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Impacts of supportive feedback and nonmonetary incentives on child immunisation in Ethiopia

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About this report

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Executive summary

Despite substantial economic and development progress, Ethiopia's full immunisation rates remain low, with only 39 per cent of children aged 12–13 months and 22 per cent of those in the three lowest wealth quintiles fully immunised in 2016. In this study, we sought to explore two questions: (1) how to engage communities by using behaviourally informed feedback to mobilise healthcare workers around improving immunisation rates; and (2) how to create a positive reinforcement loop for those improvements using nonmonetary rewards.

The key outcomes of interest for immunisation rates were children receiving a full dose of the DTP/PCV vaccine,[1] full immunisation coverage, and incomplete immunisation. This information was collected for each vaccine through household surveys conducted before the intervention was implemented, and upon completion of the intervention. For health extension worker (HEW) service provision, the outcomes of interest were an increase in home visits from baseline to follow-up and an increase in HEW self-reported efficacy, which were measured in baseline and follow-up surveys with the HEWs.

Our impact evaluation design was a randomised controlled trial, which used random assignment to control and treatment groups at the health post level. The main intervention was a tracking poster, called the 'Protected Children' poster, which used a stamp system as a simple and salient way for HEWs to track immunisation achievement and dropouts.

The theory of change for this intervention postulates that public feedback via the posters will lead to additional HEW outreach to families with children in need of immunisation. This outreach will then lead to parents taking their children to immunisation clinics at health posts or other centres for immunisation. Improved community immunisation rates will lead to recognition for HEWs, which will start a positive reinforcement cycle for them, leading to improvements in self-efficacy and increased engagement with the community. The increase in immunised children will lead to lower overall morbidity among children.

Our study area consisted of 90 health posts from Marie Stopes International Ethiopia field offices in Aris and East Shewa zones in 12 *woredas* (districts) of the Oromia region. Of these health posts, 45 were randomly assigned to the treatment group, with the remaining 45 assigned to the control group, which only received the Ministry of Health's standard training. Marie Stopes field staff were responsible for recruiting, training and monitoring the HEWs. Participants were not blinded to their treatment assignment as they had to undergo additional training.

We observe that there is no statistically significant treatment effect on children receiving a full dose of DPT/PCV. The mean difference in full dose DPT/PCV between treatment and control health posts implies a non-significant reduction of 1.13 percentage points (p-value = 0.506) from the behavioural intervention. We found a 7.7 per cent increase in HEWs' household visits (p-value = 0.2) as a result of the intervention. Although only marginally significant across some models, these results indicate that treatment may have induced changes to HEWs' behaviour and effort but had no effect on immunisation rates.

[1] Diphtheria, pertussis and tetanus/pneumococcal conjugate vaccine

Although we find some suggestive evidence that HEWs' behaviour changed because of the intervention, we do not observe changes in immunisation. One potential reason is that the data were collected during a time when overall immunisation rates increased rapidly. Given other health system changes and economic growth that occurred in Ethiopia during data collection, we were not able to differentiate the additional impact of treatment from the underlying upward trend.

Also of note, the time between baseline and follow-up was substantially extended compared to our pre-analysis plan because of civil unrest and political instability in Ethiopia during data collection. These results indicate that even when evidence has been developed indicating that behavioural interventions are effective, they must be tested in a wide range of contexts to build evidence on what circumstances maximise the likelihood of impact.

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List of figures and tables

Abbreviations and acronyms

DPT	Diphtheria, pertussis and tetanus
ECIIN	Ethiopia Child Immunisation and Information Network
HEW	Health extension worker
HP	Health post
MSI-E	Marie Stopes International Ethiopia
PAP	Pre-analysis plan
PCV	Pneumococcal conjugate vaccine

1. Introduction

To make further progress in improving immunisation rates, Zerihun Associates – in partnership with Marie Stopes International Ethiopia (MSI-E) and ideas42, a non-profit firm that uses behavioural science to improve social outcomes – identified bottlenecks to immunisation, then created and implemented an intervention to address them. We did so by using behavioural economics to develop multiple designs, which were refined through an iterative process based on implementation feasibility, how well the design addressed the identified bottlenecks, and the costs associated with piloting and scale-up.

This intervention aimed to increase understanding of how behavioural economics can be used in healthcare, an area currently underexplored in the literature. We sought to explore two questions: (1) how to engage communities by using behaviourally informed feedback to mobilise healthcare workers around improving immunisation rates; and (2) how to create a positive reinforcement loop for those improvements using nonmonetary rewards.

This report first presents the intervention and research questions, including a discussion of the changes to the initial research questions. We then discuss the implementation of the intervention in detail, including changes made during implementation, and finally report on the impact that the intervention had on immunisation rates in the Oromia region in Ethiopia.

2. Background and context

Despite substantial economic and human development progress, Ethiopia's full immunisation rates remain low. In 2011, only 24 per cent of Ethiopian children aged 12–13 months were fully immunised with all basic vaccinations, and rates for the lowest wealth quintile were below 20 per cent (CSA and ICF 2012). In 2016, this rate improved, with 39 per cent of children 12–13 months being fully immunised. However, the rate for the lowest three wealth quintiles was 22 per cent (CSA and ICF 2016). This coverage rate is low compared to the global immunisation rate of 86 per cent of children aged 12–13 months worldwide, and 74 per cent in all African countries (Feldstein et al. 2017).

Full immunisation coverage for children under the age of 12 months in Ethiopia is defined as a child receiving a Bacillus Calmette-Guérin vaccination against tuberculosis; three doses of diphtheria, pertussis, and tetanus (DPT) vaccine; three doses of pneumococcal conjugate vaccine (PCV); at least three doses of polio vaccine; and one dose of measles vaccine (CSA and ICF 2016). WHO recommendations for immunisations among children under 12 months include all the vaccines mentioned for Ethiopia, as well as three doses of hepatitis B, three doses of haemophilus influenza type B, at least two doses of rotavirus and one dose of rubella (WHO 2019).

While there have been many attempts to involve communities in increasing immunisation rates, such efforts have mostly focused either on increasing awareness or interest in immunisation (Pramanik et al. 2018) or have employed direct (often financial) incentives (Basinga et al. 2011; Flodgren et al. 2011). Underlying the use of these techniques is the standard neoclassical economic assumption that people's actions directly reflect their intentions. This belief is standard and is frequently correct. However, behavioural science shows that there are many important situations wherein people's actions may not be consistent with their intentions or desires.

In particular, it is possible for a mother to be convinced of the importance of immunisation without her necessarily taking the required steps to immunise her children; therefore, further education or awareness-building may have limited effects. Additionally, a health extension worker (HEW) may have the intention to follow up with every child due for an immunisation visit, but may also have limited cognitive bandwidth to determine which children need follow-up, or may be distracted by other duties. As immunisation clinics are time-sensitive, ensuring that all eligible children attend clinics would increase their cost-effectiveness and efficiency.

Behavioural economics research shows that feedback focusing on making injunctive norms (what most people typically approve or disapprove of) more salient (Cialdini et al. 1990; 2006) and aligning injunctive norms with descriptive norms (what most people typically do) leads to an increase in the desired actions (Cialdini 2003). In a study looking at recycling habits, Cialdini and colleagues (1990) found that study subjects' littering habits improved (from 25.42% of people littering to only 10%) when their attention was drawn to an injunctive norm against littering. Another study looking at injunctive norms in relation to theft of natural resources and recycling called for further research into how communications can be structured in a way that makes messages more salient during the moment of action, not just when a decision is made (Cialdini 2003).

A study on vaccine use in Sierra Leone explores how descriptive norms can link to injunctive norms to improve vaccine uptake. It found that when a vaccine had a high perceived benefit (positive injunctive norm) linking it to a signal that a child received the vaccine (in this case, a bracelet), this had a large positive effect (an increase of 13.7 percentage points). Additionally, the bracelets provided social proof to the community that the parent was properly vaccinating their child (Karing 2018). These studies all touch on the evidence that most individuals make efforts to conform to behaviours that they perceive as social norms. Thus, correcting misperceptions or making the injunctive norm more salient leads to improved behaviours (Datta and Mullainathan 2014).

As with the literature on feedback making injunctive norms more salient, the literature on nonmonetary rewards does not include situations in which individual healthcare workers and communities are working together to improve a community's results. A study in Zambia explored the effects of providing non-financial and financial incentives to individual condom sellers. They found that the sale of condoms by hairdressers who were in the treatment group receiving nonmonetary rewards (stars posted on a progress thermometer and an invitation to a recognition ceremony for the highest sellers) were double those of either the control group or the treatment groups that received monetary incentives (Ashraf et al. 2012).

Ashraf and colleagues' work builds on the theoretical work of Besley and Ghatak (2005), which postulates that nonmonetary incentives are most powerful for individuals aligned with the mission of their organisation. In addition, social norm messaging among health workers increases the use of long-acting reversible contraception (Barofsky et al. 2018). Related to immunisation directly, social signalling through a simple, nonmonetary intervention has been found to increase completion of vaccination schedules for children in Sierra Leone (Karing 2018).

2.1 Impact evaluation

This evaluation sought to provide evidence on the efficacy of supportive feedback coupled with nonmonetary incentives in engaging HEWs and their respective communities to raise immunisation rates.

The main research questions sought to assess the extent to which supportive feedback and nonmonetary incentives could accomplish the following:

1. Bringing children into the health system for their first immunisations
2. Keeping children in the health system for continued childhood immunisations
3. Increasing the frequency of home visits by HEWs
4. Increasing the self-reported efficacy of HEWs

Our impact evaluation design was a randomised controlled trial that used random assignment to control and treatment groups at the health post (HP) level, the lowest health service delivery unit in Ethiopia. An HP has a catchment area roughly equivalent to a village, though it may sometimes include two villages. Cluster randomisation at the HP level enabled us to answer key research questions while respecting constraints on operational feasibility. Comparing treatment and control groups allowed us to quantitatively assess the impact of the intervention. The project utilised data on child-level immunisation status obtained from baseline and follow-up household surveys.

Some 45 of 90 HPs from the MSI-E field offices of Aris and East Shewa zones in 12 *woredas* (districts) in the Oromia region were randomly assigned to the treatment group, with the remaining 45 HPs assigned to the control group. In total, 2,700 households, equally split between treatment and control communities (based on their HP) were surveyed to collect baseline and follow-up data. Thirty households with children under three years of age were randomly sampled within each HP's catchment area.

At the follow-up survey, the same sample households were surveyed and all children below five years of age were included. This allowed us to collect data on all children surveyed at baseline. Data were collected using World Bank Survey Solutions Computer-Assisted Personal Interviewing® electronic questionnaires and analysed using STATA® software.

2.2 Intervention and research hypotheses

The objective of this intervention was to apply a behavioural science approach to increase uptake of all relevant vaccines for children aged under one year. The primary hypothesis was that a behavioural intervention that combined salient injunctive norms, social proof and nonmonetary rewards using 'Protected Children' posters would have positive average impacts on full immunisation coverage and the full dose of DPT/PCV. The proposed intervention aimed to evaluate the following questions, under the categories of immunisation outcomes and HEW service provision outcomes:

2.2.1 Immunisation

1. To what extent can nonmonetary incentives and various frequencies of supportive feedback bring children into the health system for their first immunisations?
2. To what extent can they keep children in the health system for continued childhood immunisations?

2.2.2 HEW service provision

1. To what extent can they increase the frequency of home visits by HEWs?
2. To what extent can they increase the self-reported efficacy of HEWs?

For immunisations, the outcome of interest was vaccination dropout rate, or whether a child received each vaccine according to the recommended vaccination schedule. The outcome variable ‘vaccination dropout rates’ refers to DTP1–DTP3 dropout rates, or loss to follow-up between the first DTP and last DTP vaccination. The secondary outcomes were measles vaccination coverage and no vaccinations. This information was collected for each vaccine through household surveys conducted before the intervention was implemented, at baseline, on completion of the intervention and during follow-up. For HEW service provision, the outcome of interest was an increase in home visits from baseline to follow-up and an increase in HEW self-reported efficacy, which were measured in baseline and follow-up surveys with the HEWs.

The main intervention was tracking ‘Protected Children’ posters, which incorporate a stamp system (Figure 1).

Figure 1: ‘Protected Children’ posters

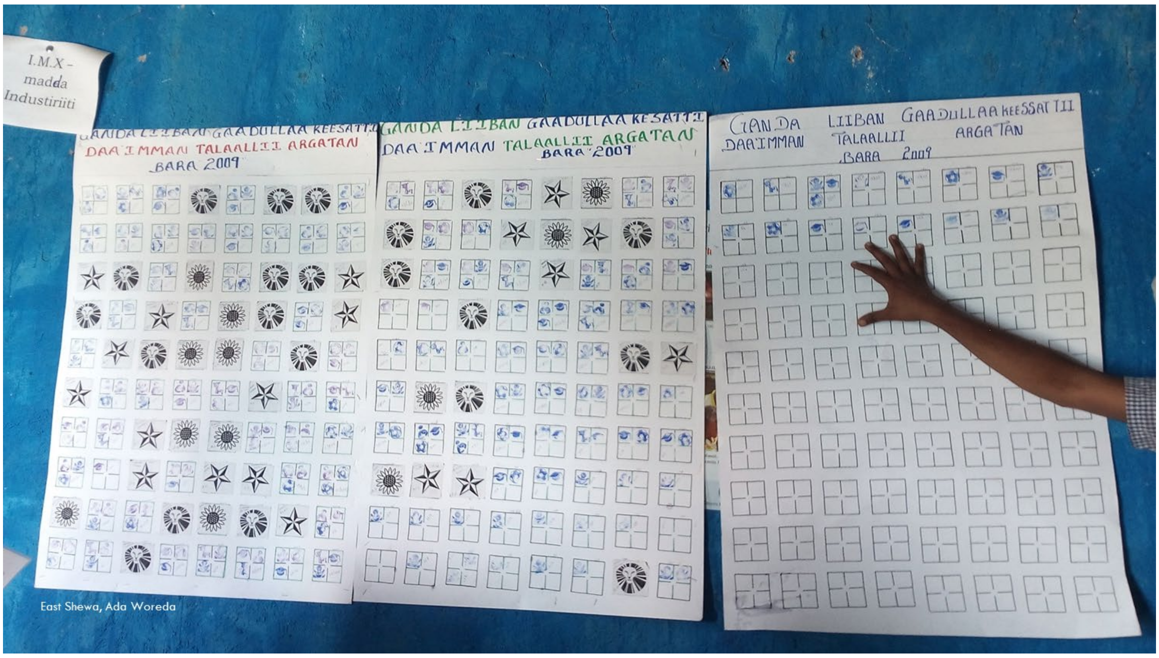


Photo credit: Project team

The posters and stamp system were designed to be a simple and salient way to track immunisation achievement and dropouts (Appendix A and Appendix B). These posters were hung on the wall of the HP to provide positive reinforcement for HEWs of their progress and to provide easy visual tracking. Each square on the poster represented a child and their specific immunisation schedule. The square was divided into four smaller squares, with each section representing a follow-up immunisation visit of 6 weeks, 10 weeks, 14 weeks and 9 months. Each time the caregiver and their infant went to the HP for an immunisation, the caregiver put a stamp in the appropriate section of their child’s square and additionally put a stamp on the immunisation card if they wished.

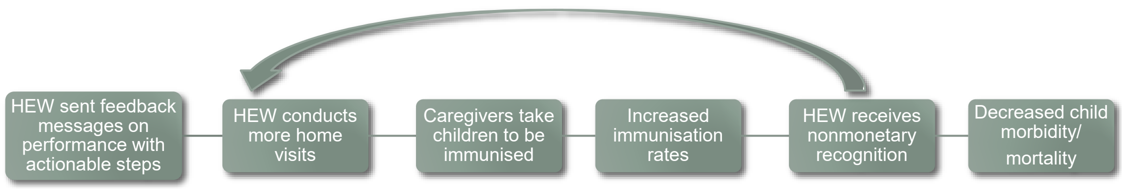
Caregivers chose from five stamps with aspirational or cultural values: a graduation cap, a football, a baby, a giraffe or a woman holding a child. When a child was fully immunised (i.e. all four squares had been completed), a completion symbol was placed over that square – a lion, star or sunflower – as visual recognition of those children who were fully immunised. The mother also took home a completion symbol, which was a black and white printout on a piece of paper. Over the duration of the research, the posters filled up with stamps and then filled up with completion symbols. The HEWs were given the option to leave the completed posters on the wall of the HP or take them down.

Initially, along with the tracking posters, the intervention was to include outreach SMS messages for HEWs. This was called the Ethiopia Child Immunisation and Information Network (ECIIN). A text message was to be sent to HEWs three days prior to the upcoming immunisation clinic, encouraging them to take a moment to make a plan to perform outreach specifically for immunisation. This message was designed to cut through the noise of the many activities that HEWs face on a daily basis and focus their energy when it is most applicable and effective to address immunisation defaulters. However, due to political unrest, which caused internet service inaccessibility because of the state of emergency declared by the Ethiopian government, the ECIIN was not implemented.

2.3 Theory of change

The theory of change (Figure 2) outlines the hypothesised causal pathway for inputs, outputs, as well as short- and long-term outcomes (both the initial theory and the updated theory once the ECIIN was cancelled).

Figure 2: Initial theory of change



The intervention uses feedback on individual HEWs' performance in terms of immunisation coverage through posters that make full immunisation of children a salient, injunctive norm. Nonmonetary awards spur HEWs to close the immunisation gap by driving up immunisation rates. Individual feedback included: providing information on the change in her own relative performance; immediate and salient tracking on how many children were completing all immunisations; and an easy visual showing which children were defaulting on their immunisations. This feedback was made public to the community, thus driving the nonmonetary rewards of social signalling through receipt of progress stamps and immunisation completion symbols to recognise HPs and households that demonstrated improvements.

The theory of change postulates that public feedback will lead to additional outreach to families with children in need of immunisation. This outreach will lead to caregivers taking their children to immunisation clinics at the HP or other centres. The improved community immunisation rates will lead to greater recognition of HEWs, which will start a positive reinforcement cycle for them, leading to improvements in self-efficacy and increased engagement with the community. The increase in immunised children will lead to lower overall morbidity for children. Below we detail the transmission routes and assumptions underlying each.

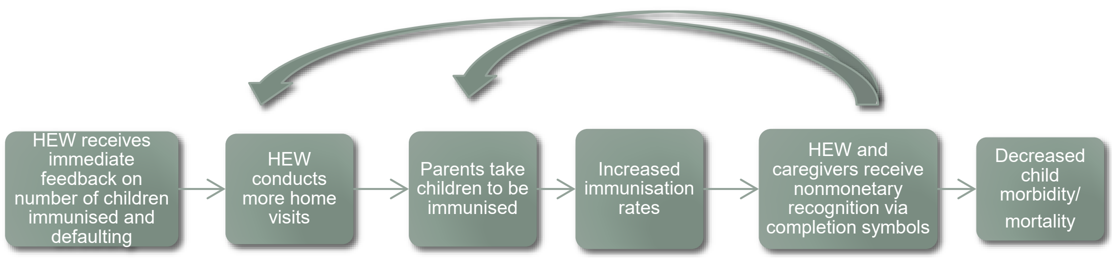
This theory of change is based on assumptions that community members see and understand the posters, and care about immunisation performance. If any of these aspects were missing, then the poster would not be an appropriate avenue for public recognition. In the areas where we implemented the design, scoping interviews and user testing of the posters showed us that it was likely that the community cared about immunisation. Additionally, when people in the health facility saw the posters, they asked what they represented because the images sparked their curiosity.

Since complete vaccination of a child under 12 months of age should be completed by the time they are nine months old, it is anticipated that intervention activities, including training and supportive supervision of the intervention's rollout, should be able to translate into full vaccination coverage within one year of complete intervention adherence.

As mentioned above, initially HEWs were to receive reminders about immunisation clinics and feedback on their work via SMS. This was meant to motivate them to conduct more home visits, thus leading to more parents bringing children to be immunised. Then, the HEWs were going to receive nonmonetary recognition via SMS and public display of the posters. Since the ECIIN system was not implemented, we pivoted our design to fully focus on the posters as public recognition for both HEWs and parents.

The fact that the ECIIN system was not implemented represents an important lesson related to independent verification of project activities (using mobile service provider records). Future implementation of interventions based on these designs should include this validation feature.

Figure 3: Updated theory of change with description of components after ECIIN cancellation



The following provides a description of the mechanisms in the updated theory of change:

- **Feedback to HEWs leads to more home visits** – The key mechanism is that the poster makes it easier for HEWs to track children who have not received timely immunisations. Additionally, this feedback makes immunisation status and non-compliance more visible in the community, leading the HEWs to take action. The assumption is that HEWs have the resources – including time, money and mental bandwidth – to complete home visits. HEWs should also believe that home visits have value, that they are an integral part of their role as community health leaders, and that they will result in parents taking their children to be immunised. Under the initial plan with the ECIIN, HEWs would have also received a reminder SMS before immunisation clinics to look at the posters and reach out to families who have children in need of an immunisation. This reminder would likely have increased the number of home visits and parents who attended the immunisation clinics.

- **More home visits lead to parents taking children to be immunised** – The causal mechanism is that a visit by a HEW will provide sufficient motivation for the parents to take their child to a health facility. In order for this to happen, there is an assumption that the parents will understand the importance of immunisation following the HEW visit. We also assume that the HEW will be able to visit the home when decision makers are present.
- **Parents taking children to be immunised leads to increased immunisation rates** – If parents take their child to be immunised, the vaccines are effective, and health workers record the immunisation, then immunisation rates for the community will increase. The facility also needs to have vaccines available (i.e. no stockouts) and parents need to be able to afford the vaccines (i.e. there are no hidden fees at the healthcare facility). Even without stockouts, if HEWs were under orders to prevent wastage by not opening new vials unless enough children were in the office to use an entire vaccine vial, this would also represent a threat to our theory of change. We did not encounter this situation, but if sufficiently prevalent, it would represent another constraint to the intervention's theory of change.
- **Increased immunisation rates lead to HEWs receiving nonmonetary recognition** – By increasing immunisation rates, the community and the HEWs will be able to see the community's statistics improve. If they improve enough for community members to see improved health outcomes, HEWs will earn nonmonetary recognition from the community for their efforts. The key assumption is that improvement will be enough to earn recognition. This recognition will create a positive feedback loop for more HEW home visits and greater community mobilisation, which we predicted to be the main activities that would increase immunisation. Recognition is also gained among caregivers of children, who can pick out a stamp to add to the poster with each immunisation, thus encouraging and reminding them to come back for the next round. The 'Protected Children' poster on the wall of the HP provides positive reinforcement for HEWs regarding their progress, and provides easy visual tracking. Caregivers choose from five stamps that may hold aspirational or cultural value. When a child is fully immunised (i.e. all four squares are completed), a completion symbol will be placed over that square as visual recognition of those children who are fully immunised. The mother can take home a completion symbol as well. In the original plan with the ECIIN system, HEWs would have received recognition and comparisons to other HEWs via SMS as well as via the poster, thus strengthening their awareness of supervision of improvements in their work.
- **Increased immunisation rates lead to decreased child morbidity and mortality**: If vaccines are effective, child morbidity and mortality should decline (assuming that preventable childhood illnesses are a measurable cause of morbidity). Vaccines should also not be counterfeit and should have been appropriately stored and checked for expiration dates to maintain their efficacy.

2.4 Context

2.4.1 Local context and participants

The intervention areas – Aris and East Shewa zones – cover 62 government health centres based in urban and semi-urban areas of Oromia, a populous region with poor immunisation coverage (12% according to the 2012 Demographic and Health Survey [CSA and ICF 2012]).

This programme targeted entire communities, which are composed of HEWs, households with unimmunised children and the broader community. While households with unimmunised children were the ultimate targets, the programme sought to explore how communities could empower these households to act. Thus, we considered the entire community as programme participants. At the same time, the intervention built on the existing health system, which enabled HEWs to identify households with children in need of immunisation.

The intervention was implemented in MSI-E's Aris and East Shewa zones field offices in Oromia. This has implications for the transferability of findings to other parts of Ethiopia as well as to non-Ethiopian settings. The intervention areas have an identical health system structure and infrastructure compared to the rest of Ethiopia: health centres typically supervise four HPs; most villages have their own HP and only a handful of villages share HPs; and each HP has one or two HEWs who provide information to that community and reach out to individual households. HEWs are Ministry of Health employees and are part of the community leadership. From a systems and infrastructure point of view, the threats to transferability of insights to the rest of Ethiopia are relatively limited.

The choice of community feedback mechanism (i.e. whether to disseminate feedback to communities using local leaders or public posters, or whether to rely on SMS messages) was driven by the social and cultural context in Oromia. To the extent that some social and cultural features in Oromia that influenced this choice may differ from those in other parts of Ethiopia, our intervention modalities may need to be adapted if scaled up to other parts of Ethiopia.

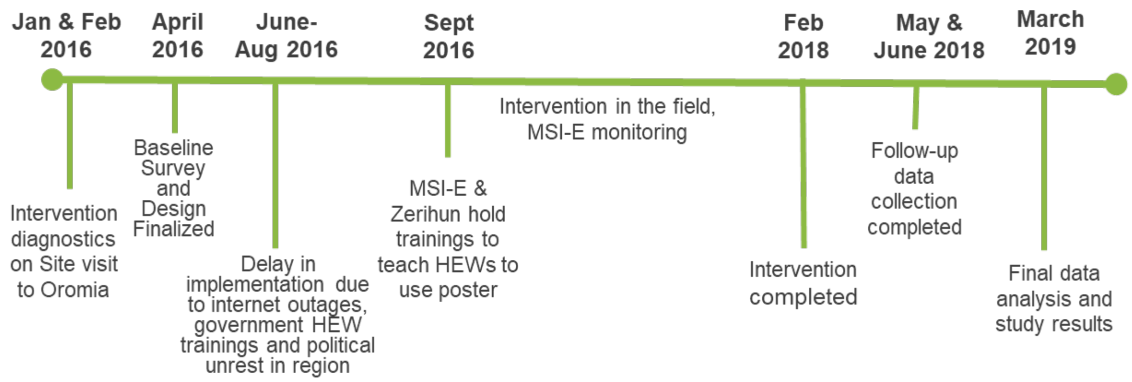
These features include relatively high rates of incomplete immunisation, overall support for immunisation as an effective health policy tool, and the existence of capable and trusted local HEWs. In addition, the intervention was implemented in the context of significant civil unrest. Although our qualitative work indicated that HEWs were trusted, instability and lack of trust in the government may have hindered the intervention's effectiveness.

Finally, we note that while this evaluation is intended to be informative in other resource-constrained, low-immunisation settings beyond Ethiopia, differences in the health system and the role of community health workers within it (e.g. whether they are paid, as in Ethiopia, or unpaid, as in several other countries in the region) will naturally influence the extent to which this assumption is justified. For example, a finding that non-financial recognition-based incentives are effective may not carry over to settings where HEWs are unremunerated. Other work using non-pecuniary social signalling to increase immunisation also indicates that one of the most important determinants of effectiveness relates to pre-existing perceptions of vaccine effectiveness (Karing 2018). The study from Sierra Leone supports immunisation completion benefits that can occur using social norms outside of Ethiopia (ibid.).

2.5 Timeline

The baseline survey occurred in April 2016. Implementation began in September 2016 and continued through February 2018, with two rounds of data collection during this period and a follow-up survey in May and June 2018. Final data analysis occurred in January–March 2019.

Figure 4: Timeline of intervention



2.5.1 Detailed timeline

- **January–February 2016:** problem definition, diagnosis of behavioural barriers and intervention design
- **April 2016:** baseline survey and user testing of intervention design
- **September 2016:** full-scale intervention started with stakeholder meetings
  - Delay of planned June–August implementation because HEWs were fully occupied during the summer, attending trainings run by the government.
  - Training for the posters included participation of extension workers from all 45 HPs. In addition, immunisation focal persons from the health centres and *woredas* of the HPs were included in the training, due to their support and follow-up needed in the future. A total of 85 HEWs and 29 immunisation focal persons participated. Training was mainly practical, and all HEWs practised on the posters. Materials needed for the intervention were distributed to HEWs for their HPs by the end of the training.
- **September 2016–February 2018:** MSI-E monitored implementation
- **February 2018:** intervention completed
- **May–June 2018:** follow-up survey conducted
- **January 2019–March 2019:** final data analysis

3. Intervention and evaluation

3.1 Evaluation and design methods

3.1.1 Ethical considerations

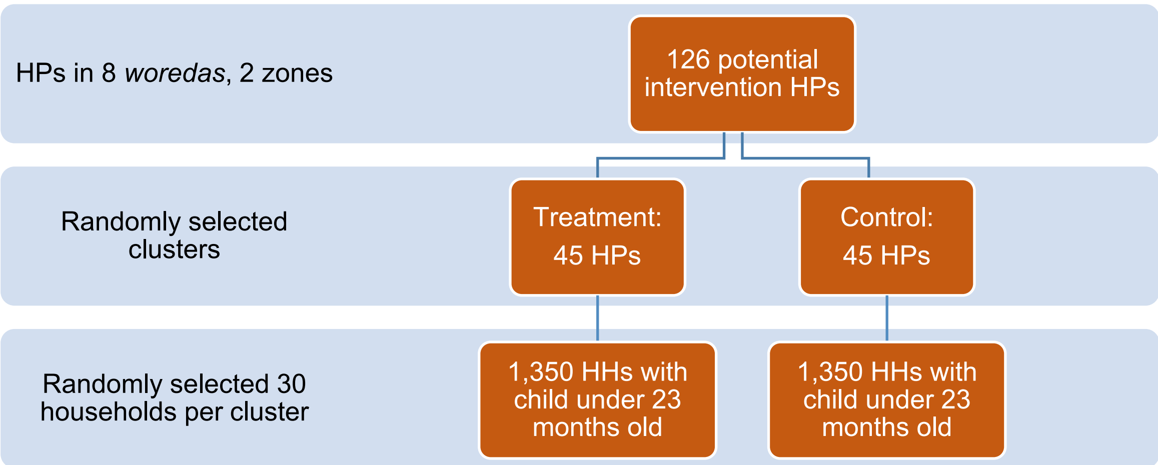
Ethical approval for this study was granted by the Ethiopian Public Health Institute (24 May 2016, Ref. #6.13/504). In addition, measures were taken to ensure ethical research. For each survey participant, consent forms were read, and verbal consent secured. Additionally, participants were informed that the information we obtained would remain strictly confidential and anonymous, and that they were free to stop the interview at any time. All data collected were anonymised and stored in secure locations.

3.1.2 Sampling strategy and methods

This study used cluster randomisation at the HP level to identify intervention impact. Both the control and treatment groups received the standard Ethiopian Ministry of Health procedures for health promotion activities and the immunisation schedule. The treatment group additionally received a behaviourally informed feedback and nonmonetary reward intervention.

The behaviourally informed intervention took place in 45 randomly selected HPs in the Arsi and East Shewa zones of Oromia. Randomisation was performed using a pseudo-random number generator. The other 45 randomly selected HP catchment areas remained controls for the study duration. In total, 90 HPs were selected, and 30 households were randomly sampled from each HP catchment cluster to be included in the household survey. In total, 2,700 households were surveyed, with equal numbers (1,350 households) selected for the treatment group (HPs with HEW outreach movement prompts and stamp system) and the control group (selected out of the outreach movement prompts and stamp system) (Figure 5).

Figure 5: Sampling strategy



Note: HH = household

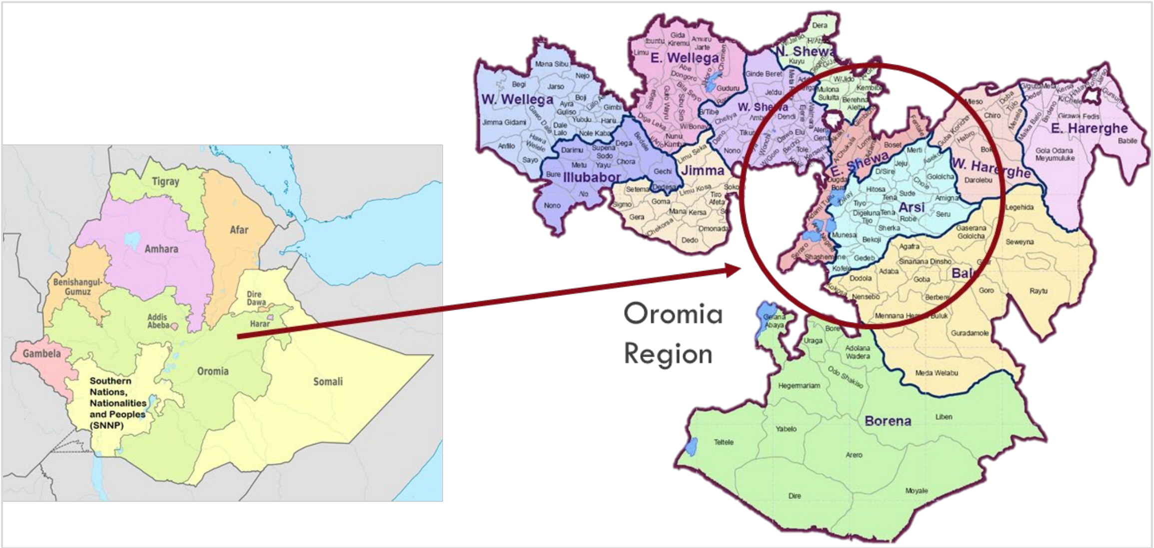
The recent household census of HP catchments conducted by the health extension programme of the regional government was used as a sampling frame to randomly select 30 sample households in each cluster. All households with children aged less than 23 months were included in the random selection of survey households. To capture the same cohort of children targeted at the baseline, our follow-up survey covered all children under four years old in the sampled households.

3.1.3 Project area selection justification

The primary considerations in selecting Arsi and East Shewa for this pilot intervention were: (1) the zones have a large number of districts with high dropout rates and HP catchment areas have some level of mobile phone coverage; and (2) the zones are geographically accessible to ensure the HP sample size is sufficient to rigorously evaluate the impact of the intervention. The overall selection frame was thus based on dropout rates, feasibility and the pilot's operational considerations.

Between the two zones, 8 *woredas* were selected based on the highest dropout rates. When a *woreda* was selected, all health centres and their attached HPs were automatically eligible for selection. Households from within these HPs were randomly sampled for inclusion in the baseline and follow-up surveys. There are, on average, two HEWs in each of the 129 selected HPs. On average, selected *woredas* have higher dropout rates than other districts in the region.

Figure 6: Map of Arsi and East Shewa zones of Oromia region



Source: UN Office for the Coordination of Humanitarian Affairs[2]

3.1.4 Power calculations

The Oromia region in Ethiopia has low levels of immunisation coverage. The 2011 Ethiopia Demographic and Health Survey calculated the current full immunisation rate for the region at 12 per cent. Therefore, we used that as our baseline, as opposed to the national rate of 24 per cent full immunisation coverage. Rates for households in the lowest three wealth quintiles nationally are less than 20 per cent (CSA and ICF 2012). For these sample size calculations, we assumed a significance level (an alpha) of 0.05 and a power level (a beta) of 80 per cent.

[2] https://reliefweb.int/map/ethiopia/reference-map-oromiya-region-ethiopia

As we employed a cluster-level design, we also estimated the intra-cluster correlation. Using the Ethiopia Demographic and Health Survey data, we estimated it to be between 0.21 and 0.25 and compared it to published values in the literature. Banerjee and colleagues (2010) found an intra-cluster correlation of 0.21 related to community immunisation in India, while a working paper from 2010 estimated the average intra-cluster correlation for full immunisation rates from 28 nationally representative demographic and health surveys in Sub-Saharan Africa to be 0.21 (Boco 2010). To remain consistent with the literature, we used a correlation of 0.21 for our calculations and performed a sensitivity analysis to ensure that it did not significantly affect the sample size.

Power calculations were completed using a 3ie sample size minimum detectable effect calculator with the parameters above. It estimated a minimum detectable effect of 8.6 per cent. Thus, we expected to be able to detect an increase in the full immunisation rate as small as 8.6% and a reduction in vaccine dropout by 8.6% – that is, for example, we expected to detect an effect in the reduction of dropout as low as 1.2 percentage points, that is to 12.3% from its current 13.5%.

Related studies in immunisation detected similar or higher-effect sizes. Banerjee and colleagues (2010) detected an impact of more than 50 per cent in immunisation with a nonmonetary incentive given to households, while Ryman and colleagues (2011) used a traditional training intervention to detect an increase of 30 per cent in full immunisation coverage in India.

3.1.5 Data collection instrument and fieldwork procedures

The questionnaires, baseline and follow-up were designed and tested to capture background information on household composition, basic demographics, education, and socio-economic and health status (Appendix C). However, the main focus was placed on information regarding health-seeking behaviour and immunisation levels. The questionnaire was translated into Afan Oromo, a commonly used language in the study area, and programmed using Survey Solutions® electronic survey software.

Prior to the baseline and follow-up surveys, enumerators employed by Zerihun Associates were given week-long trainings covering all aspects of the baseline and follow-up survey modules. Supervisors were deployed to follow up interviews and to ensure quality of data. Lists of HP clusters and corresponding household samples were provided to the field team. Thirty households per cluster were interviewed in their homes in both the control and treatment groups, totalling 2,700 respondents. Data from each interview were scrutinised by the survey supervisors and for a second time by the field manager. Every effort was made to ensure that the baseline and follow-up respondents were the same.

3.1.6 Data analysis plan

The study’s primary hypothesis is that a behavioural intervention that combines HEW outreach movement prompts and stamp systems may have positive average impacts on full immunisation coverage and vaccine dropouts. We grouped these into three primary outcomes: full immunisation coverage, full dose of DPT/PCV and incomplete immunisation. For these outcome variables we applied standard definitions from WHO guidelines. We also explored the secondary outcomes of measles immunisation coverage and no immunisations.[3]

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[3] The ‘no immunisations’ outcome included instances where immunisation data were missing.

The outcome variable of full immunisation is based on five individual immunisation variables: (1) Bacillus Calmette-Guérin and polio at birth; (2) polio 1, DPT-Hep1-Hib1, PCV1 and rota 1 at six weeks after birth; (3) polio 2, DPT-Hep1-Hib2, PCV2 and rota 2 at 10 weeks after birth; (4) polio 3, DPT-Hep1-Hib2, and PCV3 T 14 weeks after birth; and (5) measles and vitamin A at 9 months after birth. Each variable has two responses: '0' – denoting 'not received the vaccine'; and '1' – denoting 'yes, received a vaccine'. We created a new variable (labelled 'full_immunization'), which was then recoded as '1' if the child had received all doses of the vaccinations or '0' if the child had missed one or more doses.

The outcome variable 'vaccination dropout rate' refers to DTP1–DTP3 dropout rates and PCV1–PCV3 dropout rates. These indicators are measured as the absolute difference in percentage points between DTP1 and DTP3 or PCV1 and PCV3 in the study area, based on population-weighted average estimates. If the child received all three doses of both DTP and PCV, they were reported as 'full dose of DPT/PCV'.

The study measured exposure variables such as sex of child, age of mother, mother's occupation, number of children living in the household, religion, mother's education, father's education, household well-being, awareness of interventions of the project, distance to HP, ease of travel to HP, and use of HEW outreach. Place of residence and HP catchment areas were considered for the behaviourally informed exposure variables.

For both outcome indicators, the main analysis used a household survey. The change in these indicators between baseline and follow-up was considered as the impact of the project. An analysis of covariance estimation approach was adopted to measure how the project reduced dropouts and improved full immunisation. Similar analysis was carried out for other dimensions of marginalisation (e.g. children from poor households, mothers engaged in earning, parent/caregiver's level of education, etc.) and core immunisation outcomes.

The primary model was the following:

$$Y_{ipt\,=\,1} = \alpha + \beta_1 treatment_i + \beta_2 Y_{ipt\,=\,0} + M_{ijt=0} + X'\beta + \delta'\gamma + \epsilon_{ipt}$$

where Y_{iwt} is full immunisation or vaccine dropout for child *i*, in HP *p*, and during the follow-up period *t* = 1. Our coefficient of interest is an indicator for the child being located in a treatment HP, β_1. A control is included, $Y_{ipt\,=\,0}$, that reflects the outcome variable in the baseline period. $M_{ijt\,=\,0}$ is an indicator variable that reflects whether the baseline variable is missing. X' is a vector of stratification variables (use of HEW outreach, recognition of intervention, sex of child, distance to HP and ease of travel to HP).

Stratification variables were chosen by balancing the need to include characteristics likely to be associated with the outcomes of interest against feasibility of randomising increasingly small strata. We therefore focused on a combination of important child- and provider-level characteristics likely to impact HP access. Models were also run that included the variables that were not balanced at baseline, represented as δ', which include gender of household head, religion, monthly food expenditure, type of dwelling, days household went to sleep hungry, size of farming plots, perceptions of the local clinic being well-equipped for immunisation and distance to the nearest HP. Standard errors were clustered at the level of randomisation; that is, the HP level.

The main outcomes in this study were self-reported. Consequently, they could be subject to recall bias or social desirability bias differentially by treatment group and therefore bias our results. However, the survey also included a question for enumerators to verify immunisation status using vaccination cards. Most households (72%) reported that they did possess the vaccination card, but that is was not available at the time of survey; 20 per cent were able to verify their self-reported immunisation status by showing their vaccination card to the enumerator. The remaining households either never received a card or were not immunised.

Among the subset of individuals that were able to verify vaccination status using the card, we can be confident that recall and social desirability bias do not apply. To determine whether these biases are differential between treatment and control groups, and therefore impact the estimated treatment effect, we re-ran the treatment effect models only for those that were able to verify immunisation status with their vaccination card.

To estimate heterogeneous treatment effects, we ran models that investigated whether treatment effects varied by levels of civil strife in the areas around HPs (high, medium, low) during the treatment period and by levels of treatment fidelity (high, medium, low) using the following specification:

$$Y_{ipt=1} = \alpha + \beta_1 treatment + \beta_2 Y_{ipt=0} + \beta_3 treatment_i * civil\_strife_p + M_{ijt=0} + X'\beta + \delta'\gamma + \epsilon_{ipt}$$

The β_3 coefficient shows the differential effect of treatment in areas that experienced low levels of civil strife compared to the reference category (areas that experienced some civil strife). This intended to explore whether the civil strife that occurred in our treatment areas impacted the effect of intervention.

To address survey attrition, estimates were executed to determine whether survey attrition was related to treatment status:

$$A_{ipt=1} = \alpha + \beta_1 treatment_i + X'\beta + \epsilon_{ipt}$$

where $A_{ipt=1}$ is an indicator of whether household *i* in HP *p* dropped out of the study by not responding or being unable to be tracked for the follow-up survey. All other variable definitions remained the same. We tested β = 0 to determine whether survey attrition was related to treatment status. Since treatment status was not found to significantly affect attrition at 5 per cent significance level, all our estimates proceeded without any adjustment.

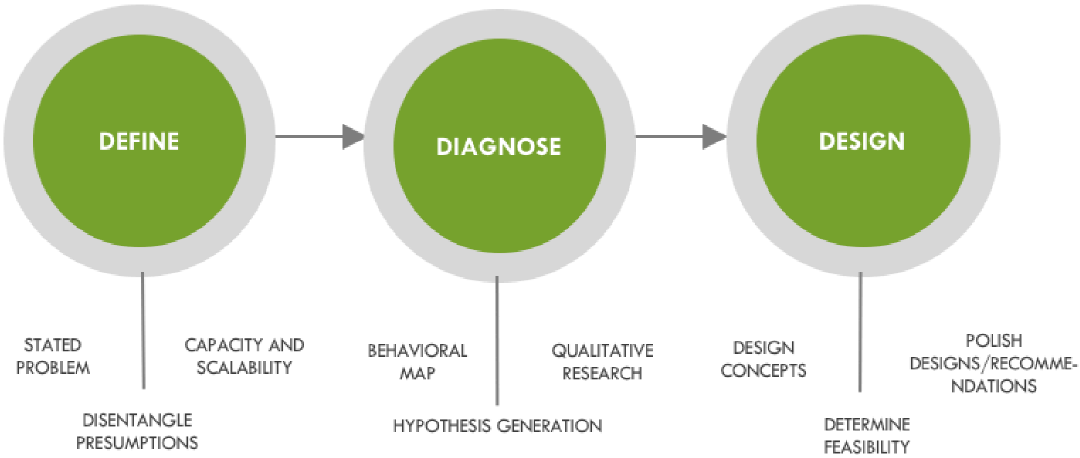
3.2 Implementation in practice

This first phase of the project involved three types of activities: diagnosis work for refinement of the intervention design; initial consultations with and approval from government entities and regulatory bodies; and preparation for the baseline survey and roll-out of the design.

ideas42 led the diagnosis and refining of the intervention design, with input from both Zerihun Associates and MSI-E. The process began with a review of the previously completed desk research on immunisation issues in Ethiopia and background on the Oromia region. From there, ideas42 completed its proprietary behavioural mapping process (Figure 7) to identify potential behavioural barriers and factors associated with channels of outreach preventing higher immunisation rates in Oromia.

The process was completed from the point of view of HEWs, as well as from the perspective of caregivers. Each of these barriers and channel factors were investigated during qualitative interviews between ideas42 staff and HEWs, their supervisors and women in the Arsi zone of Oromia during ideas42's site visit.

Figure 7: Behavioural economics framework



Source: Datta and Mullainathan (2012)

The diagnosis phase leverages insights from behavioural economics, a discipline that draws on economics and psychology to create a deeper and more realistic understanding of human behaviour. Behavioural economics demonstrates that interpersonal interactions and the design of spaces, programmes and processes can have a significant impact on decision-making and behaviour.

Our research methodology has been developed to better understand how these interpersonal interactions and environmental contexts affect how, when and why providers decide to provide health services (or not to) or, in some cases, why they fail to make a decision at all. We explored the behavioural factors that influence HEWs' intentions and actions to provide immunisation, but also caregivers' intentions and actions to seek out immunisation. Ultimately, we identified key behavioural barriers to HEWs' behaviour change related to providing immunisation services.

Following the site visit, ideas42 translated the diagnosis work into designs in consultation with Zerihun Associates and MSI-E. The key behavioural barriers identified during the diagnosis work, which the designs aimed to address, were: lack of feedback to HEWs; HEWs being overloaded with multiple tasks or messages and not having a specific moment to focus on immunisation; lack of salience of the benefits of immunisation for HEWs; mothers falling victim to time inconsistency and planning fallacy in relation to taking their child to immunisation days; and lack of social proof that infants are being immunised.

The preliminary design addressed these elements by including an improved tracking system that provided: visual feedback for HEWs; a time-specific message that encouraged HEWs to accurately plan for immunisation clinics; and social proof to give positive feedback to HEWs on their accomplishments and to demonstrate to mothers in the village that other women were immunising their babies.

The initial groundwork for this programme was initiated in 2010 in Ethiopia by MSI-E using an e-voucher intervention, which inspired the Ministry of Health to start the development of the ECIIN. The network, paired with the Frontline SMS system, was meant to send mobile-owning households and HEWs automated SMS messages about the long-term importance of vaccines, reminders about upcoming visits and information on missed visits, and the facility to register and issue e-vouchers.

The intervention planned to introduce behaviourally informed messaging into this system. Unfortunately, the ECIIN was never fully funded and the project was cancelled, so we could not include SMS in our final intervention. In each treatment HP, one poster was put up for HEWs to use. The quantity of symbol cut-outs provided to HEWs was not monitored, nor did the team collect information on how the posters were maintained. Implementation fidelity was assessed using a self-reported measure from the HEWs. However, because we could not verify how these assessments were made, we did not use them in our analysis.

In addition to cancellation of the ECIIN programme, there was unrest in the Oromia region during the project implementation phase, stemming from a land dispute between the federal government and some residents of the region. The unrest led to violence and deaths, with reports of journalists being detained. Initially, MSI-E reported that its activities in the zones we selected for the intervention were not significantly affected in the area, thus we did not add more intervention areas. However, the unrest later spread, and some intervention areas were affected (Table 3).

The political situation was the major challenge to implementation of the intervention. It resulted in the delay of training and several postponements in training schedules. The SMS intervention was not implemented because of internet service inaccessibility that resulted from the state of emergency declared by the Ethiopian government. Additionally, due to the unrest at the time of the implementation, some HPs lost records that would have served as an input as they had started using the posters, and many HEWs changed positions to live in more stable areas.

The ongoing unrest made it difficult for MSI-E to carry out all monitoring activities and for Zerihun Associates to complete the post-intervention qualitative interviews. As a result of this unrest, monitoring of exposure to treatment was not implemented according to the pre-analysis plan (PAP).

4. Impact analysis and results of key evaluation questions

In the baseline report, we found low literacy rates and a high proportion of traditional dwellings and households that were on the lower end of the wealth spectrum. We also found significant room for improvement in health behaviour, especially in the realm of immunisation. While rates for immunisation at birth were found to be high in the baseline survey, the rates dropped sharply for subsequent vaccination. Only 44 per cent of children in treatment and control areas were vaccinated with the final vaccination (measles at nine months). Outcomes were either recorded from children's immunisation cards or self-reported by children's caregivers or the person in the household who completed the survey. Since the number of cases of missing values was small, we omitted those values from the analysis.

With regard to the baseline balance, we provide the full results in the baseline report and appendixes of this report. Appendix Tables I1 and I2 provide the full set of covariates we test for balance. The tables show that some covariates are not balanced at baseline. We adjust for this in our analysis by including characteristics where we do not achieve balance in our treatment effect regressions to determine whether this imbalance impacts the treatment effect. As shown below, we do not observe substantive changes in the measured treatment effect when controlling for these unbalanced variables.

One reason for this is that because of our large sample size, we can have statistical difference even when variables differ by an amount that is not economically meaningful. For example, although distance to nearest HP differs statistically, the difference in means is 4.8 minutes, which we assess not to be an economically meaningful driver of care-seeking behaviour.

Further, the level of difficulty in travelling to HPs also differs, but this difference is also not meaningful. Some 49 per cent of respondents in control areas, and 47 per cent in treatment areas, reported that travel to the nearest HP is very easy. We add this context to the text to clarify how we address imbalance in the analysis (see Appendix Tables I1 and I2 for the full set of covariates we test for balance).

Overall, we test 539 covariates for balance. Among the continuous variables tested for balance, we find 5 of 21 unbalanced at baseline. Among the categorical variables tested for balance, we find 225 of 518 unbalanced. However, these are not independent observations because this calculation includes each category of a given variable in a separate t-test. If one category is unbalanced, it is likely that most or all others will be as well.

In addition, as described above, although we observe more unbalanced variables than would be predicted by randomly drawing from a distribution where the mean difference is zero, these differences are overall not economically meaningful. Finally, when we observe imbalance in a variable that could impact immunisation behaviour, we include it in our controls and do not observe substantive differences in our treatment effects.

4.1 Immunisation outcomes

When looking at summary statistics from the follow-up survey, we found that in our sample 86.5 per cent of children received all three doses of DPT and PCV (85.9% in the treatment group and 87.1% in the control group). From baseline to the follow-up surveys, the level of full dose DPT/PCV immunisation increased substantially, from a rate of 59% to 87%. Full descriptive statistics are in Appendix H.

Our main regression results on our primary outcomes of interest show the proportion of children receiving a full dose of DPT/PCV, the proportion of children with incomplete immunisation and the proportion of children with full immunisation (Table 1).

Table 1: Summary statistics from follow-up survey of primary and secondary outcomes

	Control (C)		Treatment (T)		Difference (C-T)	
	Mean	**SE**	**Mean**	**SE**	**Mean**	**SE**
Full dose of DPT/PCV	0.8705	0.0079	0.8590	0.008	0.0114	0.0112
n	*1,784*		*1,902*			
Incomplete immunisation	0.0532	0.0053	0.0573	0.00533	–0.004	0.0075
n	*1,784*		*1,902*			
Full immunisation	0.7247	0.0105	0.7208	0.0103	0.0039	0.0147
n	*1,784*		*1,902*			
Measles coverage	0.8160	0.0094	0.8181	0.0091	–0.0021	0.0131
n	*1,680*		*1,771*			
No immunisation/missing	0.062	0.0057	0.0720	0.0059	–0.0098	0.0082
n	*1,784*		*1,902*			

Note: SE= standard error; no immunisation/missing category represents an indicator variable for children who received either no immunisation or all immunisation data were missing.

Table 2 presents the regression counterparts of the final results, where panel A shows results for all households and panel B reproduces those results using only the subset of households with immunisation status confirmed using vaccination cards. Panel A, columns 1–3 show the results for regressions where the dependent variable is the probability of receiving all three doses of DPT/PCV. Panel A, columns 4–6 and 7–9 show the equivalent results when the dependent variable measures the probability of incomplete immunisation and the probability of a child being fully immunised, respectively. In all cases, the coefficient of interest is that on the treatment status.

All regressions are ordinary least squares, with robust standard errors clustered at the HP level. The basic specifications (columns 1, 4 and 7) have no additional controls; columns 2, 5 and 6 add controls for stratification variables (use of HEW outreach, recognition of intervention, sex of child, distance to HP and ease of travel to HP); and columns 3, 6 and 9 add controls for demographic variables that were unbalanced at baseline. The data point ‘missing’ refers to children in households where no follow-up data were recorded or were incorrectly entered. Since we were unable to report on their final immunisation status, they were assumed to not have been immunised (6% of children in the control group and 7% of children in the treatment group).

We observe that there is no statistically significant treatment effect on full dose of DPT/PCV across model specifications. The mean difference in full dose DPT/PCV between treatment and control HPs implies a non-significant reduction of 1.13 percentage points (p-value = 0.506) from the behavioural intervention. However, when we include controls for household- and HP-level characteristics, we observe that full dose DPT/PCV is a non-significant 1.05 percentage points (p = 0.527) higher in treatment areas compared to control areas.

Since 87 per cent of the children sampled received a full dose of DPT/PCV at follow-up in control areas, these non-significant treatment effects represent less than a 1.3 per cent change in full doses received. Given that the treatment effect changes sign across specifications, is not statistically significant in either specification and represents a small percentage magnitude change, we conclude that this treatment effect is a well-estimated null.

Table 2: Linear regression results, primary outcomes

Panel A: All respondents

	Probability of receiving all 3 doses of DPT/PCV			Probability of incomplete immunisation			Probability of being fully immunised		
Independent variables	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
Treatment	–0.01	0.00	0.01	0.00	0.00	–0.01	–0.003	0.01	0.01
(SE)	(0.0173)	(0.0162)	(0.0166)	(0.0084)	(0.0090)	(0.0090)	(0.0321)	(0.0296)	(0.0283)
p-value	0.506	0.981	0.527	0.671	0.733	0.483	0.924	0.587	0.574
Baseline value	0.06***	0.04***	0.04***	0.02*	0.01	0.01	0.05***	0.04**	0.04**
	(0.0139)	(0.0134)	(0.0139)	(0.0106)	(0.0111)	(0.0115)	(0.0187)	(0.019)	(0.0191)
Stratification variables	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Unbalanced variables	No	No	Yes	No	No	Yes	No	No	Yes
Constant	0.84***	0.81***	0.87***	0.05***	0.05***	–0.003	0.71***	0.63***	0.64***
	(0.0157)	(0.0238)	(0.0543)	(0.01)	(0.0107)	(0.0327)	(0.0228)	(0.031)	(0.0954)
Observations	3,686	3,165	2,946	3,686	3,165	2,946	3,686	3,165	2,946
R-squared	0.01	0.01	0.03	0.001	0.002	0.01	0.003	0.01	0.03

Note: SE= standard error; robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Panel B: Respondents who were able to verify immunisation status with vaccination card only

	Probability of receiving all 3 doses of DPT/PCV			**Probability of incomplete immunisation**			**Probability of being fully immunised**		
Independent variables	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
Treatment	–0.0338	–0.0097	0.0126	–0.0175	–0.0138	–0.0228	–0.0505	–0.0318	–0.0489
(SE)	(0.0472)	(0.0459)	(0.0443)	(0.0170)	(0.0199)	(0.0211)	(0.0532)	(0.0513)	(0.0470)
p-value	0.465	0.745	0.843	0.311	0.639	0.361	0.337	0.532	0.297
Baseline value	0.186***	0.164***	0.157***	0.0315	0.0182	0.0131	0.118***	0.112***	0.0978***
	(0.0337)	(0.0329)	(0.0351)	(0.0246)	(0.0280)	(0.0298)	(0.0331)	(0.0341)	(0.0343)
Stratification variables	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Unbalanced variables	No	No	Yes	No	No	Yes	No	No	Yes
Constant	0.564***	0.527***	0.557***	0.0917***	0.0894***	0.0713	0.478***	0.402***	0.360**
	(0.0405)	(0.0604)	(0.121)	(0.0132)	(0.0269)	(0.0971)	(0.0387)	(0.0562)	(0.181)
Observations	1,054	875	788	1,054	875	788	1,054	875	788
R-squared	0.039	0.059	0.129	0.003	0.010	0.024	0.015	0.037	0.112

Note: SE= standard error; robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2, panel A, columns 4–6 show results for the same models, but with incomplete immunisation as the outcome. We observe a similar pattern of small, non-significant changes to dropout. In column 4, the treatment coefficient is slightly positive at 0.004 (p-value = 0.671), but with full controls we observe a treatment effect of –0.006 (p-value = 0.483). We also observe a sharp drop in the percentage of children with incomplete DPT immunisations from almost 20 per cent at baseline to 5.5 per cent at follow-up.

Finally, panel A, columns 7–9 show the same models for full immunisation. The pattern is again similar, with treatment effects that are small and non-significant. Over the study period, the change in full immunisation is even more substantial than the other two outcomes, rising from 33 per cent at baseline to 72 per cent at follow-up. Overall, these results also support the conclusion from columns 1–3 that the behavioural intervention generated a well-estimated null impact.

Table 2, panel B shows similar results to panel A. All treatment effect coefficients across the three primary outcomes and model specifications are not statistically significant and are of small magnitude in terms of potential health impact. Although all treatment effect coefficients are negative point estimates, no p-values are below 0.25. Overall, this robustness check reinforces our interpretation of a well-estimated null result from regressions using all respondents.

In Table 3 we explore how civil strife affected the primary outcomes. The panels have the same controls per columns as Table 2. We find larger treatment effects in places with rare civil strife. The ‘treatment rare civil unrest’ row looks at the interaction between civil unrest and treatment status and the differential performance of HPs located in areas with rare or frequent civil unrest. In Panel A, the coefficient is positive, showing that areas with rare civil unrest have a higher probability of receiving all doses of DPT/PCV, but not in a statistically significant way. However, these coefficients give us a sense of what the treatment effect might have been if there had been no outbreak of civil unrest.

Table 3: Heterogeneous treatment effects on areas with civil strife (primary outcomes)

	Panel A: Probability of receiving all 3 doses of DPT/PCV			Panel B: Probability of incomplete immunisation			Panel C: Probability of being fully immunised		
Independent variables	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
Treatment (SE)	–0.0107	0.00525	0.0049	–0.00104	–0.000835	–0.0043	–0.0109	0.0130	0.0104
	(0.0196)	(0.0182)	(0.0190)	(0.0099)	(0.0106)	(0.0107)	(0.0354)	(0.0320)	(0.0303)
Constant	0.828***	0.802***	0.858***	0.0521***	0.0493***	–0.0127	0.712***	0.633***	0.636***
	(0.0161)	(0.0260)	(0.0502)	(0.0070)	(0.0117)	(0.0337)	(0.0239)	(0.0353)	(0.0862)
Baseline value	0.0581***	0.0442***	0.0418***	0.0203*	0.0148	0.0136	0.0489***	0.0392**	0.0423**
	(0.0136)	(0.0132)	(0.0139)	(0.0106)	(0.0111)	(0.0116)	(0.0186)	(0.0190)	(0.0192)
Rare civil unrest	0.0300	0.0446	0.0112	–0.00936	–0.00462	0.0141	–0.0142	–0.000747	–0.000735
	(0.0295)	(0.0274)	(0.0311)	(0.0114)	(0.0123)	(0.0140)	(0.0558)	(0.0572)	(0.0549)
Treatment*rare civil unrest	0.0105	–0.0124	0.0275	0.0207	0.0140	–0.0076	0.035	0.00707	0.0210
	(0.0374)	(0.0374)	(0.0333)	(0.0182)	(0.0193)	(0.0168)	(0.0748)	(0.0776)	(0.0715)
Stratification variables	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Unbalanced variables	No	No	Yes	No	No	Yes	No	No	Yes
Observations	3,686	3,165	2,946	3,684	3,165	2,946	3,686	3,165	2,946
R-squared	0.009	0.013	0.027	0.002	0.002	0.011	0.003	0.01	0.03

Note: SE= standard error; robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

From the baseline survey to the follow-up survey, the control group saw a slightly higher but non-significant percentage of households dropping out of the survey, with 93.8 per cent of treatment households and 92.2 per cent of control households remaining in the survey (Table 4).

Table 4: Survey attrition

	Treatment (T)	Control (C)	Difference (T-C)	p-value
Yes				
# cases	83	106	–1.6	0.08
% of cases	6.17	7.85		
No	1,262	1,243		
# of cases				
% of cases	93.83	92.15		

4.2 HEW service provision outcomes

Table 5 shows the relationship between HEW household visits and the behavioural intervention. We show that the mean difference between treatment and control areas in HEW visits is 7.5 percentage points (p = 0.068) with no controls. As we add controls, we see that the treatment effect exhibits marginal statistical significance (p-value < 0.10) for all models except for the final one, which includes controls for variables that are unbalanced at baseline. Even in this model, we find a point estimate that indicates 4.3 percentage points more households receive a HEW visit in treatment than control. This represents a 7.7 per cent increase in HEW visits (p-value = 0.2). Although marginally significant across some models, these results indicate that treatment may have induced changes to HEWs' behaviour and effort.

Table 5: Linear regression results for effect of intervention on HEW household visits

	1	2	3	4
Variables	**HEW home visit**			
Treatment group	0.0741*	0.0670*	0.0559*	0.0414
	(0.0407)	(0.0387)	(0.0341)	(0.0338)
Constant	0.554***	0.471***	0.405***	0.297**
	(0.0268)	(0.0299)	(0.0328)	(0.118)
Household ever visited by HEW		0.162***	0.124***	0.101***
		(0.0268)	(0.0251)	(0.0248)
Sex of child			–0.0103	0.00535
			(0.0155)	(0.0165)
HEW recognition for good performance		0.0878**	0.0876**	
			(0.0362)	(0.0368)
Difficult to travel to HP			–0.0971**	–0.0241
			(0.0418)	(0.0462)
Heard of intervention poster			0.335***	0.291***
			(0.0266)	(0.0302)
Sex of head of household				0.0912*
				(0.0474)
Orthodox Christian				0.273***
				(0.0597)

Catholic Christian				0.0106
				(0.0706)
Protestant Christian				0.0774*
				(0.0417)
Muslim				0.0815
				(0.0531)
Other religion				3.41e–05**
				(0.0000)
Traditional house (hut made from thatch or mud)				0.100***
				(0.0289)
Informal structure or shack				–0.262**
				(0.105)
Tent				0.228**
				(0.113)
Goes to bed feeling hungry				–0.0161
				(0.0119)
Minutes to walk from home to HP				–0.00244***
				(0.0008)
HP moderately equipped for immunisations				0.118
				(0.102)
HP fairly equipped for immunisations				–0.0107
				(0.104)
HP not equipped at all for immunisations				–0.258**
				(0.108)
Observations	3,443	3,443	3,165	2,946
R-squared	0.006	0.033	0.127	0.193

Note: robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

4.3 Divergence from the PAP

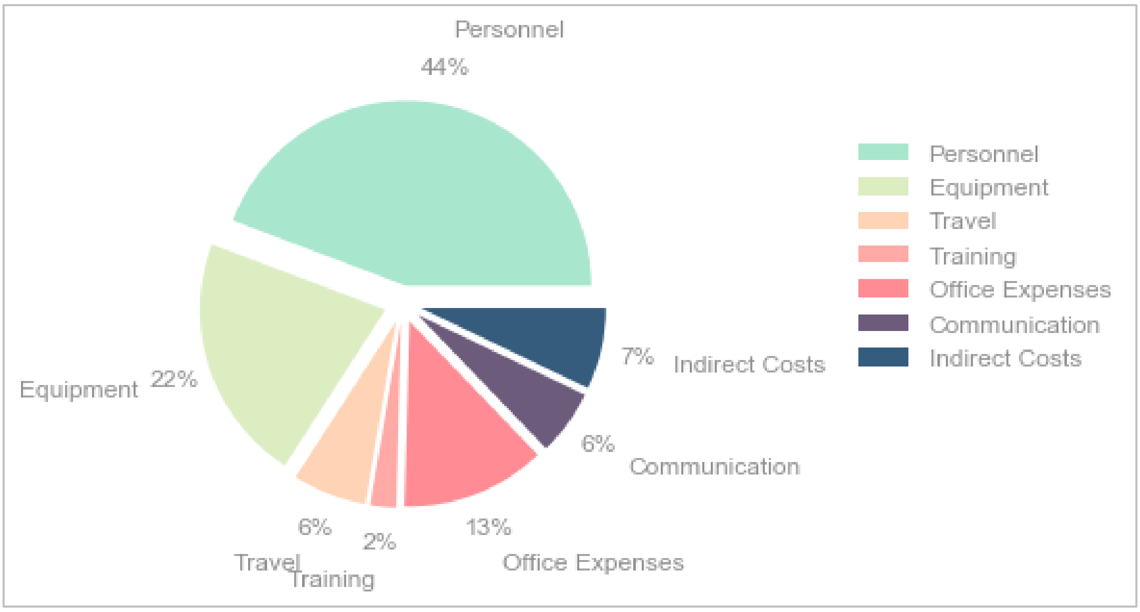
All attempts were made to follow the PAP. Despite this, we were not able to collect some planned *exposure* variables. These included: parity; birth order; birth; if the mother received antenatal care at least four times; and place of delivery. Due to the high number of rural households in our sample (98%), we were also unable to measure how the intervention reduced barriers to immunisation services among rural women, as the comparison group was too small.

Since the Oromia region experienced civil strife during the period of our study, we reported on the impact of the intervention as affected by this strife. As travel was restricted because of intermittent conflict in the region, we were unable to complete rigorous qualitative data collection or analysis. In addition to the analysis we planned in the PAP, which included full immunisation or incomplete immunisation, we also reported which children received a measles immunisation and no immunisations. Additionally, we explored how the intervention impacted the work of the HEWs by exploring self-reported survey data to see the extent to which the intervention increased the frequency of HEW home visits and their self-reported efficacy.

4.4 Cost of the intervention

A total of US$222,255 in costs was associated with project implementation. This included: project personnel costs of US$98,367 (44%); travel costs of US$14,325 (6%); training costs of US$5,000 (2%); equipment purchases of US$48,073 (22%); office expenses of US$28,000 (13%); communication costs of US$12,701 (6%); and project administration (indirect costs) of US$15,789 (7%) (Figure 8).

Figure 8: Nature of intervention-related expenditures



Training costs mainly included HEWs' training and workshops. Travel costs were related to domestic travel to project locations. Equipment purchases included smartphones and accessories, ECIIN application equipment, servers and promotional materials. Office expenses included fixed cost support to project field offices.

Cost per participant, obtained by calculating the total expenditure associated with project implementation divided by the total number of participants, was approximately US$3.31. According to the 2007 Ethiopia population and housing census, the 45 treatment villages in Arsi and East Shewa zones had a total population of 154,170 (CSA 2008). According to both our baseline and endline surveys, each household had an average of two children targeted by the project. Using the Oromia region average household size of 4.6 people (CSA 2008), we found that 67,031 children were direct project targets.

4.5 Contamination

Contamination of control participants has two related effects. It reduces the point estimate of an intervention's effectiveness, and this apparent reduction may lead to a type II error – that is, rejection of an effective intervention as ineffective because the observed effect size was neither statistically nor economically significant. Cluster randomisation is often used to minimise treatment 'contamination' between intervention and control participants.

Table 6 shows whether households in the control group were aware of the intervention. We asked study participants in both treatment and control villages if they had heard of and participated in the ‘Protected Children’ poster activities.

Table 6: Programme participation

		Mean (SE)		Difference	
	n (sample size)	Treatment (T)	Control (C)	T-C	p-value
% heard about the ‘Protected Children’ immunisation poster	3,681	0.258 (0.010)	0.205 (0.009)	–0.053	0.000***
% ever participated in ‘Protected Children’ immunisation poster activities	858	0.721 (0.020)	0.737 (0.023)	0.016	0.599
Average number of children who participated in ‘Protected Children’ poster activities	625	1.349 (0.036)	1.481 (0.046)	0.132	0.023**

Note: SE= standard error. * statistical significance at $p < 0.10$; ** statistical significance at $p < 0.05$; *** statistical significance at $p < 0.01$.

About 20 per cent of respondents in the control villages reported having heard about the ‘Protected Children’ posters; 74 per cent of those who had heard of them reported participating in the poster activities. Although the difference in the proportion of study participants in treatment and control villages who reported having heard about the ‘Protected Children’ intervention was statistically significant, in absolute terms the difference was small.

Results from a monitoring visit conducted by teams from Zerihun Associates and MSI-E in March 2018 (towards the end of project implementation) showed that there were no HPs in control villages with ‘Protected Children’ poster interventions, leading us to conclude that the programme participation questions in the questionnaire were not a measure of a spillover effect, but a result of acquiescence bias, where participants respond in agreement to all questions in the survey.

5. Recommendations and conclusions

5.1 Discussion

We did not observe changes in immunisation that can be attributed to this intervention. One potential reason is that the data were collected during a time when overall immunisation rates increased rapidly. Given the other health system changes and economic growth that occurred in Ethiopia during data collection, we were not able to differentiate the additional impact of treatment from that underlying upward trend.

Also of note is that the time between baseline and follow-up was substantially extended compared to our PAP because of civil unrest and political instability that occurred in Ethiopia during data collection. These results indicate that even when evidence indicates that behavioural interventions are effective, they must be tested in a wide range of contexts to build evidence on which circumstances maximise the likelihood of impact.

5.1.1 Internal validity

Our study faced various temporal threats. The main threat was the presence of conflict and civil strife in our study region. Due to this instability, many of the original HEWs in our study had to stop working or were moved to more stable regions. Of the original HEWs in the treatment group, 9.2 per cent dropped out before we conducted our follow-up survey, as compared to only 5.9 per cent of HEWs dropping out in the control group. This change in personnel and the closure of some HPs due to civil strife may have led to our intervention having little to no effect on vaccination rates. At the same time, we also observed an overall increase in immunisation rates, suggesting that the unrest might not have been the main reason for insignificant treatment effects.

Our study also had potential measurement effects. Since the stamp system called attention to vaccinations in the treatment group, HEWs knew they were being observed for childhood immunisations. This could have led them to focus their time on immunisation to the detriment of their other duties. However, the results for full immunisation use or incomplete immunisation came from household surveys, so the HEWs' knowledge of the intervention would not change how immunisation was reported by the caregivers. Our choice of testing instruments was also open to bias. The household and HEW surveys had a high potential for receiving answers skewed by response bias.

Households without immunisation cards to which the surveyor could refer might have stated that their children where immunised if they thought that was the 'right' answer. Additionally, HEWs might have reported more household visits or higher efficacy than they really felt, also as a result of social desirability bias. In our attempt to discover whether households in the control group were aware of the intervention, we asked both arms if they participated in 'Protected Children' poster activities. A total of 73.8 per cent of the control group and 72.3 per cent of the treatment group replied 'yes', leading us to conclude that this question was not a measure of a spillover effect, but rather the result of acquiescence bias.

The internal validity for this project was also affected by lack of qualitative research to confirm and explore our quantitative findings. As a result of the civil strife in the region, our study sites were intermittently inaccessible during the latter half of implementation and throughout our evaluation. This made conducting qualitative interviews with HEWs and households difficult. Thus, we were unable to discover the reasons behind the null effects found in our study.

5.1.2 External validity

Oromia was selected in part for its low immunisation rate (12%); therefore, this limits the generalisability of the results to regions of Ethiopia with higher immunisation rates. However, given the overall low rates in Ethiopia, as well as particularly low rates among the lowest socio-economic strata, this may not be a major concern. Nonetheless, the results from this evaluation may be most relevant to other low-performing regions in Ethiopia rather than to all regions.

As this is the only intervention we are aware of that has attempted to use a behaviourally designed reminder poster to increase childhood immunisations, we are unable to say if our findings are congruent with existing evidence. This study was the first attempt to study the use of community feedback and nonmonetary incentives to improve vaccination outcomes.

5.1.3 Impact of the deviation from treatment design on the results

The implementation of this intervention had very low fidelity to the original design. This intervention was designed with the expectation that the SMS message portion could be rolled out as part of the ECIIN, an immunisation tracking programme that planned to have HEWs report immunisation dates and mother and child data using SMS messages. Due to data issues and conflict in the region, ECIIN was cancelled. A large portion of the treatment design was based on the use of SMS messages with HEWs to: (1) remind them to check the 'Protected Children' posters to see which households needed a reminder about the upcoming immunisation clinics; (2) record immunisation outcomes; and (3) tell them how their work compared with that of their peers.

Since ECIIN was cancelled, we could not use the system to track the few text reminders that were sent out to HEWs, send peer comparison SMS messages or collect data; thus we have no results to report on this arm of the intervention. Additionally, we planned to use vaccination data from the ECIIN system in tandem with vaccination outcomes reported in the surveys.

In our final evaluation, we were only able to capture results from self-reported vaccinations in surveyed households; therefore, it is likely that we did not capture the actual numbers of fully vaccinated children in both the treatment and control groups. Self-reported vaccination rates open our results up to social desirability bias and acquiescence bias. Since childhood vaccinations are widely recommended by health providers in Ethiopia, it is likely that caregivers know that they are expected to vaccinate their children. Therefore, when asked if they have done so, social desirability bias would lead them to say yes, regardless of the true answer, leading us to overestimate vaccination rates through questionnaire data collection.

With regard to acquiescence bias, our questions were framed in a way that asked if children had received certain vaccines, so the caregiver could easily just say 'yes', again leading us to overestimate the vaccination rate. Both of these biases were likely present during the baseline and follow-up surveys and in the treatment and control groups, leading us to overestimate the overall vaccination rate, but they are unlikely to have affected our null result between treatment and control.

This left only the 'Protected Children' posters with the stamp system to provide positive reinforcement to HEWs regarding their progress, and to provide easy visual tracking as a reminder of which children lacked immunisations in treatment HPs. Additionally, the stamp system was an interactive way for a caregiver to participate in the child's vaccinations (by keeping records with the stamp and ultimately receiving a completion symbol to take home). Since this part of the treatment was rolled out with varying levels of fidelity, it is possible that any increase in vaccination rates in the treatment group could be attributed to this programme; however, the results showed no significant change in full immunisation.

5.1.4 Key lessons

For future research into feedback using salient injunctive norms and nonmonetary incentives in improving vaccination rates, we would recommend incorporating social benchmarking into the feedback. Behavioural economics research shows that providing feedback that enables individuals or communities to benchmark their own performance against that of an appropriate peer group can be an effective spur to action, especially when it primes and leverages a sense of pride in one's group identity and is combined with recognition for improvements and performance. Set up appropriately, such a system can lead to a virtuous cycle wherein individuals and communities strive to achieve higher performance on indicators that are rewarded, albeit non-monetarily (Cook and Berrenberg 1981). Providing feedback to individuals has been shown to improve pro-social and community outcomes.

In a healthcare context, performance quality increased among cardiac surgeons in the US who were given new feedback on their performance, compared to financial incentives (Kolstad 2013). A study on water conservation in Costa Rica demonstrated that providing households with feedback on their own water usage, and descriptive social norms of their community's water usage, reduced household water usage by 3.7–5.6% (Datta et al. 2015). Similar conservation results were found by providing feedback to households on electricity usage in the US (Allcott 2011).

In these cases, individual feedback resulted in improved community outcomes through changes in individual choices. We hope to build upon the literature by investigating the effect of the interaction between providing simultaneous, actionable and supportive feedback to individuals and outcomes for the community as a whole. It is possible that one reason we found null results in this study – whereas other studies using feedback found positive changes (Allcott 2011; Cialdini et al. 2006; Cook and Berrenberg 1981; Datta et al. 2015; Karing 2018; Kolstad 2013) – is that we were unable to incorporate peer comparisons between HEWs into our feedback.

Furthermore, it is important that researchers choose appropriate data sources that allow them to more rigorously verify that improvements are attributable to the intervention. Relying on household baseline and follow-up surveys does not guarantee that you capture all children being vaccinated. It is possible that the households in the survey did not have children younger than two years during the study period, or that they did not keep their own records about vaccination. It would be more appropriate to supplement survey data with vaccination reports from HPs.

Surveys are useful in assessing barriers to vaccinations and determining how households feel about the services provided by HEWs; however, they are not effective for tracking incomplete immunisation or full immunisation coverage. Our study would also have benefitted from an evaluation plan that detailed how to track and record treatment fidelity. Additionally, contingency plans should be incorporated into the evaluation plan in the case of unexpected civil strife.

We recommend that this study be carried out in other settings with low immunisation rates that have better mobile phone coverage and are not experiencing civil strife. This is a low-cost intervention that has the potential to use behaviourally informed feedback and reminder mechanisms to increase rates of vaccination follow-through and motivate

HEWs by tapping into their intrinsic motivations. While our study showed no effect, a similar study with the ability to track vaccination rates more rigorously and to ensure treatment fidelity may show a positive effect on increasing immunisation rates and minimising incomplete immunisation.

5.2 Policy implications

Despite the null results of this study, policymakers in Ethiopia still need to explore interventions that can increase immunisation rates, especially in regions prone to civil strife. Immunisations are a cost-effective health intervention that not only reduce morbidity and mortality, but also maximise the full lifetime potential of children and the economic health of the countries in which they live (Bärnighausen et al. 2014).

With a full immunisation rate of only 39 per cent among children aged 12–13 months, it is necessary for the Ethiopian Ministry of Health to try a low-cost intervention A recent study on immunisation default in Ethiopia showed that lack of systems to track defaulters is the main reason for default (Zewdie et al. 2016). The ‘Protected Children’ posters with the stamp system is a low-cost intervention to fill this gap and potentially decrease default.

To maximise the impact of an immunisation intervention in a large, varied and challenging context, a comprehensive approach involving both larger systemic redesigns and smaller-scale changes is needed. The Ethiopian Ministry of Health has already spearheaded a number of interventions, from encouraging nomadic families to vaccinate their children through community champions (WHO 2012) to rehabilitating and enhancing the temperature-controlled supply chain (Belete et al. 2015). Applying behavioural principles could help to further optimise these improvements – specifically, by using behaviourally informed feedback (Wahlström et al. 2003) to mobilise healthcare workers to improve immunisation rates in tandem with nonmonetary, visual incentives (Kolstad 2013).

Online appendixes

Online Appendix A: Sample design English

https://www.3ieimpact.org/sites/default/files/2021-03/TW10.1125-Online-appendix-A-Sample-Design-English.pdf

Online Appendix B: Sample design Oromo

https://www.3ieimpact.org/sites/default/files/2021-03/TW10.1125-Online-appendix-B-Sample-Design-Oromo.pdf

Online Appendix C: Survey instruments

Online Appendix D: Pre-analysis plan

https://www.3ieimpact.org/sites/default/files/2021-03/TW10.1125-Online-appendix-D-Pre-analysis-plan.pdf

Online Appendix E: Sample size and power calculations

Covered in the main report's section 3.1.4

Online Appendix F: Monitoring plan

https://www.3ieimpact.org/sites/default/files/2021-03/TW10.1125-Online-appendix-F-Monitoring-Plan.pdf

Online Appendix G: Treatment group and civil unrest by zone

https://www.3ieimpact.org/sites/default/files/2021-03/TW10.1125-Online-appendix-G-Treatment-Group-and-Civil-Unrest-by-zone.pdf

Online Appendix H: Descriptive statistics

https://www.3ieimpact.org/sites/default/files/2021-03/TW10.1125-Online-appendix-H-Descriptive-Statistics.pdf

Online Appendix I: Balancing tests

https://www.3ieimpact.org/sites/default/files/2021-03/TW10.1125-Online-appendix-I-Balancing-Tests.pdf

Online Appendix J: Costs

We do not have this information.

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