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Exploiting social influence to magnify population-level behaviour change in maternal and child health: study protocol for a randomised controlled trial of network targeting algorithms in rural Honduras

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ABSTRACT

Introduction: Despite global progress on many measures of child health, rates of neonatal mortality remain high in the developing world. Evidence suggests that substantial improvements can be achieved with simple, low-cost interventions within family and community settings, particularly those designed to change knowledge and behaviour at the community level. Using social network analysis to identify structurally influential community members and then targeting them for intervention shows promise for the implementation of sustainable community-wide behaviour change.

Methods and analysis: We will use a detailed understanding of social network structure and function to identify novel ways of targeting influential individuals to foster cascades of behavioural change at a population level. Our work will involve experimental and observational analyses. We will map face-to-face social networks of 30 000 people in 176 villages in Western Honduras, and then conduct a randomised controlled trial of a friendship-based network-targeting algorithm with a set of well-established care interventions. We will also test whether the proportion of the population targeted affects the degree to which the intervention spreads throughout the network. We will test scalable methods of network targeting that would not, in the future, require the actual mapping of social networks but would still offer the prospect of rapidly identifying influential targets for public health interventions.

Ethics and dissemination: The Yale IRB and the Honduran Ministry of Health approved all data collection procedures (Protocol number 1506016012) and all participants will provide informed consent before enrolment. We will publish our findings in peer-reviewed journals as well as engage non-governmental organisations and other actors through venues for exchanging practical methods for behavioural health interventions, such as global health conferences. We will also develop a 'toolkit' for practitioners to use in network-based intervention efforts, including public release of our network mapping software.

Trial registration number: NCT02694679; Pre-results.

Strengths and limitations of this study

- The sample includes a full population of individuals in 176 villages, 30,000 people.
- Measures capture comprehensive network data inclusive of a wide range of relationships.
- Intervention assesses the impact of network targeting on a variety of important reproductive, maternal, neonatal and child health health outcomes.
- A primary study outcome is a tool to allow interventionists to do network targeting with fewer resources.
- Limitations are that study results may depend on the geographical or public health setting.

BACKGROUND

Neonatal mortality in Honduras

Despite global progress on many measures of child health, rates of neonatal mortality remain high in the developing world. Neonatal mortality now accounts for about 40–50% of under -5-years child deaths.[1 2] Intrapartum complications, prematurity and infections—including sepsis, pneumonia and meningitis—account for the majority of these deaths.[1 3] About 75% of neonatal deaths occur in the first week of life.[4]

Honduras has made considerable progress in its efforts to improve the health of it population[5] but it still lags behind much of Mesoamerica. In Honduras in 2008, neonatal deaths accounted for 51% of all deaths of children under 5 years of age; 40% of these deaths were attributable to premature labour[6] and another 40% were attributable to asphyxia and infection. Furthermore, 57% of all births occur in rural areas where perinatal care may be insufficient or unsafe.[7] Although 79% of neonates start breast feeding within an hour of birth, only 30% are exclusively breast fed for the first 6 months of life.[5]

Although many deaths can be prevented through provision of clinical care services, emerging evidence suggests that a substantial reduction in poor reproductive, maternal, neonatal and child health (RMNCH) outcomes can also be achieved with simple, low-cost interventions within family and community settings.[8–10] The challenge is how to implement interventions in low-resource environments where health systems are often weak, and where intervention delivery is often dependent on short-term funding cycles.[11] Another equally fundamental question is how these interventions might be delivered so that communities actually adopt the behaviours being promoted, and how to ensure that those behaviour changes are sustained. To promote improvements in key RMNCH behaviours, change is needed in the provision of service and also in community-level demand for service and practices.

Behaviours related to RMNCH care are often socially reinforced, and can therefore be difficult to change, particularly in traditional cultural settings.[8] In these contexts, the study of norms, influence and social position are important to understand the functioning of interventions attempting to improve RMNCH outcomes. Social network analysis can be used to understand these dynamics. In particular, two distinct but interacting social network mechanisms can affect health decisions: contagion and connection.[12] Contagion refers to the spread of a behaviour from individual to individual. Connection refers to the standing of the individual within the wider social structure (see figure 1). While exposure to a new idea or behaviour through social contacts can influence an individual to change one's behaviour,[13] an individual's overall position within the larger network can also impact the possibility of behaviour change.[14] For example, people who are on the periphery of a network may have less access to important resources, simply by virtue of where they are positioned within the network, while people at the centre of the network may be less willing to change behaviour because their actions are under greater scrutiny.[15 16] In other words, social position can often promote or hinder exposure to new ideas, while the social cost inherent in adopting a new behaviour can also differ according to one's position in the network. Those most socially central, for example, may be those who are expected to uphold strongly established norms.[12]

Social networks are therefore a highly relevant substrate for the impact of social norms.[12 17 18] A social-norms perspective on behaviour change considers the choices of individuals to be significantly affected by the behaviours and/or opinions of those in their salient reference groups, or those to whom an individual turns for expectations regarding behaviour.[19 20] If social norms are the driving force behind behavioural decisions,

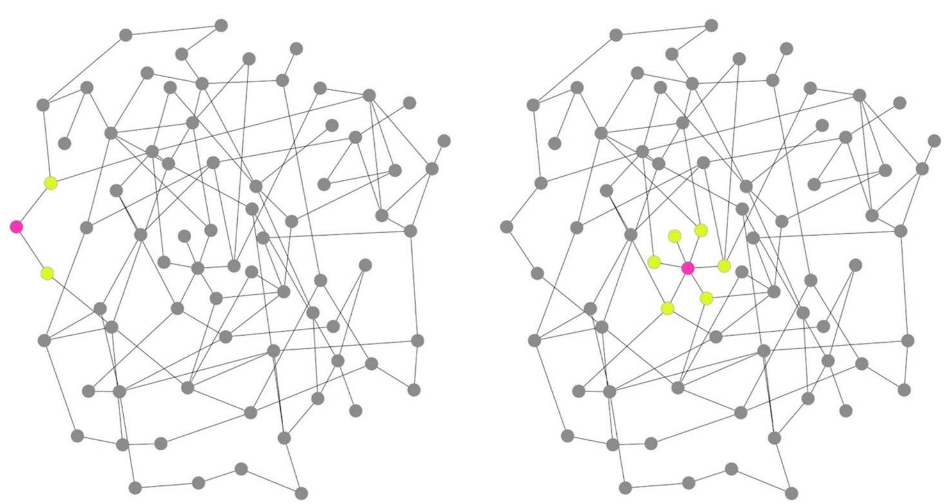

Figure 1 Variation in structural position in a network. Different individuals are typically able to exert variable amounts of social influence depending, in part, on both their number of connections and their location within the larger social network. The person in the left panel (red node) has two network ties (degree=2) and occupies a peripheral position in the network. In contrast, the person in the right panel has six connections (degree=6) and holds a central position in the network. The extent of potential spill-over effects a person may induce is generally likely to be higher for the node on the right than for the node on the left.

looking at the structure of a community's social networks might help us to better understand why individually focused behavioural interventions may be ineffective. Since the transmission of norm-changing behaviour (such as those promoted by many community interventions) often requires multiple reinforced exposures, it can be initially inhibited by highly connected networks and yet also ultimately requires the reinforcement of such networks to successfully occur.[21 22] The effect of injunctive norms, or what people believe is approved of within their reference group,[23] may be more powerful in highly interconnected communities. The promotion of 'acceptable' behaviours and sanctioning of 'unacceptable' behaviours may be stronger.[18 24–26] Yet, this dynamic also works inversely, such that when a critical mass of a highly interconnected group has adopted a behaviour, the probability that any individual in that group will adopt also increases.[21 27–29]

Owing to the powerful impact of social networks on human behaviour, social network targeting (where structurally influential individuals are selected for the receipt of interventions) shows great promise in the implementation of sustainable community behavioural change. Recent work by our group in a different part of Honduras has demonstrated that interventions targeting friends of randomly selected individuals might be more effective than interventions targeting those individuals themselves.[30] A school-level intervention on bullying achieved the greatest reduction in student conflicts through the targeting of 'social referents', or those within the top 10% of individuals according to how many friendship nominations they received.[31] Smoking and alcohol cessation programmes that exploit peer influences that modify the social network of the target have been shown to be more successful than those that do not.[32–38]

In sum, social network targeting represents a paradigm shift in how we currently implement interventions in global health settings. Many behaviour-change interventions currently seek to target all members of a population; however, face-to-face counselling for behaviour change takes time and resources. Ideally, successful social network interventions methodologies could mean that intervening in smaller segments of the populations will have the same effect as targeting 100% of the community, saving considerable time and money.

This study

Our objective is to conduct a randomised, controlled trial (RCT) of novel social network targeting techniques in order to explore how social network dynamics affect the uptake, diffusion, and group-level normative reinforcement of key RMNCH behaviours and attitudes in rural Honduras. Health behaviours will be promoted through a community-level household-based intervention that will be implemented by the Inter-American Development Bank (IDB) through the Salud Mesoamerica Initiative (SMI), with whom we are partnered for this project. We will use theoretically derived algorithms to choose a subset of structurally influential individuals from within the population to receive an intervention. Throughout this protocol the term 'treatment' refers only to the algorithms used to choose individuals, while the term 'intervention' refers only to the programme designed to promote positive RMNCH behaviours and attitudes. Our 'treatment' here is not the intervention we are using, but rather the algorithms used to choose a subset of structurally influential individuals from within the population receiving the intervention. We will assess whether differential network targeting results in differential uptake (ie, differential practice) of the RMNCH behaviours and attitudes promoted in the intervention.

We will randomise 176 villages using an 8×2 factorial design in which we will (1) vary the proportion of people targeted per village in 8 distinct groups and (2) compare random targeting to targeting friends of randomly selected people (see figure 2). Since we are intervening at the household level, our random targeting versus friend targeting randomisation will also be carried out at the household level. (Details regarding our randomisation strategies are outlined in Analytic Aim 2 and 3 and in online supplementary appendix 1). We will then measure changes in behavioural and attitudinal outcomes for individuals and communities including both those who got the intervention and those who did not. Our results will allow us to assess how the difference in adoption of interventions, at both the individual and community levels, varies across the various arms of the trial.

Specifically, we will address:

1. Can the structure of social networks provide clues regarding subpopulations at higher risk of experiencing RMNCH morbidity and mortality or at higher risk of being unresponsive to behaviour-change interventions?
2. Based on network parameters, can we exploit mathematical algorithms to identify a well-positioned set of people who exert the maximum influence on adoption of improved RMNCH care practices in the larger population, and thus have a multiplicative effect on coverage?
3. Can we, in parallel, identify sets of people who are responsive to such influence (ie, influenceable and not just influential, people)?
4. Is there a threshold effect such that when a certain proportion of the population has changed their norms around key behaviours, the rest of the population is likely to follow?

A key feature of this research is that the network data collection we are proposing is sociocentric rather than egocentric. As illustrated in figure 3, egocentric data, while it involves social network information, is collected from a sample of individuals within a given population.[39 40] In contrast, sociocentric network data creates an image of a collective whole, with comprehensive data gathered on ties

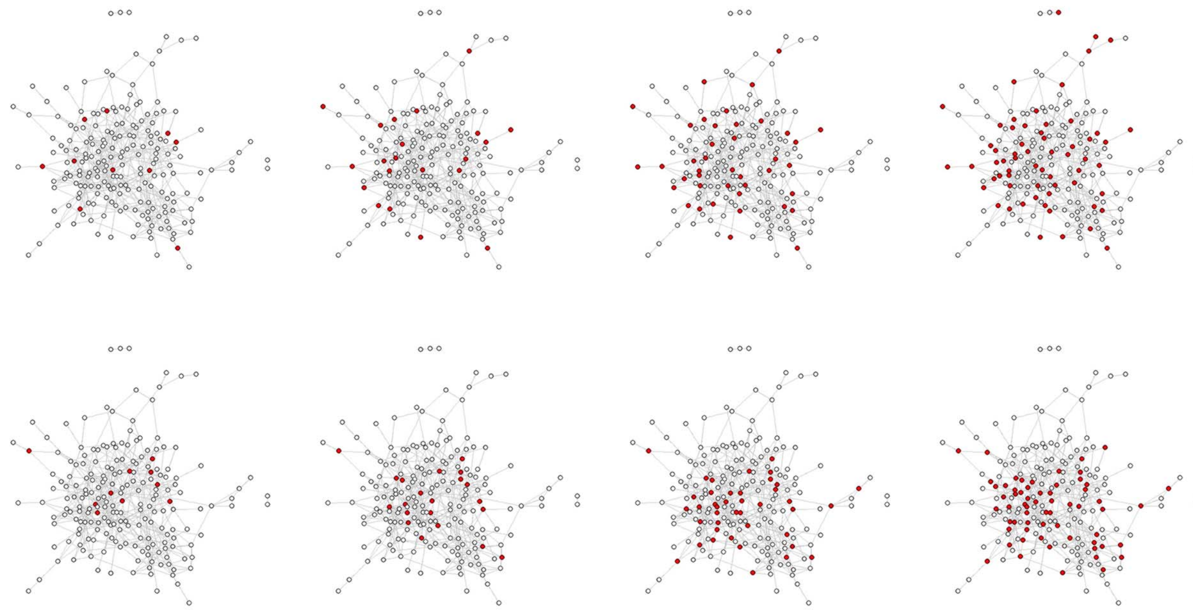

Figure 2 This figure displays a network map of a real village with 206 inhabitants in Honduras. The top row displays individuals selected at random (in red); the bottom row displays individuals selected by the 'friendship nomination' technique (they are a single randomly chosen friend of the randomly chosen individuals) (also in red). The columns display 5, 10, 20 and 30% targeting from left to right. It is apparent that (1) at the same percentage, friends of randomly chosen individuals are more central in the network and have higher degree than the random individuals, and (2) that, as the sampling fraction rises, the difference between the random nodes and the friends nodes declines (as is expected given network theory).

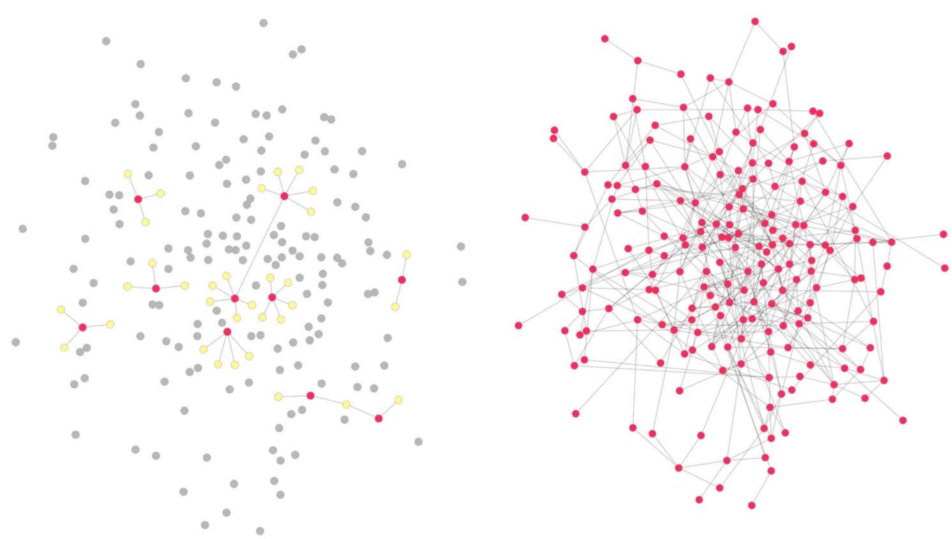

Figure 3 Illustration of network sampling. The left panel shows a network obtained through egocentric sampling. An egocentric sample consists of a set of sampled 'egos' shown as red nodes (the individuals whose characteristics are being studied) and a set of 'alters' shown as yellow nodes (the individuals who were nominated by the egos). Only ego-alter ties and some (typically very small number, if any) ego–ego ties are observed in an egocentric study, leaving all alter–alter ties outside the sample (excluded nodes shown in grey). In contrast, a sociocentric study design, such as the one proposed here and shown on the right, enables observing all existing ties among the sampled set of nodes.

between all of the people within a specified population.[39 40] Whereas egocentric data may help to improve the representativeness of a sample for a large population, sociocentric data allows measurement of larger network structures (like network communities) and individual-level network measures based on them.

METHODS/STUDY DESIGN

Preliminary work

As preparation for this project, we completed geographical mapping of over 200 villages in the study region, allowing us to gain a more precise understanding of the study population and field conditions, including terrain, rainfall and distances to health facilities, to inform planning and implementation. We developed an extensive survey instrument to capture the various outcomes that will be addressed through the household-level interventions that will be implemented by IDB/SMI, including use of folic acid, prenatal care utilisation, birth plan preparation, immediate breast feeding after birth, exclusive breast feeding for infants up to 6 months old, proper thermal and cord care for newborns, proper treatment of diarrhoea in children and paternal involvement in childcare. Our survey instrumentation is primarily composed of validated scales used widely to measure items related to RMNCH outcomes. We conducted an extensive review of the RMNCH literature and consulted global RMNCH experts for their advice on the inclusion of appropriate items in the survey. We also did extensive formative research, including detailed qualitative individual interviews and focus groups, and cognitive interviewing to assess our survey's cultural relevance and consider regional language variations specific to the study area. In addition, we conducted three rounds of pilot data collection involving network mapping and sociobehavioural interviews to test the network questions: our newly designed social networks data collection software, (named 'Trellis'; see online supplementary appendix 2): and the behavioural health outcomes instrumentation (for more details on our pilot work and field operations please see online supplementary appendix 1).

Study population

Our study is being conducted in the department of Copan, Honduras, in an area of over 200 square miles of rugged mountainous terrain with an estimated total population of 92 000 people. In order to power the 8×2 design (for details please see Aim 2), we have selected 176 villages from the 238 small towns and villages located in this area. Factors like population size, accessibility and safety were considered when selecting the final list of villages. Owing to high adolescent birth rates in this population, all individuals over the age of 12 who live or work in the study villages are eligible to enrol (see online supplementary appendix 1 for more detailed information). Individuals who are cognitively impaired and unable to provide consent are excluded. We have already conducted a photographic census in the 176 study villages. Recruitment rates are high: census data show ~32 500 eligible individuals in these villages, of whom at least 93% (N=30 460) have agreed to enrol in our study. Using the Trellis software (please see online supplementary appendix 2), we obtained demographic data, photographs and Global Positioning System coordinates of all participants who enrolled. The average age for participants is 33, and most are married or in a civil union (59%), and slightly over half (54%) are women. The total number of respondents surveyed per village range between 55 and 620 individuals and the average household size is 2.8. We have not yet begun the intervention.

Network data collection

As the photographic census is complete, we will next use bespoke software we have prepared and made publicly available (http://humannaturelab.net/resources/software/trellis/), named Trellis (see online supplementary appendix 2), to undertake the main survey, which includes a battery of 'name generator' questions to capture social relationships. The name generators will focus on several different types of affiliations including friendship, professional contacts, kinship, and contacts relevant to health behaviour. In this study, the boundaries of each network will be the village, so that individuals may nominate any individual from within their own village as a social contact. The photographs taken in the preliminary census will be used to validate the social contacts named by the respondents. Online supplementary appendix 1 provides additional technical details about social network measurement.

Measurements

1. Baseline social networks: The name generator questions to collect sociocentric data will be included in the baseline survey. From these measures, we will calculate community-level and individual-level measures of network connectivity including various measures of centrality (please see online supplementary appendix for more details). (Wave 1 2016)
2. Baseline RMNCH care behaviour and norms for all individuals in the villages. Specific maternal, neonatal and child health behaviour questions will be asked only of individuals who have already had a child. Norms and attitudes questions will be asked of everyone and will include attitudes towards RMNCH behaviours as well as the role of fathers in prenatal and neonatal health. (Wave 1 2016)
3. Concurrent norms and behaviour surveys: 1 year into the intervention, we will administer surveys to track changes in norms and behaviours as well as possible sources of intervention spread. For this survey, we will also monitor the implementation of the intervention by asking questions specific to receiving intervention activities. (Wave 2 2017)
4. Final outcome survey: a second round of social networks data collection, RMNCH care behaviour and norms questions for all individuals in the villages. Using this second round of social network data, we will recalculate network connectivity at the individual and community levels. (Wave 3 2018)
5. Contextual surveys: We will gather data on contextual factors within each village, such as the size, distance and characteristics of the nearest clinic, other intervention activities that may be taking place through local organisations or non-governmental organisations (NGOs), important geographical features, facilities and infrastructure available to residents, etc.
6. Health outcomes: For a subset of families, our surveyors will specifically measure and record temperature and respiratory rates for children under 5 years.

7. Clinic records: We will collect clinical data from birth records from the regional maternity clinic for women who give birth at this facility. We will also collect postpartum health data from the local health centres. Postpartum data is recorded by family health teams conducting home visits with postpartum women in the villages within 7 days of delivery, regardless of birth location. Postpartum health records contain information on newborn weight, respiratory rate, temperature, head circumference and signs of redness, pus or swelling in the umbilical cord,[41] as well as body temperature, blood pressure readings and presence of danger signs for the mothers.

Behavioural health intervention

The implementing partner in this project, IDB through SMI, is responsible for designing and implementing an integral intervention to promote RMNCH behaviour change in Honduras. To work within the constraints of this study, the intervention had to meet specific requirements including: (1) alignment with priorities of the Ministry of Health (MOH) of Honduras, the Bill and Melinda Gates Foundation, and the needs of the population; (2) contain new messages for the targeted population to allow for detection of changes in knowledge, attitudes and practices; (3) include tracers or identifiers which could be detected during follow-up surveys; (4) not use mass-media communication techniques, including radio spots, flyers, posters, etc, as these would contaminate the network effects of the study; (5) have a strong monitoring component; and (6) have a demonstrated effectiveness in similar settings in order to test the spread of behavior from person to person. The intervention would also have to adopt the targeting strategy, focusing on network position, as opposed to targeting households with primary audiences for the behaviour change of interest. For example, the targeting algorithm could hypothetically identify a household with two grandfathers and therefore select it to receive the intervention. This household would usually not be selected for an intervention on prenatal care or neonatal practices, given that there are no pregnant women living there but we would include it.

Given these requirements, an educational package, using the Timed and Targeted Counselling (ttC) methodology[42] complemented with alternative methods of face-to-face communication including songs, rhymes and riddles, was designed with World Vision and Child Fund Honduras. The social and behaviour change communication strategy for the intervention was designed using the 'P-Process', a tool developed by Johns Hopkins Center for Communication Programmes and used for more than 30 years for planning strategic, evidence-based health-communication programmes.[43] This intervention will be delivered by trained community health workers for 24 months on a monthly basis to the households selected for the study. Timed and Targeted Counselling (ttC), has been implemented in 20 countries worldwide by World Vision.[42] It is targeted in time (when it is needed), in space (by visiting in the home), and in individualised approaches (messages and strategies to remove barriers depending on the circumstances of a specific family). This methodology uses narrative and negotiation in a 1–2 hour visit with families to discuss positive and negative scenarios and create a list of agreements with families to try out new practices. It should be noted that ttC is normally implemented in households with pregnant women and/or children under 2 years, and counselling is provided to all family members based on stage of the pregnancy or age of the child.

This methodology was adjusted to include messages for topics of interest to the study based on findings from formative research conducted for intervention design (please see online supplementary appendix 3 for details) and evidence-based, cost-effective practices[2 44] related to study outcomes.

Study outcomes include: (1) use of folic acid in women of reproductive age to prevent birth defects; (2) receiving prenatal care in the first trimester; (3) having a birth preparation plan for seeking timely prenatal care, institutional birth, postpartum care and emergencies; (4) exclusive breast feeding for infants under 6 months; (5) immediate breast feeding after birth; (6) proper thermal and cord care for newborn infants; (7) proper treatment of diarrhoea in children, including the use of zinc (which is a new component of the SMI programme, here); (8) paternal involvement in childcare, particularly for newborns; (9) use of modern family-planning methods; and (10) delaying pregnancy until 18 years of age.

While only very specific households will be targeted according to our randomisation methods, participants from target households will not be discouraged from inviting others, and a careful record of attendees will be kept. CHWs have a team of supervisors for intervention quality control and to ensure that CHWs are visiting the correct houses. The IDB/SMI team has incorporated the use of mHealth tools to aid in intervention delivery (eg, all stories are available in an animated video format), data quality and timeliness. The IDB is also currently working on supply-side interventions with the Government of Honduras through SMI and other complementary programmes, to ensure that community members seeking services receive quality care.

As part of the intervention protocol, we will keep careful track of which CHWs have visited which households in order to allow us to evaluate possible provider effects relevant to the study outcomes. For more details on intervention implementation please see online supplementary appendix 3

Figure 4 illustrates the sequence of events in our project.

Analytic aims

We have several analytic aims, as this project is analytically very complex (for details on statistical methods

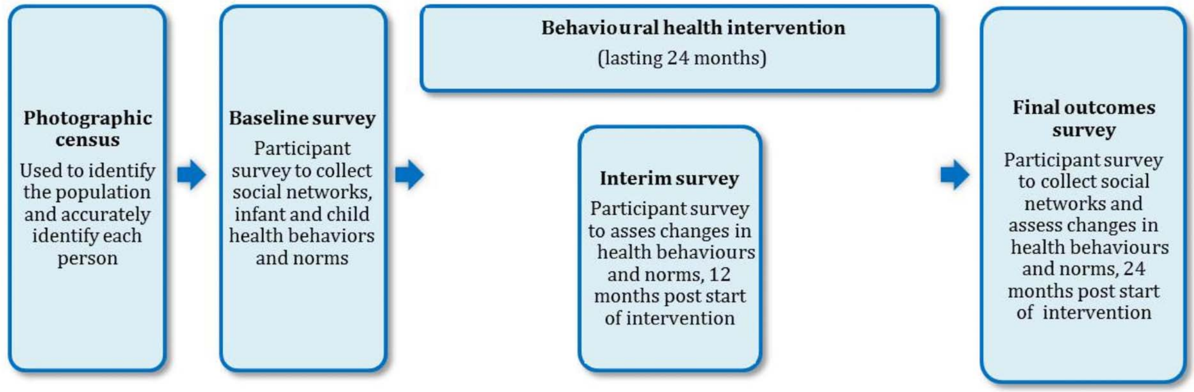

Figure 4 The sequence of events in this study.

specific to each aim please see online supplementary appendix 1). The extensive amount of network data will allow us to undertake an unprecedented level of analysis regarding the relationship between community-level social dynamics and the uptake, diffusion and maintenance of intervention behaviours, and norms. These network data will be used to examine how attitudes and behaviours spread across network ties.

Analytic Aim 1: evaluate the extent to which behaviour change regarding RMNCH care spreads

We will use the results of the randomised controlled trial (RCT) to ascertain the extent to which beneficial or harmful RMNCH care attitudes and behaviour change in one person can potentially modify the attitudes and behaviour in other people to whom they are connected. With the longitudinal data that we will collect in this study, we will be able to track the change in attitudes and behaviour among individuals over time. By examining the correlation between the behaviour of an individual and the behaviour of those to whom she is linked, we will be able to determine to what degree the intervention effects spread beyond those who were directly exposed to those who were not exposed (a 'cascade' or 'spillover' effect).

Here, our work benefits from experimentally exposing individuals to the intervention. This randomisation will allow us to assess causal relationships between connected individuals by measuring how participants' outcomes are affected by their social contacts' (randomly assigned) exposure. Across villages, we will assess how varying the overall treatment rate causes both treated and untreated individuals to alter their behaviour.

The magnitude, and possibly even the direction, of network effects should vary according to the social distance between the actors. In general, our previous research has demonstrated weaker effects with increasing social distance, so that, for example, a behaviour change (eg, performing hygienic cord care) in a social contact would have progressively weaker effects in terms of motivating behaviour change as one moves along, say, the continuum from sibling to friend to neighbour. Among socially connected individuals, we will be able to distinguish among mutual relationships (when both nominate each other) and 'one-way' relationships (when nominations are not reciprocated). We expect that social contagion will be more likely between mutual relationships compared to one-way relationships.

Analytic Aim 2: test for social effects in community adoption rates

Most interventions are focused on individuals. Researchers identify a group of individuals to enrol in a project, randomly assign some to the intervention and some to control, and then test the efficacy of the intervention on treatment individuals versus control. Individuals may be chosen from one defined community or from many. The impact of the intervention on the community as a whole is usually not measured or taken into consideration. However, we know that, for behaviours with a social component, the individual is not thinking or acting in a vacuum. As more and more individuals in a community are exposed to new behaviours and new ideas, it is increasingly likely that any given individual in that community will adopt those behaviours and ideas (see figure 5). There is a threshold to this effect, however, beyond which exposing additional individuals will not increase overall adoption. Analytic aim 2 is to learn where this critical threshold lies in order to capitalise on the social effects that create it. It may be possible to achieve near-maximum adoption with substantially less effort than is currently being exerted by interventions.

We plan to test for this threshold by using an 8×2 RCT factorial design. In the first dimension of this design, we will assign villages to one of eight dosage treatments (0%, 5%, 10%, 20%, 30% 50%, 75%, 100%) indicating the percentage of households in each village that will be randomly chosen to receive the intervention package (for details on randomisation methods see online supplementary appendix 1). We will use these randomly assigned treatment percentages and observed rates of health behaviour adoption (in the entire communities) to test for this threshold. What is the minimum proportion of the population to target in order to achieve

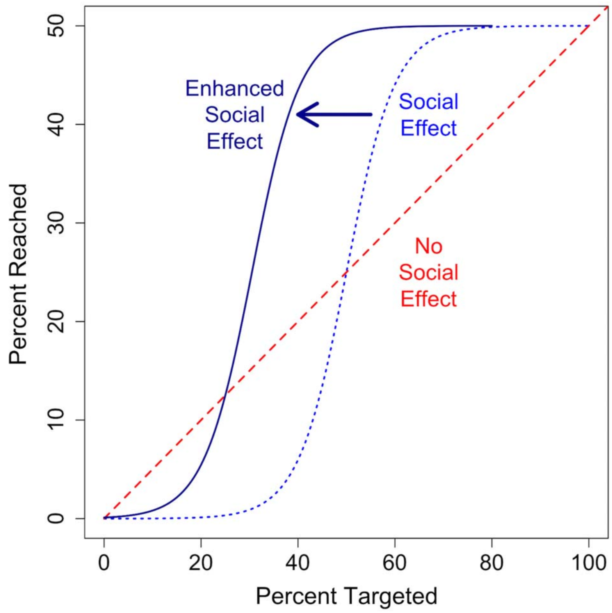

Figure 5 This figure illustrates possible results. The X-axis denotes the fraction of a village targeted for an intervention, and the Y-axis denotes the fraction that ultimately adopts the intervention. The red dashed line denotes the results for no social effect. Each person targeted has an equal chance of adopting regardless of the number of others treated. The blue dotted line denotes the results for a social effect. If we target people at random for an intervention, many of them may be reluctant to change their behaviour when few others have, so that the intervention is less effective per-person until a critical threshold is reached. At that point, adoption is more likely because of social reinforcement and the per-person effect of each targeted individual grows exponentially. Eventually, so many people have adopted that there is no willing person left to adopt and the per-person effect decreases once again, approaching 0. Understanding this dynamic is important, since even high targeting percentages that fall below the critical threshold might yield low adoption. Similarly, if there is a social reinforcement effect, it may not be necessary to target everyone. In the example above, targeting 60% of the individuals capture nearly 100% of the total possible intervention benefit. Finally, the dark blue solid line denotes results when we enhance the social effect through friendship targeting. If targeted people are well-connected, there will be greater exposure to the intervention through diffusion, shifting the whole S-shaped curve to the left. It takes fewer people to reach the critical threshold, and is possible to reach saturation with a smaller percentage targeted. Note that the Y axis denotes 0–50% and the X axis includes the full 0–100% as we assume there will be an upper limit on adoption associated with any particular intervention (here, we arbitrarily chose 50%).

maximum levels of adoption at the community level? Since we have several outcomes on which we are intervening, we will be able to test to what degree this threshold differs (or does not differ) across behaviours, and how this may differ according to the behaviour-change mechanism we observe (with due attention to multiple comparisons).

The two mechanisms by which we believe social networks affect intervention adoption are through the spread of information and the spread of norms.[21] On the one hand, the spread of information can be understood as being a process of social learning[45] where people mostly observe others adopting the behaviour and assess the benefits that the early adopters may be enjoying. On the other hand, the spread of norms is more likely to occur as the result of direct social influence.[45] If the innovation is in direct opposition to an ingrained norm, community members may sanction others for adopting the new behaviour. The proportion of the population that must be exposed in order to overcome that resistance might be higher than it is for a diffusion of technology or knowledge. Once a critical mass adopts, however, and a new norm has taken hold, then we would expect that social influence would switch and work strongly in support of the innovation. In figure 5, at the higher levels of adoption, there is strong upward influence pushing the community in support of the behaviour of interest.

Diffusion and norms processes will both yield the S-shaped curve shown in figure 5, though potentially occurring at differing rates. Elucidating the difference between the two is crucial; however, the intervention strategies necessary to elicit successful change will differ between the two. To further distinguish these two mechanisms, we plan to generate questions in our survey that will help us to differentiate whether a health behaviour showing a social effect is influenced more by information transmission or through social influence and social sanctions. We will also analyse social structural features that affect this (see online supplementary appendix 1).

Analytic Aim 3: test the impact of the ‘nomination’ network targeting method

If people who are initially selected to receive the intervention are socially well connected, it follows that, because their local social networks are larger (and for other topological reasons as well), the number of people who will be exposed to the intervention at an early stage, through spill over, will also be larger. Moreover, if we identify these central people through the most topically salient relationships, they might also be people with high credibility around our behaviours of interest. In other words, we anticipate that these individuals will not only have more influence in the community due to their greater level of access to others, but that the probability that any individual within their network adopts might be greater due to their higher level of general credibility.[46] Theoretical network research supports both these hypotheses,[47 48] but there is little empirical evidence to test these claims.[30]

Hence, a unique dimension of this study is that we will vary how initial targets are selected. In our 8×2 factorial design, the ‘8 conditions’ differ according to proportion of the community targeted (see Aim 2). The 'two conditions' differ according to how households within that community are chosen. In one condition, individual households are selected at random; in the other condition, households are selected using the 'friendship nomination' procedure that will identify individuals who are more connected in the village.[49 50] In the friendship nomination condition, we will randomly select individuals and then, rather than intervene on those randomly selected individuals and their households, we will intervene on the household of one of their nominated social contacts. Past work suggests that the adoption curve under nomination targeting is shifted to the left relative to random targeting because friends are more central in the network and therefore spread information and norms more quickly in the early part of the intervention.[30] This shifts the whole adoption curve to the left, as shown in figure 5 (the 'Enhanced Social Effect' shown by the solid dark blue line), and, as a result, the total percentage of individuals that need treatment to achieve nearly-maximum adoption also shifts to the left. The consequence of this left-shift is that intervention adoption increases for the same number of initial targets. This increased efficiency through the social effect is a central and overarching study objective.

Analytic Aim 4: evaluate the impact of network structural characteristics on behaviour change

We will ascertain how network structure (at the individual level and at the village level) moderates the impact of the intervention. Do people in certain parts of the network, as measured by network parameters—including centrality, transitivity and clique membership (see online supplementary appendix 1 for more details on network structural measures)—respond differentially to intervention? How does a person's location in a network, above and beyond their personal attributes, affect their response? We will also use this information to identify as-of-yet undiscovered network structural-risk factors for RMNCH morbidity and mortality. This may allow us to make important recommendations for targeting specific parts of the network in future interventions.

Since we are collecting sociocentric data (see figure 2), we will have the ability to measure a wide range of structural features to determine whether they moderate the effect of the RCT intervention. For example, is the randomly assigned intervention more effective for people in the centre of the network than it is for those in the periphery? Is the intervention more effective for those in denser parts of the network with more cross-cutting ties or for those in sparser, less-connected parts of the network? With respect to community structure, do large or small clusters in the network experience better outcomes?

Results of these analyses will include: (1) network measures for all individuals within the network and higher-order features like clusters and communities for the overall network itself; (2) information regarding the relationship between network measures and outcomes of interest, including any possibly moderating effects; and (3) recommendations for using network measures, including network position and network subgroups, for future interventions.

Analytic Aim 5: ascertain whether partial collection of network data can identify the most influential or the most influenceable people in the network

Ultimately, a key goal of this RCT is to develop means by which network approaches can be used to design, implement, and monitor health interventions most effectively. We intend to develop means by which network strategies can be incorporated into health behaviour programmes without having to map the entire network. While understanding the interaction between social relationships and intervention uptake is undoubtedly of great theoretical interest, ultimately this work is about the application of network tools to real-world projects. In real-world scenarios, the mapping of entire networks is likely to be expensive and time-consuming. A successful demonstration of the friendship nomination technique for identifying influential individuals within networks (which can be implemented without mapping the whole group) will help address this.

To further address the issue of scalability, one of our analytic aims is to use the full network data in this project to develop means by which interventionists can collect partial network data to achieve the same outcome, but at much lower cost. Recent research has suggested that, properly implemented, random network sampling can be used as a proxy for complete network information.[51] For example, in our previous work, we showed that it was possible to predict an outbreak of H1N1 influenza in a population 6 weeks in advance, even without measuring the full social network.[49] While health behaviour interventions probably spread differently than infectious diseases, the rationale behind using a network-based approach to disseminate an intervention, particularly one relying on central actors, has been proposed in a wide range of fields. Indeed, network-based approaches to health behaviour interventions using 'opinion leaders' are increasingly common in the developed world.[47 52 53]

Analytic Aim 6: assess the effect of the intervention on the village social networks

We will ascertain whether and how the interventions may actually modify the actual network structures of the villages. Does the introduction of a health intervention to a village change social interactions? And what are the possible effects of these changed structures on the health of communities or the population?

While programme monitoring and evaluation typically focus on the intended objectives of a given programme, interventions can have unanticipated outcomes which are rarely measured or published in the peer-reviewed literature.[54–57] Nevertheless, it is critical that interventionists consider unintended outcomes, whether positive or negative, in order to integrate these outcomes into cost-benefit analyses for programme planning.[58]

One of the most potentially profound, but largely overlooked, peripheral outcomes of health promotion interventions is a fundamental change in social structure as a result of the intervention activities. Previous network research has demonstrated that social marginalisation can occur among people who violate the norms of their proximal networks.[59 60] As norms change due to the effects of well-intentioned interventions, the social landscape of the population in question can change as well. For example, an intervention designed to increase academic performance among air force academy students failed because the intervention unintentionally created segregated clusters of very high and very low ability students, which reinforced the academic challenges of those same very low ability students that the intervention was designed to assist.[61] Our past research (in the USA) has shown that social isolation of smokers increases the likelihood that they will cluster together, which further reinforces their smoking behaviour.[62]

It is possible that those embracing the novel behaviours in our RCT may form new connections based on their mutual interest in the intervention and, as a result, help create clusters of 'adopters' within the network. When adopters create clusters, non-adopters may be left to form their own clusters. Examining to what degree this occurs can inform future interventions and ameliorate any possible unintended health disparities that might result from an otherwise successful intervention. The intervention may also strengthen the social health of the community, yielding unintended but beneficial effects on a wide range of health outcomes other than RMNCH-related outcomes.

To the best of our knowledge, this will be the first large-scale evaluation of how an RCT in a developing world setting possibly modifies social interactions and what impact this modification has on health. We will assess changes in egocentric and sociocentric network structures from wave 1 to wave 3 (when the network is mapped for the second time, roughly 2 years later), and compare those changes across the different arms of the trial, examining whether there is a significant difference in the evolution of the networks for villages exposed to the intervention versus those that are not exposed. Specifically we will be looking for: (1) the emergence of clusters of intervention adopters, (2) the emergence of clusters of intervention non-adopters and (3) large-scale, and ego-centric, changes in the overall network (such as increased density).

DISCUSSION

Our study is unusual in that it will allow us to both (1) understand the way in which social network dynamics affect the uptake of a large scale RMNCH intervention, and (2) evaluate how network-based targeting can maximise the overall impact of the same intervention. We will use a detailed understanding of social network structure and function to identify novel ways of targeting influential individuals so as to foster behavioural cascades and population-level behaviour change.

We will achieve this objective by conducting a randomised controlled trial of network targeting algorithms, to be deployed in a sample of 176 villages in Honduras, with a 2-year package of monthly RMNCH care interventions. Our work will involve both experimental as well as observational analyses, and it will be one of the largest efforts to map face-to-face networks of which we are aware (involving over 30 000 people). We will test a scalable method of network targeting that would not, in the future, require the actual mapping of social networks, but that would still offer the prospect of rapidly identifying influential targets for public health interventions. If successful, we will have developed procedures that will allow us, and others, to accelerate the change of attitudes and behaviours in entire populations much more efficiently and comprehensively.

Limitations

Our study has limitations. Our sample is limited to rural Honduras and so, while many network characteristics tend to be similar across different social and cultural contexts,[63] some of the norms surrounding RMNCH care might be special. This may limit the application of some of our results within other contexts. Also, while we will use objective measures whenever possible, many of the outcomes we will be assessing will be measured using self-report. Finally, while between-village ties would strengthen our understanding of network dynamics within this population, the extensive resources involved in that level of data collection preclude our ability to collect that data (though we forecast such ties to be less relevant and less numerous).

Our intervention itself also has limitations. In the ideal scenario, we would use an evidence-based intervention for community-based neonatal health tested in rural Honduras implemented to 'guarantee' behaviour change among the initially targeted individuals, the ripples of which would be studied in the RTC. However, given the constraints of the study, this 'ideal intervention' does not exist. For example, in the ideal behavior change communication intervention, the person or group whose behaviour change is sought receives the intervention messages as many times as possible. In the typical community-based intervention, in addition to face-to-face counselling, community members would be exposed to radio messages, banners, flyers, mass text messages and other media-based communication methods to reinforce messaging. Given that the study relies on information passing through the social network, mass media communication are being excluded. The intervention team has been creative in adding tools to the intervention, and has been mindful of including a variety of behavioural changes along the continuum of pregnancy, childbirth, postnatal care and child health in order to maximise the possibility that one or more intervention components are adopted. Intervention targeting is also affected. Normally in an intervention targeted at changing behaviours in maternal and neonatal health, households would be selected depending on where pregnant women live. We could not take these criteria into consideration, given that the targeting mechanism is based on position in the network, rather than whether or not a woman in the household is pregnant.

Finally, not all aspects of the desired behaviour change rely on adequate knowledge, attitudes, and practices at the household level. A clear example is how the conditions in health centres and hospitals affect behaviours of the population. Although messaging is provided to families regarding the importance of male involvement in birth, if the hospital or health centres do not have adequate infrastructure to have private birthing rooms (the norm in Central America), men cannot be in the room during the birth if another women is also in labour. Although SMI works closely with the MOH to improve supply-side conditions, some aspects are out of the scope of the programme. The study team is aware of these limitations and is documenting them to have a clear picture of these external factors which also impact the success of the community-based behaviour change intervention.

CONCLUSION

This study is unique in its scale of network data collected in a developing world setting, the integration of complete network data with the results of a large RCT, and, most importantly perhaps, the testing of network algorithms as a method of boosting uptake of the intervention. With billions of dollars spent each year in attempts to achieve behaviour change in at-risk populations, we are still unsure of the best strategies for implementing these interventions in ways that maximise sustained change. This study will be an important contribution towards that goal, with results that can be applied in disparate global contexts.

We believe that the knowledge gained will have substantial practical application. Global health practitioners are beginning to understand that ignoring social reinforcements and expectations in relation to the outcomes of interest can herald less -than- optimal results. It is not clear however, how to apply that understanding to the practicalities of intervention work. By looking at behavioural and attitudinal outcomes of our intervention, we have the opportunity to take a real-world, on-the-ground intervention and analyse it in conjunction with gold-standard, complete social network data. By exploiting network insights, we will be able to develop strategies for interventionists to implement in order to shift the norms of the communities towards acceptance and uptake of the desired behaviours.

Dissemination

The primary applied aim of this project is to develop methods by which interventionists in global health settings can integrate a network approach in order to maximise the effectiveness of their programmes. The analytic aims we have outlined above are essential for achieving this goal. Analysis and publication in peer-reviewed journals alone, however, will not make this body of knowledge accessible to those for whom it is intended, namely, global health practitioners who are working in communities to achieve sustainable behaviour change. To be of more practical assistance for future implementation, we intend to engage NGOs and other actors through global health conferences and other venues for exchanging practical methods for improving health interventions.

These dissemination efforts will be more thoroughly planned during the first year of the project and will ultimately result in the development of a 'toolkit' for practitioners to use in network-based intervention efforts. This toolkit will provide guidelines for collecting network data; develop open-source software to collect network data and identify network targets in field settings; and develop network data-collection materials suited to project goals and social context.

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Contributors HS, DS, AH, TK, JF and NC participated in the conception of the idea for the study and wrote the grant proposal. JN and EI wrote the intervention grant proposal. JN, EI and MF managed the design, implementation and supervision of the intervention. RN, NC, MM, JF, DS and LN helped with software development and data management. TK, RN, DS and JB managed field operations. HS, TK, RN, DS, JB and NC were responsible for survey development. HS, TK, RN, JN, NC and EI worked on the development of the RMNCH intervention. HS, DS, NC, JF, JN, AH, RN, EA and TK contributed to the discussion, writing and review of the manuscript, and HS coordinated this. All authors were responsible for the study design and provided professional, logistical or statistical support. All authors approved the final version of the manuscript.

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Supplementary Appendix 1 for Exploiting Social Influence to Magnify Population-Level Behavior Change in Maternal and Child Health: Study Protocol for a Randomized Control Trial of Network Targeting Algorithms in Rural Honduras

Distribution of village populations and expected eligibility:

Our initial census estimates show approximately 32,500 people within the study population of 176 villages. Of these, approximately 11,495 (or 38%) are women between the ages of 15-49, 8% of whom (~920) may have a birth event during the period of our intervention (based upon age specific fertility rates available for the Copan region of Honduras on the Demographic and Health Survey Statcompiler). Final intervention enrollment numbers will depend on exact sampling strata.

Social network measures:

Social network analysis can provide several distinct measures that together allow researchers a comprehensive vantage point from which to study health behavior. The building blocks of social networks are individuals and their connections ("nodes" and "edges" or "ties"). In analysis, individuals under study are termed "egos" and their identified social connections are termed "alters" (see SA Figure 1, panel A).

Network level measures

In this study, we will be collecting sociocentric network data, and so we will be able to examine structural characteristics of entire villages and the individuals within them. For example, we can identify natural communities in the network ("cliques") that comprise groups of people who are connected together through relatively strong groupings of ties (e.g., SA Figure 1, panel D). We may find that certain clusters of women localized within particular regions of the network resist or adopt intervention uptake. We can also measure network *density*, which is the proportion of all possible two-person connections that are actually present. We may find that intervention adoption is more easily facilitated in higher density villages. Analysis of structural network characteristics like these will provide us with powerful tools for future interventions by allowing us to (1) predict what areas of the network will be most resistant or amenable to change, (2) leverage existing resources to target the population most effectively using network characteristics, and (3) understand possible changes in network structure that can either hinder or support intervention diffusion.

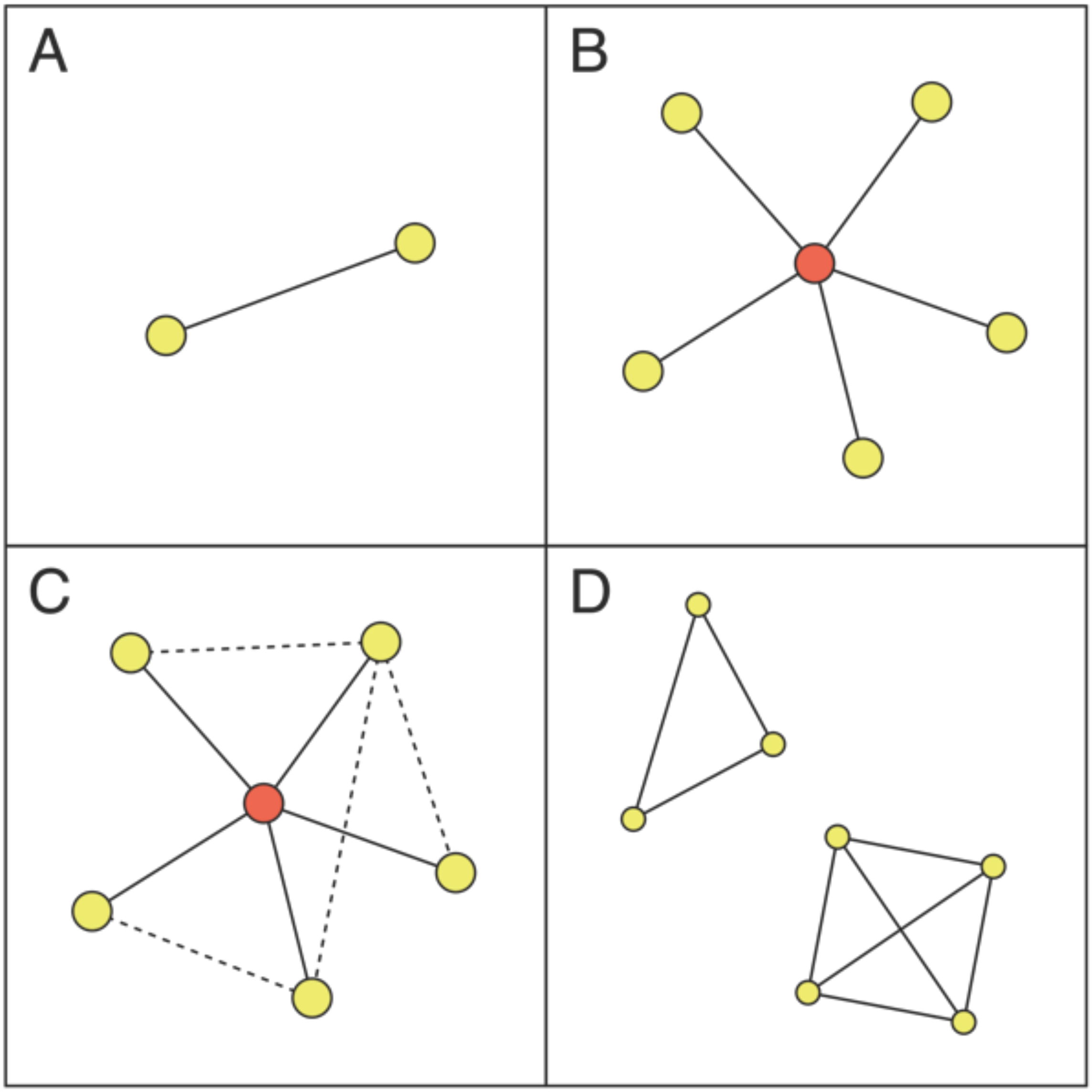
Individual-level measures

In addition to network-wide metrics, we will also examine individual network characteristics. The most frequently measured individual-level network metric is centrality, which is relatively intuitive in concept and is a measure of how "central" an individual is within any given network. In these analyses, we will be able to investigate to what degree individual network centrality can affect the adoption of the intervention components by any individual within the population. For example: (1) Do participants with higher centrality adopt the new intervention more readily than those with lower centrality? (2) Do individuals connected to high centrality social contacts adopt the new intervention more readily? (3) Are socially isolated individuals less likely to adopt the new intervention? (4) Is the relationship between intervention adoption and education moderated by network characteristics (e.g., will educated women who are less socially connected adopt the intervention less readily than educated women who are well connected)?

Centrality can be operationalized in a number of ways. Each operationalization holds the similar feature that the score assigned to the individual increases when the individual is more central under the particular objective function. For example, one centrality measure aims to identify individuals who are key "hubs" in the network through which information must travel to get from place to place; another centrality measure aims to identify individuals who have a large number of well-connected friends[1]. Although distinct centrality measures are built to identify distinct features of the position of the individual, in practice, each of the measures are correlated with one another.

Several centrality measures have been shown to perform well in identifying important individuals in social[2] and epidemiological networks[3]. The basis of most centrality measures is "degree", or the total number of ties that an individual holds. *Degree centrality*[4] then, is simply the total number of unique social connections that nominate or are nominated by the subject (SA Figure 1, panel B). Degree is often referred to as a first-order metric, since it simply counts the number of ties connected to each individual; degree has the desirable property of being rank-unbiased in a sampled network[5]; however, it degree does not provide any information about an individuals' more general location in the network. Further refinement of this measure might generate scores based only on social nominations received (*in-degree centrality*) or sent (*out-degree centrality*).

Other measures of centrality look beyond direct ties to provide a fuller measure of where an individual ties within in the larger network. It is possible to measure the social distance between any pair of individuals in the network by defining one's friends to be at distance 1, the friends of one's friends at distance 2, and so on. *Closeness centrality*[6] is the inverse of the average distance between a respondent and all other people in the network[7]. A third measure, *betweenness centrality*[8], identifies the extent to which an individual in the network is critical for passing support from one individual to another while a fourth measure, *eigenvector centrality*[9], assumes that the centrality of a given individual is an increasing function of the centralities of all the individuals that support her.



SA Figure 1: *A schematic illustrating some fundamental concepts about social networks. (A) Nodes and ties are the elementary building blocks of networks. A tie connecting two nodes indicates a social relationship between the two individuals. (B) The degree of a node (sometimes called "degree centrality") is a metric that quantifies the number of connections (acquaintances, friends, etc.) a given node has. For example, the red node at the center of the figure has a degree of five. (C) The clustering coefficient is a metric that quantifies the extent to which the network neighbors of a given node are directly connected to one another. More specifically, the clustering coefficient of the individual at the center of panel C (red node) is given by the number of ties that exist among his or her friends (the dashed four ties) divided by the number of ties that could exist between them (in this case, 10), yielding a value of 4/10 or 0.4. This number can also be interpreted as the probability that any two randomly chosen network neighbors of an individual are connected. (D) Social networks typically possess meaningful structure beyond the level of nodes and ties. So-called "cliques" are typical examples of such structure, and panel D exemplifies a 3-clique (top) and a 4-clique (bottom), which consist of three and four nodes, respectively, and the realization of all possible ties between them.*

Pilot work

Prior to the rollout of our community census and baseline survey efforts, we conducted 3 separate pilot surveys. The first pilot survey, conducted in the summer of 2014, was given to 1018 individuals in 2 towns in the department of Lempira, Honduras, of whom we were able to collect network, demographic, and social normative measures on 831. The second pilot survey was conducted in February 2015, and covered 4 small villages in the Copan region, and included 165 individuals with questions on networks, demographics, as well as reproductive, maternal, child and neonatal health (RMNCH) knowledge, attitudes, norms, and behaviors. Our final pilot survey was conducted in May 2015, for which we enrolled 577 individuals. After each pilot we carefully analyzed the data for anomalies, including large number of missing responses, no variation in item responses, or responses that seemed irregular in ways suggesting response bias or misunderstanding of questions. The survey was then revised accordingly.

Field operations

We recruited and trained over 100 local surveyors to perform preliminary data collection and infrastructure development, complete participant recruitment and census enumeration, and conduct the survey interviews. We also established 3 field offices in geographically strategic locations to minimize travel time to and from the study villages. All offices are fully equipped with data collection tablets, netbook computers, printers, high speed internet, and a local server to insure secure download, synchronization and transfer of research data. All surveyors were extensively trained in the use of our survey instruments and software by US-based project managers and supervised by Honduran project coordinators with whom we have daily contact in relation to data collection and other implementation activities.

Community engagement

We developed valuable relationships with local government and health officials, participating in staff meetings for health center personnel and community health workers. We presented project plans and obtained approval of our field operations and data collection procedures from the Honduras Ministry of Health (MOH). The MOH has reviewed our study protocols, consents and survey instruments and provided feedback. Our field teams have also met with local community leaders and indigenous council members to present study objectives in all study villages prior to beginning recruitment and data collection.

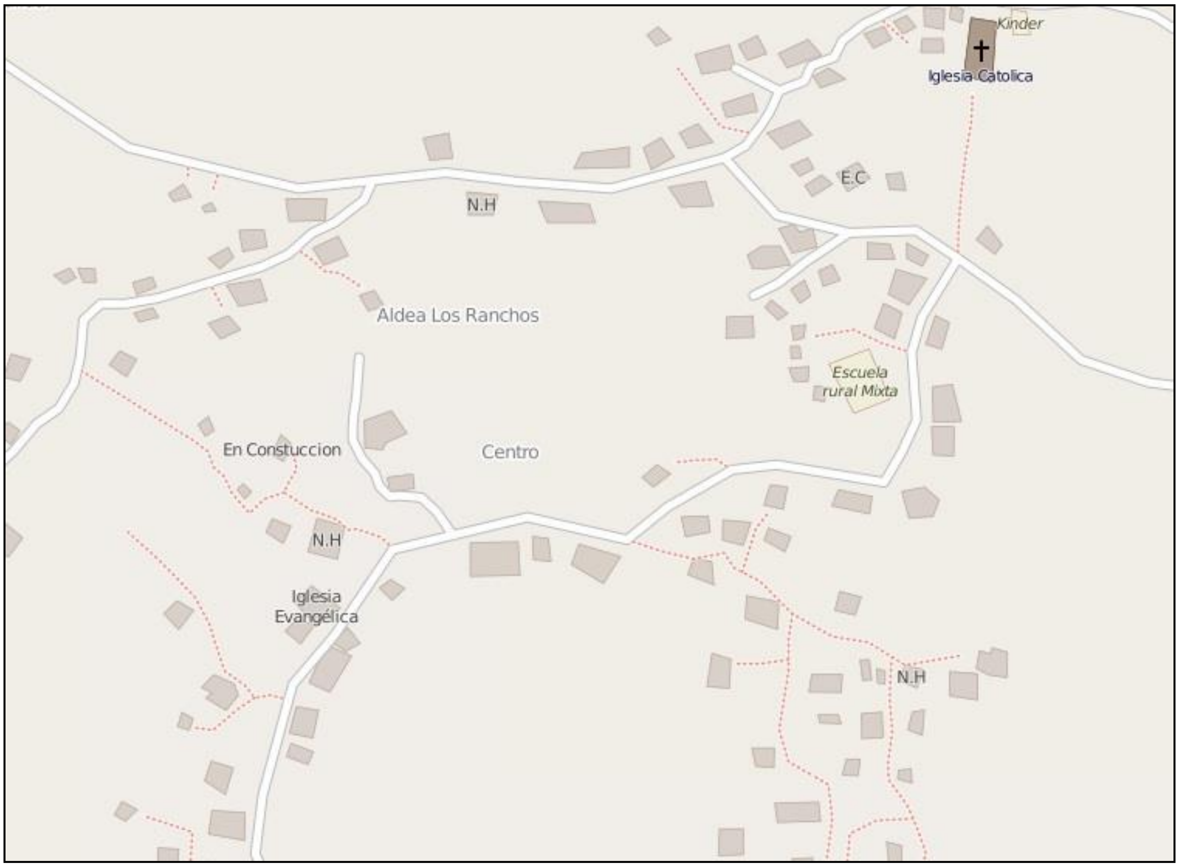
Registration

This trial is registered with ClinicalTrials.gov, number NCT02694679.

Data collection

Geographic Maps

The first stage in data collection is making geographical maps of each village. The maps serve as a means of quality control and help focus the efforts of each survey team. SA Figure 2 shows a map from a Los Ranchos, a village in our pilot study. The village of Los Ranchos is typical of the size and makeup of communities in this study's sample. The mapmakers use satellite imagery to get an idea of the town's layout, after which the team goes into to the village and place every edifice on the layout. Once the map is completed every structure is given a unique code that enumerators will use to when conducting census and surveys



SA Figure 2. *Map of Aldea Los Ranchos, a village in Copan. All dwellings and roads added and confirmed by our study surveyors are now available for public use on www.OpenStreetMaps.org*

Photographic Census

After the map of a town is complete, enumerators will visit each building and ask to photograph each person living in the residence. These pictures will be taken on tablets preloaded with *Trellis*, a tablet-based survey program designed to facilitate network data collection in the field. The pictures and some preliminary demographic information will be saved to the program's database so the pictures can be used in the next stage to increase the accuracy and efficiency of network data collection. We use photo identification primarily because to solve issues of unique identification in the presence of name similarity and high illiteracy. During previous data collections efforts in this area, the research team learned that in one town fifteen women were named Maria Hernandez. And so, when a respondent answers that Maria Hernandez is one of her friends, enumerators would have been stymied without additional information to specifically identify one Maria from the others. Past efforts have measured the additional information by asking qualifying questions about Maria; however this increases the time it takes to gather data while decreasing accuracy. Pictures are faster and more accurate.

Large Survey and Network Data Collection

Upon completing the photographic census, we will undertake the baseline survey in each village. This survey includes the battery of "name generators" that serve to identify the social alters, thereby building each villages' social network[10]. Name generators will cover a broad grouping of relationship types, including affective, kinship, and resource exchange. To uniquely identify the connections between individuals, we will use the photographs collected in the census.

Name generator questions

Does your mother live in this town?

1.) If yes, what is the name of your mother?

Does your father live in this town?

2.) If yes, what is the name of your father?

What are the names of your siblings over the age of 12 that live or work here?

Do you have any children who don't live with you but do live in this village over the age of 12?

3.) What are their names?

Are you married or living in a civil union?

4.) What is the name of your partner?

In the next section, we will ask you some questions about who are the people that you do different things with, and to name those people specifically. You can answer the questions with names of people who are OVER THE AGE OF 12 and WHO CURRENTLY LIVE OR WORK IN THIS VILLAGE ONLY. These people may include any friends, family, people you work with, people that work for you, neighbors, etc. When you answer these questions, you may answer with one person, more than one person, or say there is no one. You are free to include up to 5 people.

5.) Who do you trust to talk to about something personal or private? (including friends, family, people you work with, neighbors etc, who live or work in this village)

6.) With whom do you spend free time? (including friends, family, people you work with, neighbors etc, who live or work in this village)

7.) From whom would you feel comfortable asking to borrow 200 Lempiras if you needed them for the day? (including friends, family, people you work with, neighbors etc, who live or work in this village)

8.) Who do you think would be comfortable asking you to borrow 200 Lempiras for the day? (including friends, family, people you work with, neighbors etc, who live or work in this village)

9.) Who would you ask for advice about health related matters? (including friends, family, people you work with, neighbors etc, who live or work in this village)

10.) Who comes to you for health advice? (including friends, family, people you work with, neighbors etc, who live or work in this village)

11.) Besides your partner, parents or siblings, who do you consider to be your closest friends? (including friends, family, people you work with, neighbors etc, who live or work in this village)

12.) What are the names of the towns leaders?

We have asked questions about the people who are positive connections in your life. Our study aims to group people that get along well. Now we are going to ask you one question about the people in your life with whom you usually do not get along well. It is important that you understand that we will keep all of your responses secret and that they will only be used for the purpose of mathematical analysis. Your information will never be shared with anyone.

13.) Who are the people in this town with whom you do not get along well? (including friends, family, people you work with, neighbors etc, who live or work in this village)

Do you have a patron/patrona?

14.) What is his/her name?

Participant attrition and missing data:

Over 93% of community residents have agreed to participate in the study, so bias due to failure to recruit participants is likely to be very low. Observations with missing data will be dropped from the analyses, which will also not create bias in the data as assignment to treatment groups, and therefore missingness, is random. We will also make our best effort to keep track of study participants who move from one study village to another, in order to retain them in the study.

Research Design

Assignment of villages to treatment arms in the 8x2 design.

We ran a procedure that generates 10,000 assignments of villages to treatment arms. This procedure generates assignments while controlling balance on the following variables.

Number of respondents in the village
Average number of respondents in the household
Latitude
Longitude
Elevation
Time to health center
Time to maternity clinic

This procedure prioritizes matching the means and standard deviations of 16 treatment arms, but also weights the third moments and cross-moments (to get at covariances). The procedure also assigns a score that summarizes balance.

We checked how similar these assignments are. Of the 100 assignments with a better balance summary score, no two contained a pair of treatment arms that overlap in more than 8 elements. We also measured the average maximum overlap, defined as the maximum number of items that any pair of treatment arms have in common. On average, over the 100 assignments with better balance summary score, the maximum overlap was 3.985. So, the top 100 assignments of villages to treatment arms were are all quite different.

Focusing on the 100 assignments with better balance summary score, as candidate re-randomizations, we then ran a battery of statistical tests check for statistically significant imbalances. We focused on the following variables and tests quoted in parenthesis.

Number of respondents in the village (t test)
Average number of respondents in the household *(t test)*
Latitude (t test)
Longitude (t test)
Elevation (t test)
Time to health center (t test)
Time to maternity clinic (t test)
Number of households (t test)
Empirical distribution of household sizes (Kolmogorov-Smirnov test)
Village can be accessed in when raining (proportions)
Proportion of indigenous population (t test)
Time in minutes to main road (t test)
Village is a coffee producer (proportions)
Average number of women of reproductive age (t test)
Average age (t test)
Percent male (t test)

For each of the 100 assignments we produced a table with these tests for each of 147 contrasts: 120 contrasts reported the p-values for imbalance in pairs of treatment arms for each of the covariates; 26 contrasts reported the p-values for marginal imbalance in pairs of levels of treatment; 1 contrast reported the p-values for marginal imbalance in the two nomination schemes.

We selected assignment number 73, which had only one imbalance at significance 5%. The imbalance was for average age between treatment arms 3 (random nomination, 10% treated households) and 14 (friend nomination, 50% treated households), which is an unimportant contrast for the purpose of conducting the analyses proposed in this study.

Assignment of households to treatment/control in each village.

Treatment is to be understood as percentage of households, which was applied to the number of households in each village and the rounded to the nearest whole integer number.

We built a network where a tie meant that there was a tie in any of the following name generators: personal private matters; spend free time; closest friend.

For each village, we generated 10,000 assignments of households to treatment in villages in the friend nomination arm. For each of these assignments we then computed balance for the following covariates.

Number of census respondents
Number of women of reproductive age
Number of children under the age of 12 who live in this household
Existence of handwashing location observed by enumerator
Household electricity
Separate room in the house that is used as the kitchen
Self-reported health status
Network degree centrality of the household

We also considered the following individual variables averaged at the household level.

Age
Sex
Baseline self-reported health status of the individuals in the households

Here, for each of the 144 villages assigned to a treatment arm in which the percentage of household treated differs from 0% and 100%, we only considered the contrast: treatment vs. control.
We then selected the set of assignments of household to treatment and control in each of the 144 villages that minimized the number of tests for imbalance failed at 5%.These balanced sets of assignments constituted the final assignments of households to treatment in villages in the random nomination arm, but only the initial assignment of households to treatment in villages in the friend nomination arm. For villages in the friend nomination arm, we picked each of the households initially assigned to treatment, in turn, then picked a person in the household, then picked a tie at random among this person's ties. Whenever we ran into a duplicate, we restarted the choice. This procedure generated a final set of assignments of households to treatment in villages in the friend nomination arm.

Statistical methods and details of analysis plan

Analytic Aim 1: Evaluate the Extent to which Behavior Change Regarding RMNCH Care Spreads
For this analytic aim, we will use the results of the RCT to ascertain the extent to which beneficial or harmful RMNCH *attitudes* and *behavior change* in one person can influence the likelihood of attitudes and behavior in other people to whom they are connected. The success of this aim will allow us to develop methods by which we can target specific individuals in future interventions to disseminate RMNCH care interventions in order to achieve the greatest uptake at the lowest cost. To test whether attitudinal or behavioral changes related to the outcome variables diffuse, we will measure the *indirect effect* of the treatment on the *friends* of the treated. We can do this with the following model:

$$E(y_j) = \alpha + \beta\tau_j + \lambda\Sigma_k a_{jk} \tau_k \qquad (1)$$

where y_j is a binary variable indicating whether subject j engages in the outcome behavior (e.g., skin-to-skin warming or following dry cord care protocol); τ_j indicates whether subject j was treated; β is the direct treatment effect; a_{jk} indicates if subject j names subject k as a friend; τ_k indicates whether subject k was treated; and λ is the indirect treatment effect that estimates the effect on subject j of having an additional friend who was treated. We will test the hypothesis that $\lambda = 0$. If $\lambda > 0$ then it means the treatment is spreading to other people.

Note that τ_k is randomly assigned, but $\Sigma_k a_{jk}$ is not (it is simply the total number of friends subject j has). Therefore, to preserve pure randomization, we have to condition model (1) on $\Sigma_k a_{jk}$ by running separate regressions for all individuals with the same number of friends (one model for all subjects who have 1 friend, another model for all subjects who have 2 friends, and so on). We then combine the coefficients from the separate regressions, weighting them according to the number of observations in each regression and adjusting the standard errors accordingly (this is similar to using meta analysis to combine effect sizes estimated from separate randomized controlled trials). To estimate the model we will use ordinary least squares, but to check for robustness we will also use a logit specification.

Specific questions we will be able to answer through these methods include: (1) Is there an inter-individual spillover effect for the intervention? (2) Does social influence of one's social contact facilitate faster or more effective adoption of the intervention? (3) Does influence regarding RMNCH care practices spread not only from mother to mother, but from mother-in-law to mother-in-law or husband to husband? (4) Are there covariate moderators to possible social influence effects, such as income, age, or education? (5) Is there an average number of degrees of separation (the number of steps in the network it takes to get from one person to another) at which the path of influence fades?

We will explore a variety of model specifications during our analysis phase, and conduct diverse robustness checks when the independent variable is not randomly assigned. We can evaluate the possibility of omitted variables or confounding events explaining the associations by examining how the type or direction of the social relationship between ego and alter affects the association between ego and alter (using a "network directionality" test)[11]. If unobserved factors drive the association between ego and alter, then directionality of friendship should not be relevant; the outcome variable in the ego and the alter will move up and down together in response to the unobserved factors. In contrast, if an ego names an alter as a friend but the alter does not reciprocate, then a causal relationship would suggest that the alter would significantly influence the ego, but the ego would not necessarily influence the alter.

To further test the robustness of the results, we will experiment with different error specifications, such as Huber-White sandwich estimates with clustering on the egos. We will also test for the presence of serial correlation in all GEE models using a Lagrange multiplier test[12]. Additionally, we will also conduct a sensitivity analysis specifically recommended by Vanderweele [13] in which we estimate the bias in the association between ego and alter that might be caused by an omitted variable that is correlated with the prevalence of the outcome variable in both ego and alter. This class of omitted variables includes those explain friendship formation based on the trait (homophily) and those environmental factors that could affect ego and alter independent of their relationship (confounding). These sensitivity analyses show how the association changes given differences in prevalence of an omitted variable that are conditional on the alter's trait and given the size of the effect of the omitted variable[13]. In this way we can evaluate how likely it is that an inference from observational data depends on something we have not measured. Of course, our experimental study is relatively unaffected by such concerns.

Analytic Aim 2: Test for Social Effects
One of the primary goals of this project is to understand the ways in which social network strategies can be used to maximize the population-level impact of interventions. By understanding and anticipating social effects, we can direct intervention resources so that they achieve the maximum impact per dollar invested.

Suppose we imagine an intervention in which there is no social effect (the "No Social Effect" red dashed line shown in **Figure 5**). In this situation, we would expect there to be a proportional increase in the number of people who change their behavior in response to the number of people targeted. So, perhaps when we target 20% of the population, 10% actually adopt; when we target 30% of the population, 15% actually adopt, and so on. In other words, there is a linear relationship, and the rate of adoption is directly proportional to the rate of initial exposure. In this scenario, no matter how many people we target, the *rate* of adoption will remain constant. We get just as much return on our investment when we target the first person as we do when we target the last. In contrast, if there are social effects, the relationship between percent treated and percent adopting will be *nonlinear* (the "Social Effect" blue dotted line shown in **Figure 5**). By social effects, we simply mean that people will tend to be influenced by the behavior of their social contacts. We hypothesize that, initially, this effect will be negative. When an intervention is unfamiliar to a community, people may discourage potential adopters, so that the rate of adoption will be lower than the "no social effect" line. However, we anticipate that once the intervention reaches a certain level of saturation enough adopters will exist in the population that people will stop being discouraged from adoption. Moreover, at some level, some untreated subjects will copy the behavior of their social contacts who have adopted. As more people adopt, the overall community attitude towards the intervention becomes more encouraging, and the positive cycle that ensues will create higher rates of adoption for every person targeted. Once most of the people who are likely to adopt have done so, however, the rate of adoption slows again, creating the classic S-shaped adoption curve shown in **Figure 5**.

A key aim of this study is to test whether or not we observe S-shaped adoption curves in the various behaviors that we target. Our RCT plan calls for varying the proportion of individuals that are exposed to the intervention at the town level. This variation allows us to test the difference in the rate of adoption at these various treatment proportions in a way that is un-confounded by the spillover that we aim to leverage. The null hypothesis is that adoption is a linear function of the number of people treated across villages that are randomly assigned to different treatment regimes:

$$y_i = \alpha + \beta x_i + \Delta t_i + \varepsilon_i \qquad (2)$$

where y_i is the outcome variable (such as early initiation of breastfeeding, use of thermal care, facility births, etc.) for village i; α is the baseline rate of adoption; x_i is the randomly-assigned proportion of individuals in the community that are treated; β is an estimate of the full treatment effect when all individuals in the village are treated; t_i is a binary variable that indicates whether the village has been randomly assigned to receive the friend-targeting treatment, and Δ is an estimate of the difference between friend targeting and random targeting.

In model (2), a test of $\beta = 0$ can be used to evaluate whether there is a significant effect of the treatment on the outcome. A test of $\Delta = 0$ can be used to evaluate whether friend targeting is more effective than random targeting. But this model does not test our hypothesis about social reinforcement because it assumes that the treatment effect is constant regardless of the fraction of the village treated. We therefore introduce an alternative model:

$$y_i = \alpha + \beta\,(1 + \exp(\,-(x_i + \Delta\, t_i - \theta) / \sigma)\,)^{-1} + \varepsilon_i \qquad (3)$$

Just like model (2), model (3) can be used to test the treatment effect (β) and the difference between friend targeting and random targeting (Δ). However, model (3) includes two extra parameters that allow us to test whether social reinforcement is present. The shape parameter σ indicates the strength of social reinforcement (lower σ yields more curvature towards 0 for low values of x_i and more curvature towards 1 for high values of x_i, indicating it is harder to get people to change when few people are treated and easier when many people are treated). The location parameter θ indicates the critical threshold where social reinforcement *in favor* of the behavior change takes over social reinforcement *against* the behavior. These two parameters will allow us to learn what fraction of a village can be treated in order to achieve nearly the same effect as treating the whole village.

To test whether social reinforcement is present, we can use nonlinear least squares to fit both models and conduct an ANOVA F-test. Note that this test requires the models to be nested and in this case they are since model (3) converges to a linear model just like model (2) as σ becomes large. If model (3) fits the data significantly better than model (2), then it suggests that social reinforcement is present. Detection of social reinforcement requires a 2 x 8 design because we need additional precision in the region between $x_i = 0$ and $x_i = 0.5$ to estimate the curvature away from the linear model. And we have chosen more cells where x_i is less than 0.5 because anecdotal evidence suggests that the critical threshold is in that region – some practices can become popular once just a few people adopt them.

Analytic Aim 3: Test the impact of the "nomination" network targeting method
In this aim, we attempt to ascertain whether it is possible to reduce the total percentage of people we need to treat in order to achieve the same effect.

Nomination targeting is the practice whereby an individual is chosen at random from the population, and then a random friend of that individual is targeted to receive the intervention packet. This targeting leads to intervening with individuals who have more social connections, and as a result are connected o more people in the network, and are therefore potentially more influential than individuals chosen at random. Previous work suggests that the adoption curve under nomination targeting is shifted to the left relative to random targeting because friends are more central in the network and therefore spread information and norms more quickly in the early part of the intervention. This shifts the whole adoption curve to the left, as shown in **Figure 5** (the "Enhanced Social Effect" shown by the solid dark blue line). As a result, the total percentage of individuals that need treatment to achieve nearly-maximum adoption also shifts to the left (in the example, it shifts from about 60% to 40%), meaning that we can treat even fewer people to achieve the same desired effect within each village.

We can explicitly test whether there is a shift in the adoption curve with the shift variable, Δ, noted in equations (1) and (2) shown above. A significant coefficient would suggest that friend targeting achieves significantly more efficient results than random targeting for a given health behavior (without increasing the number of people targeted or the expense involved). And, if so, it also suggests that it might be possible to apply this method to behavioral intervention targeting strategies in a variety of different contexts in order to maximize intervention uptake in many global settings.

Analytic Aim 4: Evaluate the Impact of Network Structural Characteristics on Behavior Change
Descriptive characteristics will be calculated for the network at the village level. Subgroups such as cliques, components, and communities will be identified within these village networks (in particular, using state-of-the-art community detection algorithms that have recently been invented [14]). These subgroups will be identified in two different ways. We will identify subgroups based solely on network properties, independent of any covariate properties; and we will also identify subgroups based upon correlated values of covariates, such as education, income, or gender norms.

Discovery of characteristic-based subgroups involves testing the assumption that individuals with strong network ties will be more likely to share common attributes, and that the stronger the network tie (i.e., the closer to each other they are within the network), the higher the correlation between attributes. While there is no unique method for characterizing this network correlation structure, we can build on methods from spatial statistics, making suitable modifications to adapt to the complexities of network topology.

Mean values of covariate variables within each relevant subgroup will be calculated and compared using regression models to determine whether these differ across groups. We will create additional variables for each individual participant for subgroup membership and regress our outcomes of interest, including adoption of the intervention and rate of adoption, against these predictors. With this type of modeling, we might address descriptive questions such as: (1) for which health behaviors or demographic characteristics are network correlations stronger or weaker; (2) are correlations based upon certain types of attributes (such as education or wealth) more predictive of RMNCH care behavior than others; or (3) are covariate-based subgroups more predictive of intervention uptake than covariate-independent subgroups?

Individual-level network characteristics, such as centrality and transitivity, will be calculated for each participant in the dataset. We will test whether these characteristics are predictive of individual intervention adoption as well as overall subgroup intervention adoption (i.e., do differing mean values of individual network characteristics at the subgroup level predict different rates of intervention adoption). Using the general framework in equation (3), we will then test whether these network characteristics are moderators of the relationship between important predictors, such as education and income, and our outcomes of interest.

Results of these analyses will include: (1) network measures for all individuals within the network and higher-order features like clusters and communities for the overall network itself; (2) results showing the relationship between network measures and outcomes of interest, including any possibly moderating effects; (3) recommendations for utilizing network measures, including network position and network subgroups, for future interventions

Analytic Aim 5: Ascertain Whether Partial Collection of Network Data Can Help Identify the Most Influential or the Most Influence-able People in the Network

One of the main hypotheses of the RCT of network targeting using our intervention is that the nomination technique will help us to quickly identify the most central people, and that this means of identification will allow us to diffuse the intervention more effectively. However, another important question we will address is what name generator to use to identify the nominated friends in the future. Is *friendship* the most important relationship to identify in order to find those who are most influential with respect to RMNCH care behaviors, or do name generators need to be specific to the context?[10] In other words, a person who is most socially central to the community may or may not be the most central in terms of health behavior opinions. Hence, we will analyze which name generators uncover the relationships most influential to our RCT behaviors. These analyses will inform a deeper understanding of the utility of name generators in identifying the most influential people depending upon the context.

The structure of networks can also greatly influence adoption. Depending on the behavior in question, a tightly knit community may resist changing norms, but it may change swiftly once such norms begin to be established. By applying network simulations to our network data, we will develop strategies for estimating optimal targeting given the observed rates of behavior transmission and the measured density and transitivity of the network. We will then experiment with sampling techniques that would allow for choosing which strategies work best when there is only partial network information.

Network centrality is intuitively appealing in terms of disseminating interventions. It makes sense that the people who are most "central" to a community may be those whose opinions have the most exposure within that community. If one wins those people over, then new behaviors and attitudes are likely to spread. However, this also assumes that those most central may be the most amenable to accepting the new behavior of interest. There is evidence that this may not always be the case. In densely connected communities where norms are tightly held within communities, *the most central individuals might be the most resistant to change*[15]*.* This may particularly be the case where the behavior in question challenges a strongly held norm, and where the community conspicuously sanctions transgressions. Central individuals may pay the highest cost for infractions, as their conformity to the norm is a matter of great interest to those around them.

Our work on the adoption of latrines in India suggests that the most central may resist adoption, and are potentially less amenable to change than those on the periphery of the network[15]. In a situation such as this, initially infusing the network from the periphery may make more sense than beginning at the center. On the other hand, interventions that use the center of the network as the point of infusion may find that the most peripheral to the network have not been engaged in the intervention. In this case, those people more peripheral to the center of the network, who were potentially the most marginalized to begin with, may become even more marginalized as a result of not being reached by the intervention. Our analyses will allow us to explore this too, and to develop guidelines for which kinds of interventions and which kinds of contexts benefit most from targeting the core versus the periphery of a network.

Analytic Aim 6: Assess the effect of the intervention on the village social networks

To assess the direct effect of treatment on individuals' local networks in the study, we will measure individual level network statistics (degree centrality, transitivity, closeness centrality, betweenness, and eigenvector centrality) at the end of the study and use a simple difference of means test to evaluate whether these metrics vary between subjects assigned to treatment and those that were not assigned to treatment. We will additionally regress network statistics on an indicator for whether the subject received treatment and network variables measured at baseline to improve efficiency in the comparison, and we will cluster standard errors by village to be sure that between village effects do not interfere with the estimation of the individual level effect.

To assess the indirect effect of treatment, we will use model (1) to estimate the effect of a friend's treatment on the subject's network measures as noted above. For example, if a friend receives treatment does that increase the number of the subject's social contacts within the village?
We will additionally measure village level network characteristics (mean degree, variance in degree, and mean transitivity) and assess the extent to which treatment assignments influenced these outcomes using model (2) as noted above. To improve efficiency, we can also include as an independent variable the village level network measure as assessed at baseline.

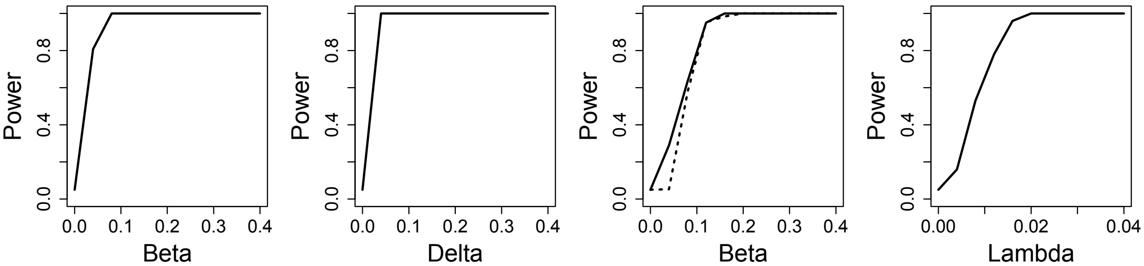
Power Calculations
We have conducted extensive power calculations on all 3 models shown above. In our core simulations, we assume that individuals in each village are exposed to the randomized treatments proposed for each village and the "true" probability of an individual outcome is generated by models (1), (2), or (3). We then used those probabilities to draw a Bernoulli random outcome for each individual based on their assigned probability.

Once we generated the "true" data, we then fit the corresponding model to it to see if the model generated significant hypothesis tests for a given set of parameters. We repeated this process 1000 times to estimate the percent of the time the model was able to identify significant results.
For models (2) and (3), we also explored whether model misspecification affected power (we used model (1) to generate the data and model (3) to recover it, and vice versa) and found that a two-stage approach works best. We fit both models to the data, and if there is evidence that model (2) fits better using the ANOVA test, we then use the estimates from that model. Otherwise we use the estimates from model (2).

In our core simulations, we assume that the baseline rate of the outcome is $\alpha = 0.2$. Across all simulations, most assumptions of the baseline rate yield identical results (only values near 1 potentially change the results because of ceiling effects on the possible size of the treatment), so this assumption is not critical. For simulations where we are not evaluating their power, we assume that the full treatment effect $\beta = 0.1$, and the difference between friend targeting and random targeting $\Delta = 0.1$.
These assumptions are conservative compared to the known treatment effects of the interventions we are evaluating here. For example, in our own pilot, we find that friend targeting can increase uptake of a nutritional intervention by 12.2%, compared to control populations[16]. They are also conservative relative to the power of the design, since below we show that we have more than enough power to identify effects of those magnitudes.

For simulations from model (3) we also assume that the shape parameter indicating the strength of social reinforcement $\sigma = 0.1$, and $\theta = 0.25$, which yields moderate curvature of a maximum about 0.2β away from the linear model when the percent of the village treated is $x_i = 0.25$. Little is known about what the right assumptions are here, but values of $\sigma > 0.2$ are approximately linear given the range of treatment effects for β that we tested, and alternative simulations that assume $\theta = 0.5$ yield similar results.
Finally, for model (1), we assume that the social connections a_{jk} are distributed as a random network with mean degree $\Sigma_k a_{jk} = 5$ (in other words, the average person has 5 randomly chosen friends). In alternative simulations utilizing small world and scale free networks, the results are identical, suggesting that the degree distribution and clustering in the network are not critical.

Figure SA 3 shows the main results of our power tests.



SA Figure 3: *The first panel shows that when we use our core simulation assumptions and vary the full treatment effect (β), we can detect values of β > 0.05 at least 80% of the time. Similarly, the second panel shows that when we vary the difference between random and friend targeting (Δ), we can detect values of Δ > 0.04 at least 80% of the time. In the third panel, we simulate data from model (2) and use the ANOVA F-test to evaluate whether model (3) fits better than model (2) to test for the presence of social reinforcement. The results show that even when the full treatment effect is quite low, we can still detect social reinforcement 80% of the time when β > 0.12. The solid line shows results for a 2 x 8 design, while the dashed line shows results for a 2 x 5 design. The 2 x 8 design shows slightly better power when β is small. In all other power analyses, there was no difference. Finally, in the fourth panel, we test model (1) and show that we can use within-village random variation in percent treated to detect the indirect effect on a subject of having an additional friend who was treated, even when it is very small (λ > 0.01). This value suggests we can identify a total indirect treatment effect for someone with 5 friends of about 0.05, which represents a "friend multiplier" of about 0.5 the size of the direct treatment effect. For comparison, note that total indirect effects are typically much larger than that, estimated to be about 1.7 for emotional contagion, 3.0 for public goods provision, and 4 to 5 for transmission of voting behavior .*

Evaluation of Model Specification

This is a randomized controlled trial, so the primary concern is not inclusion of control variables. Appropriate control variables can improve efficiency, but their exclusion does not bias the results as long as they are uncorrelated with the treatment. Therefore, the biggest threat to the research design is so-called "randomization failure." As with all our previous experimental studies, we plan to conduct thorough balance tests to establish whether or not – by chance – there is a relationship with assignment to treatment and village features such as population size, baseline outcome incidence prior to the intervention, and demographic factors. For model (1) our balance tests will also include outcome measures and demographic factors of friends to ensure that the friends of the treated have the same distribution of key variables prior to the experiment as the friends of the controls.

Another concern is model misspecification. We address this above by identifying a strategy for distinguishing between the linear model (2) and the nonlinear model (3) specification. For model (1) one potential alternative assumption is that the percent of friends treated rather than the total number of friends treated is the key variable for transmission of the treatment effect. We can easily test this alternative by fitting the following model for comparison:

$$E(y_j) = \alpha + \beta\tau_j + \lambda\Sigma_k a_{jk}\, \tau_k \,/\, \Sigma_k a_{jk} \qquad (1b)$$

Additionally, there may be heterogeneous treatment effects, and we can test for these by interacting the treatment variable with variables that explain the heterogeneity. For example, we can test whether there is a relationship between the sex of the friend and the indirect effect in model (1) to see if women are better at transmitting effects to friends. Similarly, we can test whether the sex of the subject influenced the indirect effect (are women more *susceptible* to indirect effects?)

For all models, we will study histograms of residuals to assess their normality and conduct Shapiro -Wilk tests. Non-normality suggests that we many want to transform one of the variables in the model or to use a different model. For example, the outcome variables in models (2) and (3) vary between 0 and 1. If treatment effects are large relative to that range (specifically, if $\beta > 1 - \alpha$), there may be ceiling effects that would create downward bias in our estimate of the treatment effect. If so, we may want to use censored regression instead of ordinary regression. Similarly, the outcome variable in model (1) is binary and so we may want to use a logit model rather than OLS, though these models typically yield similar results when treatment effects are small.

Fitting nonlinear least squares models requires additional attention to residuals. We plan to study residual Sum of Squares (RSS) contours, which are similar to the likelihood contours for a Gaussian general linear model. In particular, we will identify the 95 percent Beale's confidence region in each plane of two parameters to explore potential multicollinearities and their effect on mo del estimates. Given potential nonlinearities in the profile likelihood, we also plan to verify 95% confidence regions of estimated parameters using non-parametric bootstrapping.

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