RESEARCH ARTICLE SUMMARY

NETWORK SCIENCE

Induction of social contagion for diverse outcomes in structured experiments in isolated villages

Edoardo M. Airoldi and Nicholas A. Christakis*

INTRODUCTION: Social contagion is a powerful force in human affairs: People tend to copy the thoughts, feelings, and actions of those to whom they are socially connected. Advances in the understanding of human social network structure and function have made it possible to exploit such social contagion to intervene in the world to foster objectives, such as improving health, wealth, sustainability, learning, and more.

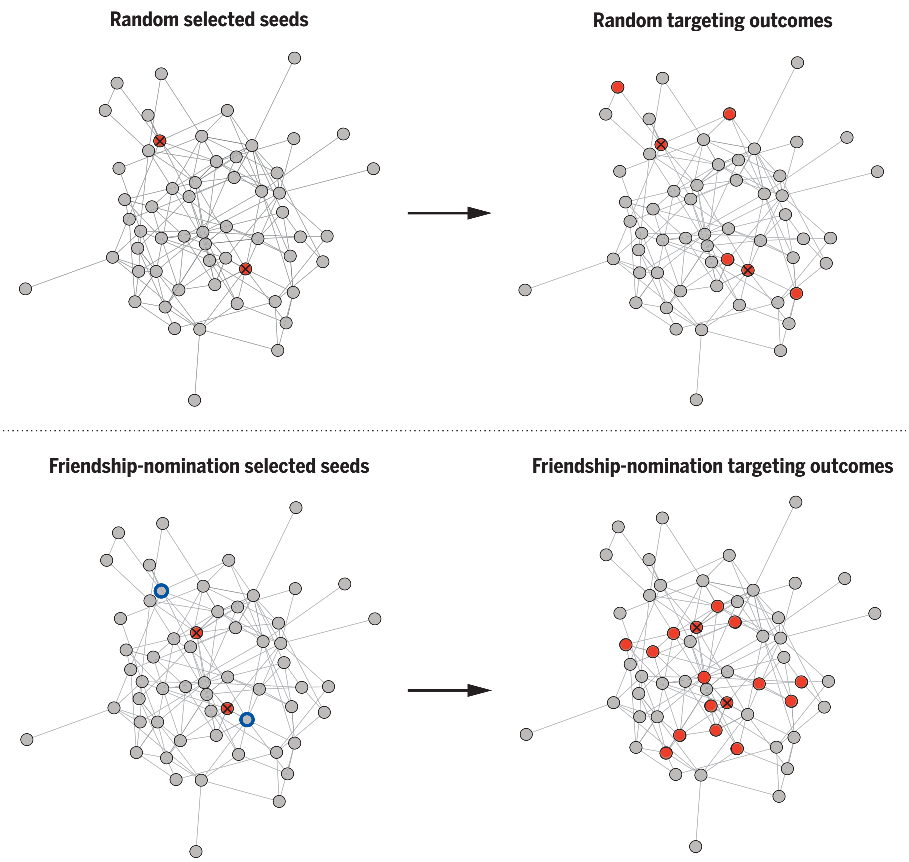
RATIONALE: To exploit social contagion, tools are needed to identify individuals within social networks who are better able to initiate desirable cascades. To be maximally useful, such tools should be deployable without having to spend time and money to actually map the face-to-face social network interactions of the people involved. Moreover, a better understanding of the threshold percentage of a population that must be seeded with an intervention to induce social contagion is also desirable.

In a large randomized controlled trial involving 24,702 people in 176 villages in the isolated western highlands of Honduras, we tested an algorithm known as friendship-nomination targeting. This approach relies on a basic mathematical fact about human social networks, known as the friendship paradox, which states that "your friends have more friends than you do." As a result, if one picks random people (or households) in a village and then delivers an intervention to a randomly chosen social connection of such random people within the same village, this should increase the level of social contagion and maximize the impact of the intervention. We compared this approach with a control strategy of picking people at random. In addition, across the villages, we also varied the fraction of people chosen for seeding.

The individuals chosen as seeds were all given the same 22-month maternal and child health intervention. We then measured outcomes 2 years later in all residents of every village—whether they got the intervention or not—so as to capture the relevant spillovers.

RESULTS: We find that a meaningful improvement in health outcomes can be achieved by deploying friendship-nomination targeting compared with random targeting. This effect manifests a threshold that, in turn, varies by outcome, which means that different fractions of seeds needed to be used for different outcomes of interest. We find that easier-to-adopt outcomes at the individual level also spread more easily to individuals (and households) that did not receive the intervention and that knowledge spreads more easily compared with practices. Relatively more-educated people induced greater spillovers. We also demonstrate that social contagion could be experimentally induced to two degrees of separation—that is, upon a person's friends' friends.

CONCLUSION: Social networks intrinsically amplify the information and behaviors with which they are seeded. Social, economic, and health interventions often seek to target all members of a relevant population, such as a village, school, or firm. But social network–targeting methodologies mean that intervening in a smaller fraction of a population, especially if it is chosen shrewdly, could have the same effect as targeting 100% of the population. Deploying interventions through friendship-nomination targeting, without increasing the number of people targeted or the expense incurred, can enhance the adoption and spread of beneficial interventions and thereby improve human welfare. ■



Using friendship-nomination targeting to enhance social contagion. (Top) Two out of 58 people (red circles with an X) in a village network are randomly chosen to be seeds for an education intervention. At follow-up, they themselves have adopted the relevant practice and have influenced four other villagers to do so (plain red circles). (Bottom) For each of the same two random people (circles with blue perimeters), a random friend is chosen to be a seed instead (new red circles with an X). These people have a different location in the network. At follow-up, these seeds have influenced more people (14) to adopt the relevant practice.

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RESEARCH ARTICLE

NETWORK SCIENCE

Induction of social contagion for diverse outcomes in structured experiments in isolated villages

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Certain people occupy topological positions within social networks that enhance their effectiveness at inducing spillovers. We mapped face-to-face networks among 24,702 people in 176 isolated villages in Honduras and randomly assigned villages to targeting methods, varying the fraction of households receiving a 22-month health education package and the method by which households were chosen (randomly versus using the friendship-nomination algorithm). We assessed 117 diverse knowledge, attitude, and practice outcomes. Friendship-nomination targeting reduced the number of households needed to attain specified levels of village-wide uptake. Knowledge spread more readily than behavior, and spillovers extended to two degrees of separation. Outcomes that were intrinsically easier to adopt also manifested greater spillovers. Network targeting using friendship nomination effectively promotes population-wide improvements in welfare through social contagion.

Advances in understanding the structure and function of human social networks have yielded insights for exploiting social contagion to change knowledge, attitudes, and practices at both individual and population levels (*1*–*12*). But deliberately fostering social contagion in face-to-face social networks requires a means for efficiently identifying a subset of structurally influential individuals upon whom to intervene. Simulations have suggested that simply targeting highly connected nodes would be effective (*13*–*17*). Other research has suggested more-complex methods for the optimal choice of targets (or "seeds") (*18*–*20*). Therefore, a range of theoretical and empirical research has indicated that network-informed approaches might magnify the effectiveness of public health and economic development efforts.

Unfortunately, virtually all extant methods for choosing targets require some knowledge of the structure (or composition) of the whole network [one less-demanding method is to survey a village to identify a subset of villagers composed of gossips to use as seeds (*21*)]. That is, most methods to amplify the effect of treatment on untreated units within a population require mapping whole social networks to identify appropriate seeds. But such mapping is costly, time-consuming, and often infeasible in real-world, face-to-face situations. Yet, if network insights are to meaningfully inform the design of policy and interventions, then simple, cost-effective procedures must be developed to identify structurally influential targets.

In this work, we evaluate, in an extended way, a previously proposed strategy that does not actually require the ascertainment of global network structure beforehand and that may offer other benefits in terms of enhancing contagion effects (*8*). The strategy exploits the so-called friendship paradox of human social networks: On average, the friends of randomly selected individuals are more central in the network than the individuals who identify them—colloquially, "your friends have more friends than you do" (*13*, *22*, *23*). Such individuals can constitute a more efficient group in which to launch interventions (Fig. 1).

In this strategy, one can, for any random person, ask them to identify their friends or other social contacts; then, one can pick one of those contacts at random to serve as a seed for social contagion. Such contacts have, on average, higher degree (and may differ in other topological metrics; see the supplementary materials, section IV). Distinctly, a further advantage of friendship paradox–based targeting is that choosing seeds explicitly on the basis of high degree or centrality, say, would typically result in clusters of interconnected seeds in the middle of the network, given the fundamental degree assortativity of social networks (whereby popular people are preferentially attached to other popular people) (*24*, *25*); this would mean that such seeds might be chosen in a potentially needlessly redundant and inefficient way. Friendship-nomination targeting distributes the seeds throughout a network. Finally, seeds chosen with the friendship-nomination algorithm might have other desirable (and potentially difficult-to-observe) nontopological traits (e.g., on average, they might be relatively more charismatic or innovative) (*26*).

Therefore, we tested the ability of the friendship paradox–based network targeting strategy to induce spillover effects and maximize overall adoption for any given amount of intervention administered, especially among nontargeted people in a population. We also explored the impact of such an approach across a broad range of outcomes potentially subject to social contagion because the ability to foster social contagion is likely to vary by the outcomes being manipulated (for instance, characterized as knowledge, attitudes, and practices or characterized according to how easy the outcomes are to adopt). Using a multifaceted, 22-months-long public health intervention delivered face-to-face in people's homes in isolated villages in Honduras, we evaluated (i) what fraction of households in a community needs to be targeted to maximally change various outcomes (captured with a diverse set of 117 measures) and (ii) whether friendship paradox–based nomination targeting is more efficient than a control strategy of random targeting.

Secondarily, we also sought to understand why social contagion might be easier to induce for some outcomes than for others. Ultimately, social contagion depends not just on network dynamics but also on individuals' cognitive and psychological processes that intersect with attributes of the spreading phenomena themselves. Individual psychology matters (*27*, *28*)—some behaviors are intrinsically easier to change than others, and a person's social environment affects their ability to enact change (*29*). This suggests further ideas that we can also test, namely, that knowledge will be easier to change at the population level compared with practices, that easy-to-adopt outcomes should spread more readily, and that adoption of innovative outcomes among a higher percentage of a person's social environment should facilitate a person's own adoption (especially when something is harder to adopt, such as a practice). Hence, by evaluating network targeting strategies in a setting with many outcomes, we also can evaluate such claims in parallel with theories about how social contagion may depend on the topological location of seeds (which is our primary focus).

Methods

Isolated villages, network mapping, and target selection

A total of 24,702 people (in 10,013 households in 176 villages) were enrolled [see supplementary materials, section II, for CONSORT (Consolidated Standards of Reporting Trials) diagram and other details], and they acceded to participate in a baseline social network and attributes survey, be randomized, get the 22-month intervention (if chosen as a target household), and be surveyed periodically. Based on a census that we performed, we can compare the 24,702 people who participated (81.2%) with

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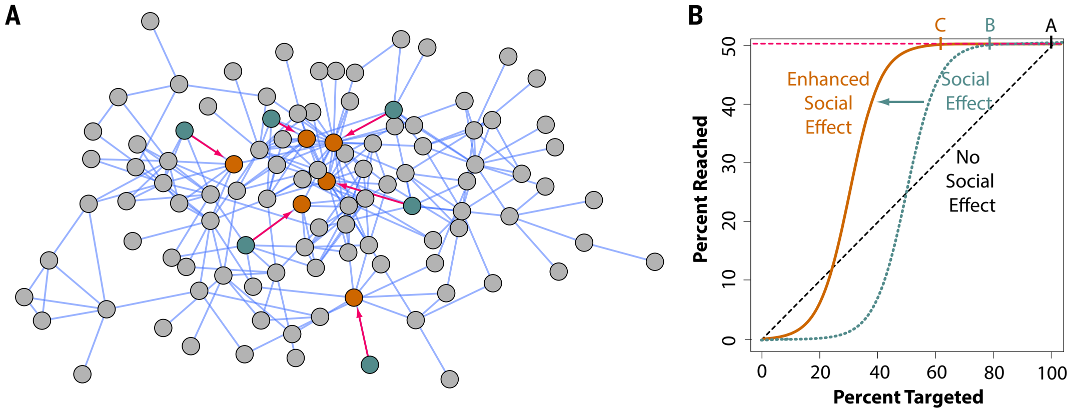

Fig. 1. Conceptual approach to friendship paradox nomination targeting. (**A**) Illustrative network map of a hypothetical village of 105 households. Households selected at random (light blue) and households selected by the friendship-nomination technique, whereby a social contact of the random person is in turn randomly chosen (orange), are shown. On average, households selected by friendship nomination are more central in the network and have higher degree compared with the randomly selected households. (**B**) Theoretical model of the impact of such network targeting. The *x* axis denotes the percentage of people in a village targeted for an intervention, and the *y* axis shows the fraction of all people in the village who ultimately adopt the intervention. Note that the *x* axis includes the full 0 to 100% range, but the *y* axis starts at 0% and arbitrarily stops at 50% because we assume in this illustration that there is no presence of the outcome initially and that there will be some upper limit on adoption associated with any intervention. The black dashed line indicates no social effect; each person targeted has an equal chance of adopting regardless of the number of others treated. The light blue dotted line shows the results under a social effect. People targeted at random for an intervention may be reluctant to change their behavior when few others have. That is, 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 rapidly. Eventually, so many people have adopted that there is no willing person left to adopt, and the per-person effect decreases once again. In this situation, targeting 75% of the individuals (point B) captures nearly 100% of the total possible intervention benefit (point A). Finally, the orange solid line shows the impact of friendship-nomination targeting. If the targeted people are well-connected, there will be greater exposure to the intervention through diffusion, shifting the whole S-shaped curve up and to the left. It takes fewer people to reach the critical threshold, and it is possible to reach saturation with a smaller percentage targeted. Targeting 58% of people is equal to targeting 100% in this illustration (point C). Points A, B, and C are estimated in the empirical analyses, within confidence intervals to account for estimation error.

the 5720 who did not participate (18.8%) along three demographic attributes. There was no difference by marital status (58% of participants were in a marriage or civil union versus 58% of nonparticipants; $P = 0.92$) or by age [mean age of participants was 32.8 years old (SD = 17.1; range, 11 to 93) versus 32.6 years; $P = 0.44$]; however, participants were more likely to be female (58% of participants were female versus 35% of nonparticipants; $P < 0.001$). The village populations (adolescents and adults) ranged from 41 to 509 individuals, and the average household size was 2.5 (±1.4); on average, households had 7.7 (SD = 5.2; range, 0 to 43) close social connections.

The networks were mapped sociocentrically (i.e., discerning all relevant social ties within each village) using Trellis software (*30*), and a battery of name generator questions captured many social relationships (table S1). To develop the core social network that was used to select the targets and perform the primary analyses, we used three key name generators: who people identified as those with whom they (i) "spent free time," (ii) discussed "personal or private" matters, or (iii) were "close friends." We symmetrized the graph (counting social connections if either party identified the other). A comprehensive survey measured personal attributes and outcomes (based primarily on validated tools) (*31*, *32*). Final outcomes were collected at 2 years after the baseline survey, shortly after the intervention was over. We measured changes in knowledge, attitude, and practice outcomes for all individuals within all the villages, including those who got the intervention and those who did not (wholly separate, masked teams administered the intervention or collected scientific data).

The 176 villages were randomized using an 8 × 2 factorial design with two axes: (i) the percentage of people targeted per village (0, 5, 10, 20, 30, 50, 75, and 100%) and (ii) the method of choosing the target households (random targeting or friendship paradox–based nomination targeting). Our protocol was published in advance (*33*). Because the intervention was delivered at the household level, targeting was also done at the household level (supplementary materials, section II)—i.e., households were the units of randomization within villages. Each of the 176 villages was randomly assigned to one of the 16 percentage-by-method combinations. At the extremes, no households (0%, in 22 villages) or all households (100%, in 22 villages) were selected for intervention (these can be seen as the anchor points in the panels of Fig. 2); otherwise, there were 11 villages in each cell. Varying the fraction of the population chosen as seeds allowed us to evaluate a threshold effect in the induction of social contagion. Methodologically, when we assess contagion here, we mean, strictly, the effect of treatment of the treated on the outcomes of the untreated, and we use the terms spillover and contagion interchangeably (see supplementary materials, sections VII and VIII, for mathematical definitions).

Maternal and child health outcomes

To assess the utility and impact of network targeting, we chose as a focus the serious challenges related to maternal and child health. Rates of neonatal mortality remain high in lower- and middle-income countries (LMICs) (*34*). In Honduras, neonatal deaths accounted for 51% of all deaths of children under 5 years of age, and 40% of these deaths were attributable to premature labor and another 40% to asphyxia and infection (*35*). Furthermore, 57% of all births occur in rural areas, where perinatal care may be insufficient (*36*).

Many interventions have been tested in LMICs and have been shown to improve the knowledge, attitudes, and practices of the people to whom the interventions are given (*37*–*39*). The experimental treatment of interest in this work is therefore not the intervention used to improve health but rather the methods used to choose a subset of structurally influential targets within each village to be given otherwise well-established interventions. That is, our objective is not to evaluate the 22-month intervention (we rely on prior work showing that the intervention does typically work among the direct recipients themselves) but rather to assess which social network targeting percentage and method maximizes the population-wide impact of the intervention, including among non–intervened-upon households. Nevertheless, because of the underlying design of our trial, we are also able to test whether the educational intervention worked

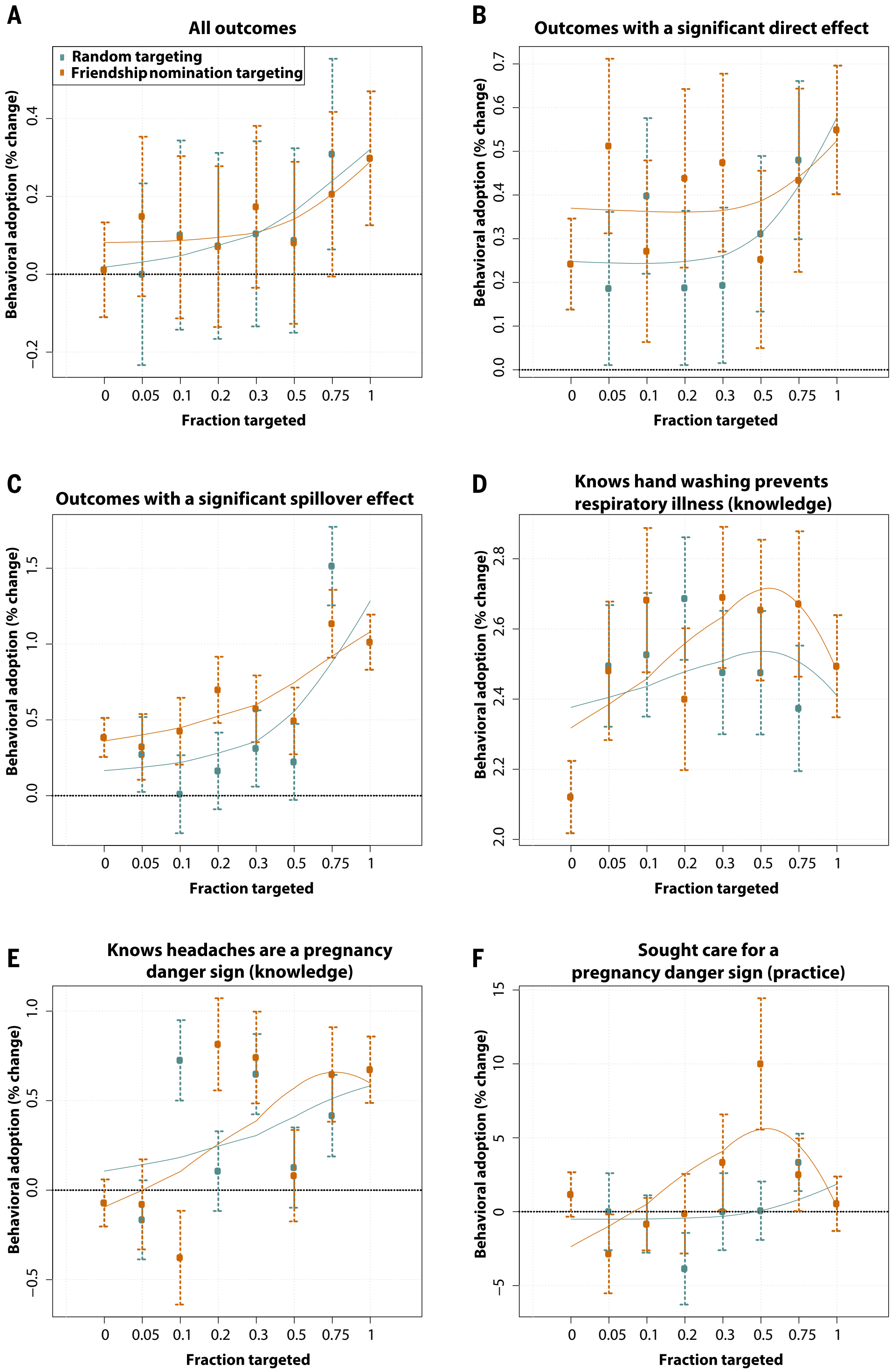

Fig. 2. Effects of network targeting as a function of targeting method and fraction of households treated in a village. We quantify change in adoption due to targeting (through either random or friendship-nomination targeting) as the log odds of the change in adoption over time for targeted versus nontargeted households. We first compute change in adoption over time for a targeted, or a nontargeted, household, as the ratio between adoption at follow-up and adoption at baseline roughly 2 years earlier for that household. Then, we compute behavioral change due to targeting as the log odds of the behavioral change over time comparing targeted with nontargeted households (*y* axes). This is plotted against the targeting fraction (*x* axes). (**A**) Effects of friendship-nomination targeting versus random targeting for all $N = 117$ outcomes. (**B**) Effects of nomination targeting for all outcomes with a significant direct effect ($N = 33$ outcomes). (**C**) Effects of nomination targeting for all outcomes with a significant spillover effect ($N = 18$ outcomes). (**D** to **F**) Effects for three illustrative individual knowledge and practice outcomes. (D) and (F) illustrate outcomes outside the group of 33 outcomes in (B). The magnitude of the improvement in adoption under nomination targeting is larger in villages with small-to-medium percentages of households targeted, as expected.

in those households to which it was directly given.

A total of 3632 households across both targeting methods and dosages were randomly selected to receive the intervention. The intervention was based on the timed and targeted counseling methodology complemented with other methods of face-to-face communication. It consisted of a series of household-level counseling sessions directed at maternal, child, and neonatal practices using 15 distinct educational modules (implemented from November 2016 to August 2018; supplementary materials, section III). As is typical, the intervention was delivered by trained community health workers on a monthly basis.

The intervention focused on many diverse outcomes involving knowledge, attitudes, and practices related to a range of phenomena: (i) use of folic acid in women of reproductive age to prevent birth defects; (ii) receipt of prenatal care in the first trimester; (iii) preparation of a birth plan for seeking timely prenatal care, institutional birth, postpartum care, and help in emergencies; (iv) immediate breastfeeding after birth; (v) proper thermal and cord care for newborn infants; (vi) exclusive breastfeeding for infants under 6 months; (vii) proper treatment of diarrhea in children (including the use of zinc) as well as the use of handwashing and other means to prevent diarrheal, respiratory, and other infections; (viii) paternal involvement in child care, particularly for newborns; (ix) use of modern family planning methods; and (x) delay of pregnancy until 18 years of age. A total of 117 measures related to these outcomes were assessed at both baseline (wave 1, in 2016) and follow-up (wave 3, in 2019) (see fig. S3 for a timeline). The objectives chosen for intervention and other features of this trial were influenced by extensive local community involvement and two-way communication (supplementary materials, section I).

An important aspect of our analyses was to observe for which outcome measures the intervention had an effect on the targeted individuals themselves. If the intervention had no primary effect on those individuals to whom it was given, we cannot generally expect it to have had a strong secondary (or indirect) effect on those people to whom the targeted households were connected. Thus, for the 117 outcomes, we first calculated the improvement in the adoption of the corresponding knowledge, attitude, or practice at the follow-up timepoint, household by household; then, we fit generalized linear models to these household-level adoption data to evaluate whether the intervention had a primary impact on the directly treated individuals (see supplementary materials, sections V, VI, and VII, for details). This proved to be the case (conservatively) for 33 outcome measures, and we call these the direct measures.

Results

Impact of friendship paradox–based nomination targeting

We quantify change in outcomes due to targeting as the log odds of the change in outcome over time for targeted compared with nontargeted households. We first compute the change in outcome as the ratio between adoption at follow-up and adoption at baseline 2 years earlier for that household (i.e., the percent of correct responses in the survey over villagers in that household, for any given outcome). Then, we compute the change due to targeting by comparing the log odds of the behavioral change over time for targeted versus nontargeted households. In essence, each amount of improvement is standardized by dividing it by its baseline. That is, the percent increase or decrease in adoption of outcomes is computed as log ratios of the adoption between t_1 and t_3, Y = log(percent correct t_3/percent correct t_1), which gives the percent increase or decrease.

Before reporting findings on individual outcomes, or subsets of outcomes, we confirmed, using a higher criticism analysis, that there was an overall effect of the intervention itself on villagers in general. Distinctly, we also confirmed that there was an overall effect of friendship-nomination targeting compared with random-nomination targeting. We did this for the ensemble of 117 outcome measures, rejecting the respective global nulls of no effect (supplementary materials, section VI).

In Fig. 2, we show village-wide adoption by treatment status, using this log odds quantity on the y axis, for each targeting fraction on the x axis (each dot is the median over 11 villages). We show this for (i) all 117 outcome measures as a group, (ii) the 33 direct outcome measures as a group, (iii) the 18 outcomes for which there was a documented spillover effect, and (iv) three illustrative individual outcome measures (related to handwashing and prenatal danger signs).

The overall effect of nomination targeting is less apparent when reviewing the results for all 117 outcomes combined (Fig. 2A) because this includes 84 outcomes for which there was no direct effect (although the benefit of increasing the targeting fraction in a village is still apparent). Still, nomination targeting generally dominates random targeting at all targeting percentages, and the curves in Fig. 2, A to F, are generally shifted up and to the left. As the targeting fraction increases to 100%, there is necessarily less and less difference in which specific households are chosen as targets, so the difference between the improvement in adoption seen when comparing nomination targeting with random targeting thus decreases or vanishes. This is visually apparent with the convergence of the curves as the targeting fraction increases.

Variation in social contagion by outcome

We quantified the advantages of targeting households using the friendship paradox–based nomination technique in several further ways. First, in Fig. 3A, the x axis measures the percentage of correct responses at follow-up (note that the percentages in this figure are different quantities than the targeting percentages), and we show the percentage of households adopting the outcome (i) in villages that received 100% targeting, (ii) in villages with varying fractions of randomly selected households, and (iii) in villages with varying fractions of households selected using the friendship-nomination technique. Then, we estimated the fraction of households (averaged over the villages in the relevant treatment group) selected by random targeting that led to an adoption rate statistically indistinguishable from the adoption rate measured in villages where 100% of the households were treated (corresponding to point B in Fig. 1B). Finally, we estimated the fraction of households selected by friendship-nomination targeting that led to an adoption rate statistically indistinguishable from the adoption rate measured when households were chosen at random (corresponding to point C in Fig. 1B).

For instance, the top row in Fig. 3A shows these three adoption percentages for the outcome of the respondent believing that a woman should be at least 18 years old to have her first child, and they are 98.7% (in villages with all households treated), 98.7% (random nomination, with 32% of the households treated), and 98.4% (friendship nomination, with 32% of the households treated). That is, using the nomination method, targeting 32% of households yielded the same result as targeting 100%. The second row in Fig. 3A shows these three percentages for the outcome of the subject believing that the father should accompany the mother at prenatal care, and they are 95.1% (in villages with all households treated), 94.4% (random nomination, with 39% of the households treated), and 94.2% (friendship nomination, with 32% of the households treated). The results for the 33 direct outcome measures are shown in Fig. 3A (see fig. S3 for results for all 117 outcomes).

Of the 117 outcomes, there are 113 outcomes (96.6%) where the outcome adoption from friendship-nomination and random targeting near-achieve the adoption of the households in the 100%-treated villages. Looking at all 117 outcomes, and based on an intention-to-treat analysis, friendship-nomination targeting is discernably more efficient compared with random targeting for 34 of them, with an average reduction in the targeted fraction of households that need to be treated of 7.4% [$P < 0.01$; 95% confidence interval (CI), 5.1 to 9.7%] to obtain an induced change in knowledge, attitudes, or practices that is statistically indistinguishable from selecting households at random (or, equivalently, from treating all households). In addition to the foregoing intention-to-treat analysis, we can also perform an empirical analysis that takes into account the actual delivery of the intervention. That is, using the empirical fractions of households receiving at least 15 (out of 22) visits as a basis for saying that a household was treated (supplementary materials, section III), nomination targeting is more efficient for 93 of the 117 outcomes ($P < 0.01$), albeit with an average reduction in the fraction of households that need to be treated of 2.89% ($P < 0.01$; 95% CI, 2.16 to 3.62%). For some outcomes, it might appear (Fig. 3A) that random targeting is more efficient than nomination targeting in the intention-to-treat analysis; however, when controlling for the actual delivery of the intervention, nomination targeting is always more efficient (and this is also reflected by the number of points to the right of the zero line versus the number of points to the left of it in Fig. 3B).

We then carried out several subgroup analyses. Focusing on the 33 outcomes with a direct treatment effect, nomination targeting is more efficient for 12 of them, with an average reduction in the fraction of households that need

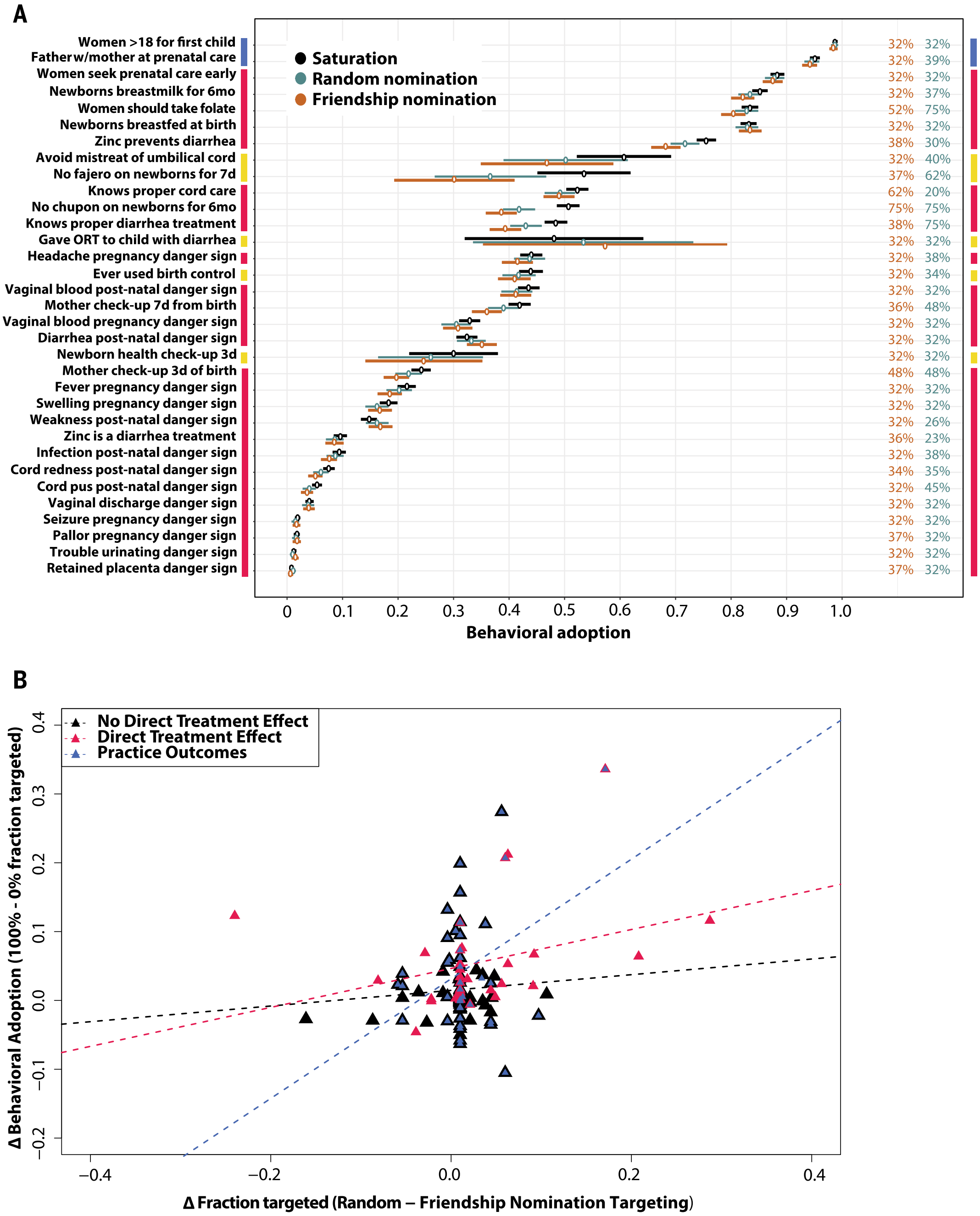

Fig. 3. Effect of nomination-nomination targeting on outcome adoption. (**A**) The *x* axis measures behavioral adoption at follow-up, in terms of the percentage of correct responses among all individuals in three sets of households: households in 100%-treated villages (saturation; black), households in villages with random targeting (light blue), and households in villages with nomination targeting (orange). This is shown for 33 of the 117 outcome measures (for which there was a direct effect), arranged in order of mean response for the outcome at follow-up in the 100%-treated villages. The lines show the average adoption (and the interquartile ranges) over the villages in the relevant treatment group. The colored ribbons near the variable names indicate knowledge (red), attitude (blue), and practice (yellow) outcomes. The percentages of households that need to be targeted in the random (light blue) and nomination (orange) strategies to achieve the same ultimate percentage as achieved in the saturation targeting (whatever that percentage might be) are noted on the far right. (**B**) Scatterplot showing the relationship, for 117 outcomes, between the ease of inducing changes in outcomes in the population, given the nature of the intervention used and the underlying opportunity for the population to change (*y* axis), and the extent to which friendship-nomination targeting is more efficient than random targeting (*x* axis). We measure ease of inducing outcome change as the difference between adoption (at follow-up) in the villages where all of the households were treated and adoption in the villages where none of the households were treated; the larger the values are, the easier it is to adopt the outcome. We measure the extent to which friendship-nomination targeting is more efficient than random targeting as the difference between the empirical (or realized) fraction targeted by random targeting that is needed to replicate the adoption in the villages where all of the households were targeted and the empirical fraction targeted by friendship nomination that is needed to replicate the adoption in the villages where all of the households were targeted; the larger the value is, the larger the advantage of using friendship-nomination targeting, in terms of a lower fraction of people receiving the intervention that is needed to accomplish the same change in outcomes. For instance, on the *x* axis, 0.2 means that 20% fewer households need to be targeted to get the same change in outcomes. On the *y* axis, 0.2 means that there is a 20% change in outcomes between the 0% and 100% villages at follow-up. For easy-to-learn or easy-to-acquire outcomes, nomination targeting is generally more efficient than random targeting (correlation 0.20). If we restrict the analysis to just the direct outcomes (red), this relationship gets stronger (correlation 0.32), and if we restrict the analysis to just the practice outcomes (blue), this relationship is also stronger (correlation 0.44).

to be treated of 12.31% ($P < 0.01$; 95% CI, 6.67 to 17.95%). Correcting for actual delivery of the intervention (using the empirical fractions of households receiving at least 15 visits), nomination targeting is more efficient for 27 of them (out of 33), with an average reduction in the fraction of households that need to be treated of 5.04% ($P < 0.01$; 95% CI, 2.77 to 7.31%).

Using the complete list of 117 measures, we also focused separately on knowledge, attitude, and practice outcomes. Out of 68 knowledge outcomes, nomination targeting is more efficient for 20 of them, with an average reduction in the fraction of households that need to be treated of 7.70% ($P < 0.01$; 95% CI, 4.22 to 11.18%);

correcting for actual delivery of the treatment, nomination targeting is more efficient for 54 of them, with an average reduction in the fraction of households that need to be treated of 3.14% ($P < 0.01$; 95% CI, 2.02 to 4.26%). Out of five attitude outcomes, nomination targeting is more efficient for one of them, with a reduction in the fraction of households that need to be treated of 7.1% ($P < 0.01$); correcting for actual delivery of the treatment, nomination targeting is more efficient for all five of them, with an average reduction in the fraction of households that need to be treated of 1.9% ($P = 0.05$; 95% CI, 0.07 to 3.73%). Out of 44 practice outcomes, nomination targeting is more efficient for 13 of them, with an average reduction in the fraction of households that need to be treated of 6.96% ($P < 0.01$; 95% CI, 3.69 to 10.23%); correcting for actual delivery of the treatment, nomination targeting is more efficient for 34 of them, with an average reduction in the fraction of households that need to be treated of 2.64% ($P < 0.01$; 95% CI, 1.67 to 4.61%). Thus, qualitatively, nomination targeting has more of a village-wide impact on knowledge outcomes than on attitudes or practices.

Last, we can focus on the effect of intervention specifically on the untargeted households for the set of the 113 outcomes for which adoption from friendship-nomination and random targeting near-achieve the adoption of the households in the 100%-treated villages. The reduction in the fraction of households that need to be treated using friendship-nomination targeting, compared with random targeting, is 1.47% ($P < 0.01$), with a range between 0.89 and 2.04%, to achieve the same adoption on the untargeted households. When we focus on the 87 outcomes (77% of the 113 outcomes) where friendship-nomination targeting is more efficient than random targeting, the average reduction in the fraction of households that need to be treated using friendship nomination is 3.26% ($P < 0.01$), with a range between 2.68 and 3.83%, to achieve the same adoption on the untargeted households. To be clear, the foregoing results are for the full dataset, including villages with both low and high nominal targeting fractions, and the marginal benefits of friendship-nomination targeting (in terms of efficiency) compared with random targeting for the adoption of the outcomes specifically for untreated villagers are materially higher at the lower targeting fractions. Combined with the insights from Fig. 2 that most of the improvements in efficiency come from the lower target fractions (e.g., 5 and 10%), these results suggest meaningful gains in efficiency (in terms of outcome adoption) in the subpopulation of people who were not intervened upon.

Given that we have 117 outcomes, we can evaluate the circumstances under which nomination targeting helps the most in a further, original way. We computed the average improvement in the percentage of correct responses with respect to the 117 outcomes among households in villages where 100% of the households were treated compared with villages where 0% of the households were treated by subtracting these two quantities for each outcome. The resulting difference is one assessment of how easy it was to acquire certain knowledge or adopt a certain practice in response to getting the intervention, given the particular population, outcome, and intervention at hand. The bigger the number is, the easier the outcome is to learn or adopt. For instance, for the outcome of having knowledge regarding the importance of exclusive breastfeeding for 6 months, the rate of knowledge about this at follow-up in villages where no household was targeted was 29.5%, and the rate of knowledge about this at follow-up in villages where every household was targeted was 50.7%; in this case, the difference was 21.2%, which means that it was easier to learn this than the outcome of having knowledge regarding treating diarrhea with zinc use, where the analogous percentages were 68.7, 75.5, and 6.9%. We also performed sensitivity analyses that account for potential floor and ceiling effects in the improvements in adoption, but they do not yield materially different conclusions, in part because most adoption rates are in the middle of the distribution (fig. S8). The ability to compute such a quantity is an appealing feature of our research design because this quantity (the improvability of an outcome) is very rarely empirically available simultaneously for a broad range of outcomes in a given setting.

Then, we sought to quantify the relationship between the individual-level improvability of the outcomes and the relative benefits in terms of social contagion from friendship-nomination targeting. We estimated the correlation, across the 117 outcomes, between the ease of expressing the outcome, on the one hand, and the differential efficiency of nomination targeting over random targeting (measured as the difference between the fraction of households targeted in random versus friendship-nomination villages that are necessary to induce a behavioral adoption indistinguishable from the adoption in villages where all households were treated), on the other hand. We found that, for easy-to-learn or easy-to-acquire outcomes, nomination targeting is generally more efficient than random targeting (intention-to-treat dosage: correlation 0.20) (Fig. 3B). In other words, experimentally induced spillover effects are larger for outcomes that are easier to change in the seed individuals themselves. The foregoing relationship gets stronger once we control for the empirical fractions that actually received the intervention (correlation 0.24). Furthermore, if we restrict the analysis to just the 33 direct outcomes, this relationship also gets stronger (correlation 0.32). If we restrict the analysis to just the 44 practice outcomes, this relationship gets even stronger (correlation 0.44). See table S5 for details.

Threshold effects in network targeting

In the foregoing analyses, we measured outcomes at follow-up in the villages treated with various targeting percentages and compared outcomes across target selection strategies. However, Fig. 4 shows summary measures of a dose-response relationship between targeting fraction and a dependent variable based on the change in percentage of villagers manifesting an outcome (the same log odds used in Fig. 2). The dose-response curves for different subgroups of outcome measures are shown. The benefits of higher targeting fractions are especially apparent for the subsets of outcomes that had a direct effect (Fig. 4A) and for the outcomes that involve practices (Fig. 4B). In other words, even though practices changed less easily than knowledge, the change in practices was more amplified by a rising fraction of villagers treated. Furthermore, higher fractions were especially effective if the outcome was easier to change (Fig. 4C) or if the outcome applied to the whole population (e.g., diarrhea treatment) rather than just to new mothers (e.g., breastfeeding) (Fig. 4D).

Relevance of education

We quantified whether and to what extent education can modulate the effect of intervention on those who were not assigned to get the intervention. Using the highest education level attained by any individuals associated with a given household indicates that the median education level is fourth grade at the household level, which we therefore labeled as high education. Looking at all 117 outcomes, we find that there is a positive interaction between education level and the effect of targeted on the untargeted, which is statistically significant, but not significantly different between high- and low-education households (supplementary materials, section X, and fig. S9). Looking at the subgroup of 33 outcomes for which there is a direct effect, we also find that there is a positive interaction between education level and the effect of the intervention on the untargeted, which is significant for both high- and low-education households; but, on this subset of outcomes, the interaction effect is stronger than the effect found in the analysis of the 117 outcomes, and the interaction for high-education households is also significantly larger than it is for low-education households (fig. S9). In other words, more-educated villagers are able to adopt and spread exogenously introduced outcomes to unexposed covillagers more readily.

Discerning social contagion beyond dyadic ties

Finally, we sought to quantify the geodesic (i.e., shortest possible distance) reach of spillovers

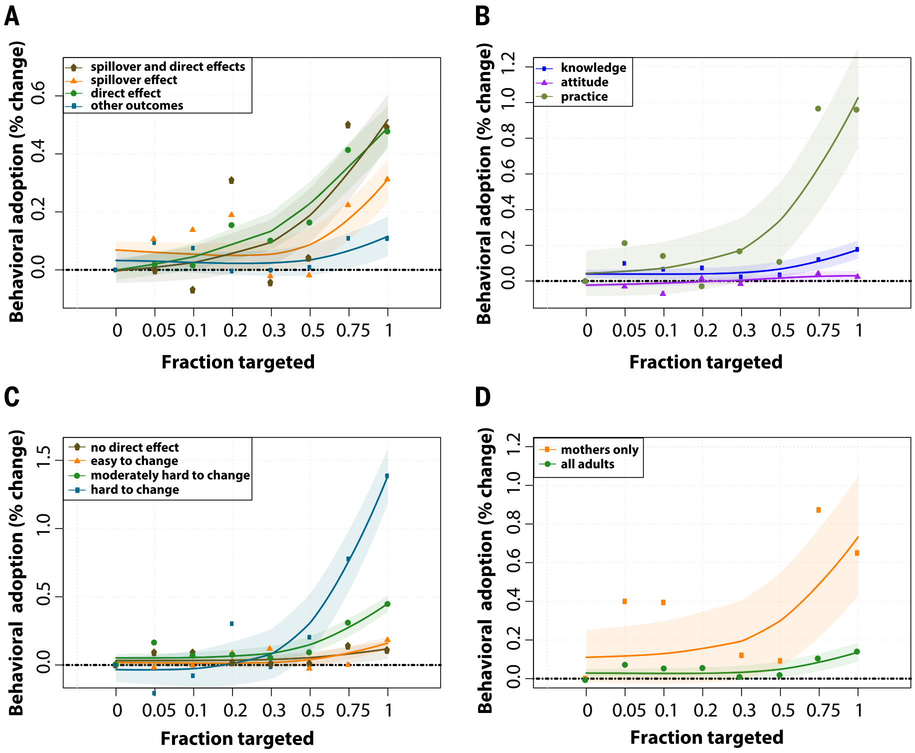

Fig. 4. Dose-response impact of targeting fraction by outcome type. In all cases, the behavioral adoption on the *y* axis is quantified as the log odds of the change in adoption over time for targeted versus nontargeted households (combining both random and nomination-targeting groups and controlling for target degree and other covariates using an additive model; see supplementary materials, section V, for details). (**A**) Effects on outcome measures according to four (partially overlapping) categories: outcomes for which the direct treatment effects are significant (green circles, *N* = 33), outcomes for which the spillover effects are significant (orange triangles, *N* = 49), outcomes for which both the direct effect and the spillover effect are significant (brown pentagons, *N* = 13), and outcomes lacking a direct effect (blue squares, *N* = 68). Higher targeting fractions are most helpful when there is a direct effect of the intervention in the individuals who get it. (**B**) Effects on knowledge (blue rectangles, *N* = 68), attitude (purple triangles, *N* = 5), and practice (green circles, *N* = 44) outcomes, separately. Higher targeting fractions are most helpful in inducing changes in outcomes for the practice outcomes. (**C**) The 33 outcomes for which there was a direct effect, organized into those that are easy (orange triangles, *N* = 11), medium (green circles, *N* = 11), and hard (blue squares, *N* = 11) to change at the individual level. Also shown are the 84 outcomes for which there was no direct effect (brown pentagons). Higher targeting fractions are more helpful in hard-to-change outcomes. (**D**) Outcomes are dichotomized according to whether they are relevant narrowly to just new mothers (e.g., breastfeeding practice) for *N* = 39 outcomes (orange squares) or broadly to everyone (e.g., diarrhea knowledge) for *N* = 78 outcomes (green circles). Higher targeting fractions are more helpful when the outcome is relevant to a smaller subset of the population.

within the villages. Namely, we consider the effect from immediate neighbors (i.e., one hop away in the network) versus further-away neighbors (i.e., two or more hops away). To quantify the strength and pervasiveness of spillover effects at a given geodesic distance, we computed the fractions of outcomes where the model with spillover effects at that distance is significant relative to a model with spillover limited to shorter distances (see supplementary materials, section VIII, for analytic approach involving nested linear models). For instance, consider the 44 practice outcomes (Fig. 5). Of these outcomes, 10 show significant total and direct treatment effects, 11 show a significant spillover effect from immediate neighbors, and 10 show a significant spillover effect from further-away neighbors (see fig. S7 for more results).

Discussion

Inducing social contagion with network targeting

This randomized controlled trial with several uncommon features demonstrated the experimental induction of varying degrees of social contagion for a diverse set of outcomes in face-to-face networks in isolated villages. We find that a meaningful improvement in maternal and child health outcomes can be achieved by deploying friendship-nomination targeting with an educational intervention of the kind that is typical of public health and development economics practice. This effect manifests a threshold, however, which in turn varies by outcome, which means that different fractions of seeds needed to be used for different outcomes of interest, depending on how hard the outcome is to modify in the first place or depending on whether a change in knowledge, attitudes, or practices is the objective. We find that easier-to-adopt outcomes at the individual level also spread more easily at the inter-individual level, that knowledge may spread more easily than practices, that the modification of practice outcomes benefits relatively more from higher targeting fractions, and that more-educated people induce greater spillovers. Finally, we were able to discern the impact of an exogenous educational intervention to two degrees of separation—on a person's friends' friends.

Social networks amplify the information and behaviors with which they are seeded (*40*), but the extent and nature of this effect may depend on the innovation being transmitted. For instance, the intrinsic complexity of understanding or implementing a new behavior, the visibility of its results, its perceived advantage over existing methods, and its baseline prevalence can all influence adoption patterns. Straightforward information (such as knowledge about pregnancy danger signs) might spread by so-called simple contagion, requiring only a single contact for transmission between two individuals. Deeper behavioral changes (such as going to a health facility to deliver a baby), by contrast, might require reinforcement from multiple social contacts (e.g., through complex contagion) (*41*), perhaps because they require significant motivation or because they require changes in long-standing beliefs and practices.

Although this randomized controlled trial was not designed to test details of complex contagion theory, our results can still be examined from this perspective. For instance, friendship targeting leads to more-clustered seeds (i.e., seeds that had a shorter geodesic distance from each other; supplementary materials, section IV, and table S3), and clustered seeding should be better for triggering complex contagion (given the need for local reinforcement) (*20*). But, in general, we find that friendship-nomination targeting is more likely to outperform random targeting for the knowledge-based contagions and for the easier-to-spread contagions. However, nomination targeting has multiple implications for the topological attributes of the nodes that are thereby selected, beyond greater clustering (or greater transitivity, etc.). For instance, nomination targeting also identifies seeds with higher degree, which is expected to enhance their ability to spread simple (e.g., informational) contagions. And, regardless, many easy-to-spread contagions could still be complex, at least in part. Finally, the detailed structure of the network around the targeted persons may also matter for diffusion (*16*).

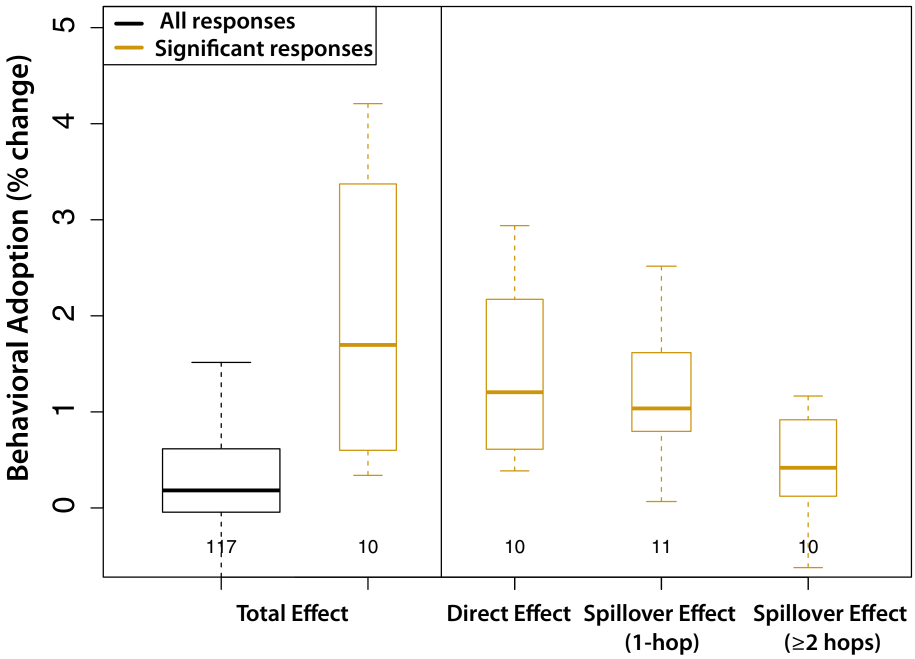

Fig. 5. Magnitude of the causal effects of intervention at various geodesic distances. The *y* axis is the change in adoption due to targeting, expressed as the log odds of a change in outcomes over time. (**Left**) Total effect of the intervention; within this section, the left boxplot (black) summarizes the effects in all the outcomes (N = 117), whereas the other boxplot (orange) summarizes the distribution of the effects specifically for the practice outcomes with a significant total effect (N = 10). (**Right**) Three components of the total effect of treatment—namely, the direct effect (seen in N = 10 outcomes), the indirect effect from neighboring households one hop away in the network (seen in N = 11 outcomes), and the indirect effect from neighboring households more than one hop away in the network (seen in N = 10 outcomes). The impact on an ego of intervening in an alter declines with the geodesic distance of the alter to the ego.

For instance, when convincing individuals to change their practices, it may be more important that they receive the information from multiple geodesic directions than that they receive the information multiple times from a similar direction (*42*). Exploring these ideas in bespoke field trials is an area for future work.

Beliefs and behaviors related to health are often socially reinforced and can therefore be difficult to change, particularly in traditional cultural settings (*43*). And social contagions facing greater resistance can require a greater critical mass of initial converts (*44*). In LMICs, people often rely on informal village networks as sources of information and support, especially in matters related to pregnancy, delivery, infant care, and postpartum health. Hence, whether a person responds to an intervention may depend on whether others around them do so. If the innovation is in direct opposition to an ingrained norm, community members may sanction others for adopting the new behavior, and the proportion of the population that must be exposed to overcome that resistance might be higher. 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. As we have seen, however, this threshold might vary according to the nature of the thing that is spreading or the kinds of people (e.g., those with higher degree identified with the friendship paradox or those with higher education) who initially adopt (*45*).

Limitations

This work has limitations, including that it was conducted in a single country and that the intervention was specific to maternal and child health. Other studies have used network-based targeting to focus on microfinance in India (*5*), vitamins in Honduras (*8*), iron-fortified salt in India (*10*), agricultural technology in Malawi (*9*), after-school tutoring in Bangladesh (*46*), or bullying in the USA (*11*). However, prior work has generally not varied the targeting fraction or evaluated such a broad range of outcomes in parallel, nor has it necessarily focused on the easy-to-implement friendship paradox algorithm. Another limitation is that not all residents of the villages enrolled in our study, and the 18.8% who did not might have been different from those who did in ways beyond attributes like gender (as noted), which might have biased the results. Furthermore, despite randomization and a large sample, the data were noisy. Finally, like other complex interventions that might affect many outcomes (*47*), it is possible that the complex, multifactorial nature of the intervention and of the outcomes that it affected could create complications for behavior change (e.g., the adoption of one practice may have affected the probability of the adoption of another). These sorts of potential interaction effects are another area for future work.

Conclusions

The friendship paradox–based targeting strategy that we assessed can be deployed without having to map the entire network and so is scalable—in the sense that it can be used easily and without the expense or delay of collecting sociocentric network data. The strategy could also potentially be enhanced—for example, by asking people to nominate a particular contact who they believe might be better able to spread innovations, or a contact who they know to have many friends, or by taking two steps in the network to a person's friends' friends—instead of simply picking a contact of theirs at random, as we did in this work. These would be more complicated approaches, and evaluating other scalable modifications to the friendship-nomination algorithm is promising (*10*).

Social interventions often seek to target all members of a relevant population, such as a village, school, or firm. However, face-to-face counseling for information provision or behavior change takes time and resources. Yet, social network–targeting methodologies could mean that intervening in smaller fractions of the population could have the same effect as targeting 100% of the population (*48*). Deploying interventions through network targeting, without increasing the number of people targeted or the expense incurred, may enhance the adoption and spread of the interventions and thereby improve human welfare.

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SUPPLEMENTARY MATERIALS

science.org/doi/10.1126/science.adi5147
Supplementary Text
Figs. S1 to S9
Tables S1 to S6
References (*50*–*61*)
MDAR Reproducibility Checklist

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