpact.org/sites/default/files/2020-06/TW10.1119-Online-appendix-M2b-Interview-Guide-for-Non-Participating-PCs-ENG.docx

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