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SUPPLEMENTARY ONLINE MATERIAL

Induction of Social Contagion for Diverse Outcomes in Structured Experiments in Isolated Villages

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§I Local involvement in the research

In keeping with proper standards for such research, we worked closely with the local population of Copan, Honduras (in the western highlands near the Guatemala border), sought feedback and approval from officials at the Ministry of Health (MOH) of Honduras, and endeavored to provide practical benefits to the local community. Here we briefly summarize this history and outline some of our principles and actions in this regard.

When we began designing this cohort project in 2013, the Bill and Melinda Gates Foundation (BMGF) introduced us to the Inter-American Development Bank (IDB), which has been supporting and doing work throughout Latin America, and IDB in turn introduced us to the Honduras MOH. After the cohort was established, we obtained additional funding from other sources as well. Because of this pathway to getting the project launched, we worked with local and regional public health organizations and with local leaders rather than with local academic institutions.

From the outset when the original underlying cohort for this study was impaneled (in 2013), we sought extensive local involvement, beginning with a needs assessment where local village residents told us about topics of concern to them in a series of meetings in villages throughout Copan. In addition to extensive community input, we sought input from the MOH. We periodically briefed both the community and the MOH about our findings (though we did not reveal the actual RCT results to the community until after completion of the trial).

Copan is a very isolated area. Over the years, as we built our data collection team in Copan (the intervention delivery team was wholly different, as discussed below), we developed deep ties to the local community, to local village leaders, to the few local health clinics, and to local transportation and infrastructure providers. Because of these ties and our commitment to the local community, we presented our results directly to these constituencies at proper intervals. We also held two joint implementation science conferences with our Honduras and Yale teams (involving many dozens of attendees).

We provided other material benefits to the local community, beyond simply providing them with information. When people were tested for parasites as part of our study, we gave them the results of their tests and arranged for them to be treated. When people had their vision tested, we provided corrective glasses. We solicited ideas from the local community about what infrastructure improvements we could make, and we repaired many local playgrounds and clinics as a result (a detailed summary is available upon request). We arranged for an American company to provide free portable handheld ultrasound devices to the local health clinics, which was much appreciated by local providers. In terms of capacity building, we hired and trained over 100 local people and built capacity in the region; and many of our former data collectors have gone on to work for other public health and development entities. Lastly, we offered a talented young person from Copan a slot as a PhD student in the USA.

Throughout our work in Honduras and given the extent of local involvement at the regional and MOH levels, we endeavored to act with integrity, curiosity, and respect in all relationships.

Finally, we note that this research would not have been prohibited in the USA. This work is not likely to result in stigmatization, incrimination, or discrimination for the participants, and we have carefully safeguarded all data from threats to the privacy or security of our participants, which has constrained the individual-level data released as part of this research.

§II Experimental design

Our study design is more fully detailed in our published protocol.(*33*) This RCT was also registered with ClinicalTrials.gov, number NCT02694679.

We first conducted a complete census in each of the 176 villages in our cohort, and then sought consent of the subjects to participate in the study. Government figures suggested that perhaps a total of 32,800 adults lived in these villages, and most of them (it would seem) agreed to be in our de-novo census, for a total of 30,422 who agreed to be censused by us. Of these, in turn, 24,702 – that is, 81.2% – agreed to participate in our long-term study. See the CONSORT diagram in **Figure S1**, which provides details regarding enrollment in the overall study and randomization to the 16 arms of the study, along with dropouts before and after randomization in each arm – none of which deviated from usual standards.

We only have limited data about those who participated in our census and refused to participate in the study. There was no difference by marital status (58% of participants were in a marriage or civil union, and 58% of non-participants, $p=0.92$) or by age (mean age of participants was 32.8 years old and of non-participants was 32.6 years, $p=0.44$). However, as is typical (especially in longitudinal health surveys), participants were more likely to be female (58% of participants were female versus 35% of non-participants, $p<0.001$); yet, because enrollment overall was relatively high, at 81.2%, the percentage of females in the sample (58%) was not as different from the percentage in the overall censused population (54%).

We then carried out essentially 176 RCTs in an analogous number of villages as part of our overall RCT, structured into two levels of randomization (**Figure S2**). The experimental units at

the first level were the 176 villages, assigned to 16 treatment arms, organized into a 2 x 8 factorial design – two household targeting methods (random selection and friendship-nomination selection) and 8 fractions of households targeted (0%, 5%, 10%, 20%, 30%, 50%, 75%, and 100%). The experimental units at the second level, within a village, were individual households. There were 11 villages in each cell. Note that, at the extremes of 0% and 100% targeting, the cells were degenerate, and we treat the 22 villages in those cells as a coherent set.

To obtain balance between cells in our randomized design, we first created 11 blocks of 16 villages that minimized the within-block variance in (1) the number of households per village and (2) the average number of subjects within the households.(*50*) Once the blocks were assigned, we sampled cell assignments without replacement within each block so that each block yielded exactly one village in each cell of the 2 x 8 design.

In pilot studies, we found that blocking on these two values effectively created balance on a number of variables based on difference of means tests for the friendship-nomination condition versus the random condition evaluated by simple linear regression. These variables included latitude, longitude, elevation, time to nearest health center, time to nearest maternal clinic, total population, household physical condition, household overcrowding, child schooling, economic dependence, age, and sex. We then uniformly sampled treatment allocation vectors of villages to cells in this 2 x 8 design, in the restricted randomization space, in order to promote balance, and we picked one at random.(*50-53*)

We checked for possible imbalance due to "failure of randomization," among a set of 16 covariates, including sex and age, which are used as illustrations here. The 149 contrasts we consider consist of all joint and marginal cell pairs in our 2x8 two-stage experimental design (i.e., two targeting nomination algorithms versus eight fractions of households targeted). That is, these 149 contrasts arise from 120 joint contrasts (i.e., 16 choose 2) and 29 marginal contrasts (i.e., 8 choose 2, plus 1). Out of 2,348 balance checks (149 contrasts for each of 16 covariates), one failed at the 5% significance level. Namely, when looking at the contrast between the 11 villages with 10% of the households targeted using random-nomination and the 11 villages with 50% of the households targeted using friendship-nomination, the t-test for balance of the covariate "age" fails with a p-value of 0.039. We did not correct these p-values for false discoveries, since, for this analysis, allowing more rejections is conservative. Upon inspection, the reason for this lack of balance relates to some component villages in this set having a higher-than-usual average age.

We collected data from the subjects in waves, as shown in **Figure S3**, and we used data from waves 1 and 3 here.

Once villages were assigned to the 2 x 8 treatment arms, in each village, we targeted households for the intervention as follows. For villages in the "random" targeting arm (comprised of 8 cells corresponding to the 8 targeting fractions), we sampled without replacement the number of households indicated by the village's dosage assignment, rounding to the nearest whole number. For example, in a village with 37 households assigned to a dosage of 20%, we randomly chose 7 households for treatment. For villages in the "friend" nomination arm, as a first stage, we sampled without replacement the number of "seed" households indicated

by the village's dosage assignment, rounding to the nearest whole number. These seed households were not, however, necessarily assigned to treatment. Instead, in a second stage, we randomly chose one subject from the household and randomly chose one of that subject's social contacts who did not belong to the same household as the subject. We then assigned that social contact's household to the intervention. In the event that the subject had no social contacts (according to three name generators, as discussed below) outside the household or the social contact's household had already been assigned to treatment, we sampled another household without replacement from among those villages that have not yet been sampled in the first stage, and repeated the procedure of choosing a randomly selected subject within the household and randomly choosing one of their social contacts' households for treatment. This procedure was repeated until the number of households treated within the village corresponded to the randomly assigned dosage.

We binarized and symmetrized the networks corresponding to three name generators (the "free time," "personal talk," and "closest friend" ties, as discussed below – see **Table S1** for name generators) where households were the nodes. And we performed sensitivity analyses on various types of networks that relaxed the binarization and symmetrization procedures (not shown here).

After the randomization procedure was completed, we provided our delivery partner (the Inter-American Development Bank and their on-the-ground implementing partners) a list of households to receive the 22-month intervention. Neither IDB nor the implementing partners knew which villages belonged to the random targeting arm and which to the friendship-nomination nomination arm. However, while we did not explicitly reveal which villages were receiving what dosage, that information could be inferred from observing approximately what percent of households were receiving the intervention. A separate team of data collectors assessed outcomes at baseline and follow-up, wholly independently from the intervention delivery team.

§III The health intervention and intention to treat

The community health workers spoke to families (mothers, fathers, grandparents – whoever was in the household) regarding several health topics, depending on current life circumstances, based on the "Timed and Targeted Counseling" methodology complemented with other methods of face-to-face communication including songs, rhymes, and riddles.(*54*) The social and behavior change communication strategy for the intervention was designed using the "P-Process." 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. This method provides counseling to all members of selected households whether or not there were young children or a pregnant woman living in the household. Because of this tailored approach, some intervention households may have received different modules at different times and at different frequencies. But all this was done entirely consistently across all sixteen arms of the RCT.

Overall, 83% of target households received at least one counseling visit (across targeting methods and dosages). Out of a maximum of 22 counseling visits, there was a median of 18

completed visits and a mean of 14 completed visits per target household. We use 15 visits as a cut-point to assess what fraction of households in each village, regardless of treatment assignment, actually got the intervention (e.g., 10% of households may have been targeted, but only 8% might have received 15 or more visits). **Table S2** tallies the average number of visits in villages with a given fraction of the households targeted.

§IV___Network features of friendship-nomination and random-nomination targeting

Here, we provide some illustration of the ways in which nodes selected with friendship-nomination and random-nomination targeting differ, in terms of a number of network features.

We first illustrate the greater centrality of households chosen by friendship-nomination in our actual data by using the villages with a small targeting fraction (5% and 10%) – because, as noted in the main paper, as the targeting fraction gets higher, there is necessarily less and less difference between friendship targeting and random targeting. Hence, taking the 22 villages with friendship nomination at 5% and 10% fractions, and the 22 villages with random targeting at 5% and 10% fractions, we can compute the average in-degree, out-degree, total degree, betweenness centrality, eigenvector centrality, and transitivity of all these chosen nodes, and compare the two types of nodes.

We first computed a correlation matrix among these features, and with the friendship targeting assignment vectors, within villages that share the same fraction of households targeted. The relationship between the binary variable of friendship-nomination targeting and the other real-valued measures of centrality was summarized by a logistic regression using friendship-nomination targeting as the dependent variable and the respective centrality measures as the independent variable. As is known from much past work, most centrality metrics are indeed correlated with one another, except for transitivity, which is a measure of local density.(*55*) As expected, we find that friend targeting assignment vectors are correlated with the centrality metrics (i.e., looking at the slope of the logistic regression and its significance). Interestingly, we find that friend targeting assignment vectors are also highly correlated with transitivity.

We also examined the geodesic distance among households that were targeted by random-nomination and friendship-nomination targeting, compared to those households that would have been targeted as highly central using a simple centrality measure (such as having the highest degree). That is, we quantified the mathematical necessity that a set of nodes chosen by friendship nomination are necessarily closer together than those chosen at random. For instance, in the villages with 10% sampling, the average *minimum* geodesic distance (normalized by village size) between pairs of all chosen nodes in the 11 randomly targeted villages was 1.58 and in the 11 friendship-nomination villages was 1.29. By contrast, the average minimum geodesic distance between pairs of the most central nodes in these 22 villages would be 1; that is, among the set of the most central nodes, each node is connected directly to at least one other member of the set. **Table S3** summarizes these results.

Hence, nomination targeting has multiple implications for the topological attributes of the nodes that are thereby selected. To be clear, these considerations should mainly be relevant at the lower targeting fractions, since, as noted above, as the fraction of seeds gets larger, the actual

topological differences between sets of nodes chosen by the two methods necessarily declines. Nevertheless, we also find that, in our data, the differences in topological properties between friendship and random targeting nodes do persist even at higher fractions of households targeted, when *comparing targeted to untargeted households* using the two methods. To clarify how nodes selected based on the friendship paradox indeed have different topological properties than random nodes in our sample, we plotted the degree distributions (i.e., estimated densities) for friendship-nomination and random-nomination targeted individuals, in villages at three dosages of targeting, comparing targeted to untargeted nodes. As shown in **Figure S4**, there are no differences in the randomly targeted villages, as expected (that is, in those villages, there is no difference in the topological properties of randomly selected "treated" nodes and non-selected "control" nodes). But, in the villages with friendship nomination, the curves are shifted, as expected (that is, friendship nomination nodes do indeed have higher degree).

Finally, we plotted the estimated densities for *other* measures of centrality, namely, in-degree, out-degree, eigenvector centrality, betweenness centrality, and transitivity for the villages with the 50% targeting fraction, as shown in **Figure S5**. Once again, friendship nomination is shifted in the predicted direction.

In summary, as theorized (*22*) and as previously demonstrated,(*23*) nodes chosen by the friendship-paradox-based nomination targeting technique generally have higher degree and centrality. Seeds chosen by friendship-nomination are also geodesically closer together and have higher transitivity. Nodes chosen by this friendship-paradox-based nomination method may also have certain other (appealing) topological properties, including being spread out in the network (compared to nodes chosen based on degree), and may have personal attributes (e.g., being wealthier) that are also possibly advantageous. Of course, nodes chosen via friendship-nomination have the *crucial* advantage of being identifiable without the delay and expense of mapping the whole network (which metrics like centrality or in-degree would require).

§V Estimated causal effects and notion of significance

We quantify change in adoption due to targeting (via either random or nomination targeting) as the log odds of the change in outcome over time for targeted compared to non-targeted households. We first compute the change in outcome over time (for a targeted, or a non-targeted, household) as the ratio between outcome at follow-up and outcome at baseline two years earlier for that household (i.e., percent of correct responses in the survey over villagers in that household, for any given outcome). Then we compute the change due to targeting as the log odds of the behavioral change over time comparing targeted to non-targeted households. In all the primary analyses, we denote these log odds for household i as Y_i, for each of the 117 outcomes separately. To simplify notation, we do not add a superscript that indicates the outcome variable name.

Our primary analyses regarding the *total effect* of the friendship paradox-based nomination targeting treatment leverage additive models for Y_i, where we do not control for the degree of the nodes in the network. The households were randomization units within the villages, and so we randomly chose the nodes (and did not base the choice on the number of connections each household had). Hence, to start, in **Figure 2**, the adoption rates on the Y axis are computed as

log ratios of the adoption between t_0 and t_3, Y = log(percent_correct_t_3/percent_correct_t_0), which gives the percent increase or decrease, and we report the median for this quantity for the 11 villages in each cell.

However, since friendship-nomination targeting is meant to leverage the topology of the network, for some models that estimated the direct effect of the intervention upon those to whom the intervention was given (e.g., in **Figure 4**), we needed to control for the contribution of topology (e.g., degree) in estimating this causal effect. That is, the analysis of the *direct effect* of the intervention on just the intervened-upon households controls for degree because, when the whole sample of villages is used (i.e., those in which households were chosen at random combined with those in which they were chosen with the friendship-nomination method), this is necessary to avoid confounding due to the topology of the network in estimating these particular causal effects.

In addition, as noted above, we performed sensitivity analyses using model-free estimators (e.g., difference in means). These primary analyses summarize effects at the village-level as described in the main manuscript, rather than at the individual household level.

Table S4 summarizes all the effects we consider in the main analysis and related sensitivity analyses (some not reported here), together with the estimators we used, and the number of outcomes for which the effects were found to be significant at standard levels (p-value < 0.05). The interpretation of parameters is consistent across models.

Below, we describe the effects that support the primary analyses in more detail.

§VI Testing global nulls of no effect, and a relevant (direct effect) subgroup

We started by testing global nulls of no effects. There are two sorts of global nulls to evaluate (i.e., regarding whether the intervention worked, and whether our "treatment," which was friendship-nomination targeting, worked, overall).

Regarding the intervention itself (i.e., the 22-month program of education), a higher criticism analysis (i.e., comparing the theoretical T distribution under the global null of no effect versus the T distribution of the 117 realized effects, with a KS test) rejects the global null of no-effect (p-value < 0.001). In addition, in order to avoid calling the difference between these two CDFs significant when such an assessment might be a function of the sample size (i.e., theoretical continuous T CDF versus an empirical CDF based on 117 points), we also approximated the theoretical T distribution under the global null with an empirical CDF based on 117 samples from the theoretical CDF, thus matching the uncertainty in the empirical CDF of the realized effects, which is also based on 117 data points. We then compared this approximate T distribution under the global null to the empirical CDF based on the realized effects, repeated this procedure 1,000 times, and recorded the corresponding p-values. The summary of these p-values is as follows: mean = 0.00192, median = 0.00029, 1st quartile = 0.00005, 3rd quartile = 0.00142, min = 0, and max = 0.089 – thus reinforcing evidence that we can safely reject the global null of no effect, with a conservative higher criticism analysis. And Tukey's method to test the global null of no effect (i.e., comparing the fraction of significant outcomes one can

expect due to chance, 5.85% [i.e., 5% x 117], against the empirical fraction of significant outcomes, 28.2%, with a binomial test) also rejects the null of no-effect (p-value < 0.001).

In short, with the above findings, we can reject a global null of no effect of the intervention (i.e., the 22 month program itself) on knowledge, attitudes, and practices in the population of villagers (i.e., on the 117 measured outcomes overall, as a group).

Next, we test whether the "treatment" here, *which is the network targeting algorithm itself*, also allows us to reject a global null of no difference in efficiency across friendship-nomination and random targeting arms for all outcomes combined (as also documented in **Figure 2a**).

Of the 117 outcomes, there are 113 outcomes (96.6%) where the adoptions from friendship-nomination and random targeting near-achieve the adoption of the households in the 100% treated villages (i.e., the maximal adoption achieved by friendship nomination and random targeting is very similar to the adoption we observe in the villages where *every* household is treated). Tukey's method fails to reject the null that there is no difference in *maximal adoption* between friendship nomination, random nomination, and the households in the 100% treated villages (p-value < 0.01 for the binomial tests on all the three pairs).

Looking at all 117 outcomes, based on an intention-to-treat analysis, friendship-nomination targeting is discernably more efficient than random targeting for 34 of them. Both a higher criticism analysis and Tukey's method reject the global null of no effect (p-values < 0.05 for both tests). In addition to the intention-to-treat analysis, we also performed an analysis that takes into account the *actual delivery* of the intervention (as there was heterogeneity in this regard). Using the fractions of households receiving at least 15 (out of 22) visits as a basis for saying a household was "treated," nomination targeting is more efficient for 93 of the 117 outcomes. Both a higher criticism analysis and Tukey's method reject the global null of no effect (p-values < 0.01 and <0.001, respectively).

In short, with the above findings, we can reject a global null of no difference in efficiency across friendship-nomination and random targeting.

We also looked at the effect of treatment on the *untreated households* only, for the set of the 113 outcomes for which behavioral adoptions from friendship-nomination and random targeting near-achieve the adoption of the households in the 100% treated villages – ignoring variation due to FDR thresholding that identifies the 113 outcomes, in this instance, though the threshold we use is conservative, as noted. Repeating the two tests above for the global null of no improvements, in efficiency, on this subgroup, we reject the global null of no effect (p-values <0.01, for both the KS test and the binomial test).

For those 113 outcomes, the average improvement in behavioral adoption from friendship-nomination targeting compared to random-nomination targeting is 1.47% (p-value < 0.01, with a paired t-test) for the untreated households, with a range between 0.89% and 2.04%. As noted, for a minority of the outcomes, there is a loss of efficiency; just for the sake of curiosity, if we focus only on the 87 outcomes where there is a gain in efficiency, the average improvement in

behavior through friend nomination over random targeting is 3.26% for the untreated households, with a range between 2.68% and 3.83%.

An important subgroup of the outcomes that we leverage in most of our analyses if the set of 33 outcomes, for which there is evidence of a direct effect (i.e., response to the intervention in the intervened-upon households). As detailed in the **Table S5**, we have performed this type of selection using multiple estimation methods, which suggest as many as 40 outcomes might display a significant direct effect, but we have chosen to carry out the main analyses using a set of 33 outcomes with significant direct effect, to be conservative. In the pre-registration paper, we describe what we termed there as the "social effect" as an add-on mechanism that enhances the primary effect of the intervention, when it is present.(*33*) These effects translate into the "indirect" and "direct" effects quantified in this paper, respectively – and they are distinct from the efficiency gain effect that friendship-nomination targeting offers over random-nomination targeting, which is the focus of the present work. That is, focusing on the subgroup of those outcomes for which the intervention has a direct effect is supported by theory and was a part of the design plan. However, we could not know in advance which of our 117 measures would in fact be affected by the intervention (i.e., have a "direct" effect). That is, while we did pre-specify our conceptual concern, and had to wait to test, empirically, which outcomes had direct effects among intervened-upon individuals. Nevertheless, we report findings for all 117 outcomes, in addition to the various subgroups.

We also tested this mechanism empirically, via a regression analysis assuming errors in variables, where each outcome has a regression weight that is inversely proportional to its estimation error.(*56-61*) In **Figure S6**, the magnitude of the dots is proportional to the standard error of the estimated direct and indirect effects, rather than the sample size of the relevant populations, which is also accounted for in the regression analysis. The relationship between direct and indirect effects is positive (slope = 1.29, p-value = 0.018) among those outcomes with a significant direct effect, while the relation is not significantly different from zero (slope = -1.33, p-value = 0.1) among the outcomes without a significant direct effect.

In sum, the existence of theoretical as well as empirical support, combined with the multiple estimation methods we employed, and our conservative reporting choices, make this subgroup analysis (of N=33 outcomes with a direct effect) valid and its results tenable.

§VII Estimating direct treatment effects and total spillover effects

We used the following model to estimate the direct treatment effect:

$$Y_i = \alpha + \tau\, Z_i + \beta\, S_i + \delta\, D_i + \epsilon_i \qquad (i)$$

Here, Y_i are the log odd of adoption for household i, Z_i is the indicator of whether household i received the intervention, S_i is the amount of spillover household i is exposed to, which is computed as the fraction of treated neighbors (in the neighborhood N_i), $\mathrm{S_i} = \sum_{j \in N_i^h} Z_j / D_i \in [0, 1]$, and D_i is the degree of household i, $D_i = \#(N_i)$. As for the parameters, α is a baseline

effect, τ is the direct treatment effect, and β is what we term "total" spillover effect, to qualify it and distinguish it from spillover effect at varying geodesic distances, which we describe next.

Note that this particular model controls for the degree, D_i , to avoid confounding potentially induced by our experimental design, since the households are randomized but the degrees are not. That is, by design, households identified by nomination targeting have higher degree.

§VIII Estimating spillover effects at varying geodesic distances in village social networks

We estimated the reach of spillover effects in a village by estimating the spillover effects to individual *i* from neighbors one hop away, two hops away, and three hops away in the social network (as in **Figure S7** and **Figure 5**). We used the following model to estimate these spillover effects:

$$Y_i = \alpha + \tau Z_i + \psi^1 S_i^1 + \delta^1 D_i^1 + \psi^2 S_i^2 + \delta^2 D_i^2 + \psi^3 S_i^3 + \delta^3 D_i^3 + \epsilon_i \qquad (ii)$$

where Y_i the log odd of adoption for household i, and other quantities remain as above. Here, we introduce spillover effects from neighbors h hops-away with the coefficients ψ^h multiplying the amount of spillover, denoted by S_i^h , $h \in \{1,2,3\}$, which is computed as the fraction of treated neighbors (in N_i^h) h hops-away, $\mathrm{S_i^h} = \sum_{j \in N_i^h} Z_j / D_i^h \in [0,1]$, where $D_i^h = \#\left(N_i^h\right), h \in \{1,2,3\}$.

Note that the model controls for the degree at different geodesic distances, D_i^h, to avoid confounding, since the households are randomized but the degrees are not. That is, by design, households identified by nomination targeting have higher degree.

§IX Estimates of targeting efficacy by outcomes

Figure S8 gives complete data for the 117 outcomes, in parallel to the 33 direct outcomes given in **Figure 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 behavior (1) in villages that received 100% targeting; (2) in villages with varying fractions of randomly selected households; and (3) 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 **Figure 1b**). And then we estimated the fraction of households selected by 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 **Figure 1b**).

Table S5 gives more correlation coefficients, as illustrated in **Figure 3b**. We estimated the correlation, across the 117 outcomes, between the ease of expressing the outcome, on the one hand, and the differential efficiency of friendship-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. This is done for various subset of the outcomes, as indicated.

Detailed variable names for **Figure S8** are in **Table S6**.

§X Estimating behavioral change as a function of household education

We sought to quantify whether and to what extent education can modulate the effect of intervention on those not getting the intervention. The median education level is 3rd grade when we consider individuals as data points. Our analyses consider households as data points, so we quantify education at the household level by recording the *highest* education level attained by any individuals associated with a given household. With this coarser definition, the median education level becomes 4th grade when we consider the households as data points, and we label a household as "high education" if its education level is at this median or above.

We used the following model to estimate the interaction between indirect effect and (dichotomous) education level:

$$Y_i = \alpha + \tau\,(Z_i * E_i) + \beta\, S_i\ + \delta\, D_i + \epsilon_i \qquad (iii)$$

As above, Y_i is the log odd of adoption for household i, Z_i is the indicator of whether household i received the intervention, S_i is the amount of spillover household i is exposed to, which is computed as the fraction of treated neighbors (in the neighborhood N_i), $\mathrm{S_i} = \sum_{j \in N_i^h} Z_j / D_i \in [0, 1]$, E_i is the new indicator variable for high education described above, and D_i is the degree of household i, $D_i = \#(N_i)$. As for the parameters, α is a baseline effect, τ is the direct treatment effect, and β is what we term "total" spillover effect. Note that this model controls for the degree, D_i , to avoid confounding potentially induced by our experimental design, since the households are randomized but the degrees are not. That is, by design, households identified by nomination targeting have higher degree.

Figure S9 illustrates the results of this analysis, for all 117 outcomes. We find that there is a positive interaction between education level and effect of intervention on those not getting the intervention, which is significant, but not significantly different between high- and low-education households. Looking at the subgroup of 33 outcomes with a significant direct effect, we find that there is a positive interaction between education level and effect of intervention upon those not getting the intervention, which is significant for both high- and low-education households, stronger than the effect found in the analysis of the 117 outcomes, and the interaction is also significantly larger for high-education households than it is for low-education households.

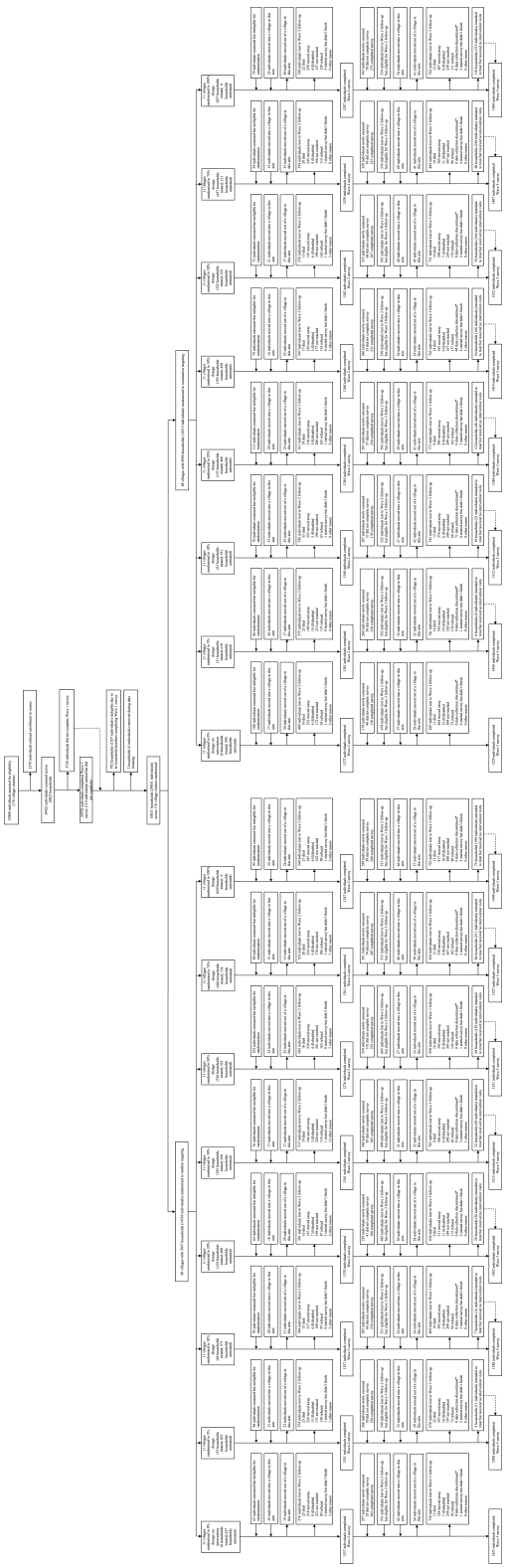

Figure S1. CONSORT chart for the RCT. Please enlarge to view.

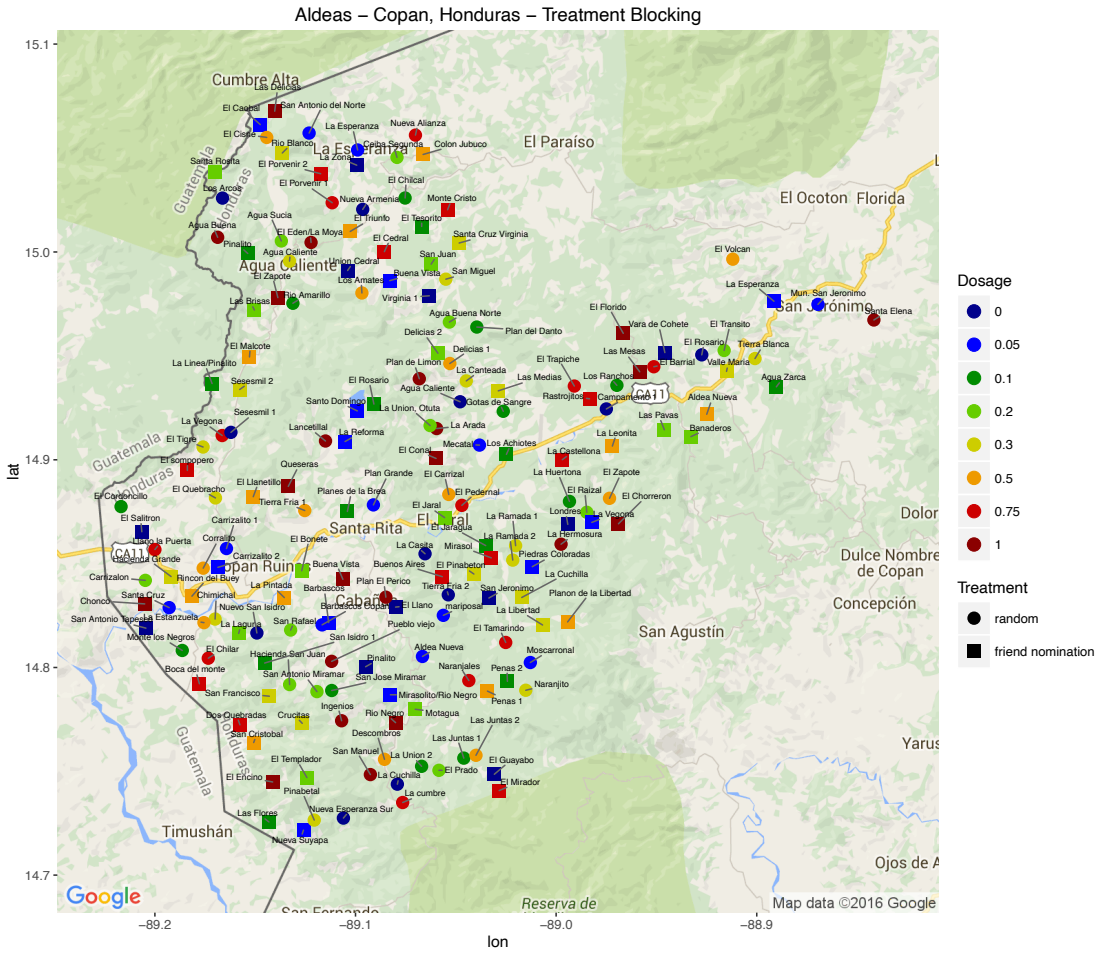

Figure S2. Map of how the village-level randomization is spatially distributed in the Copan region of Honduras.

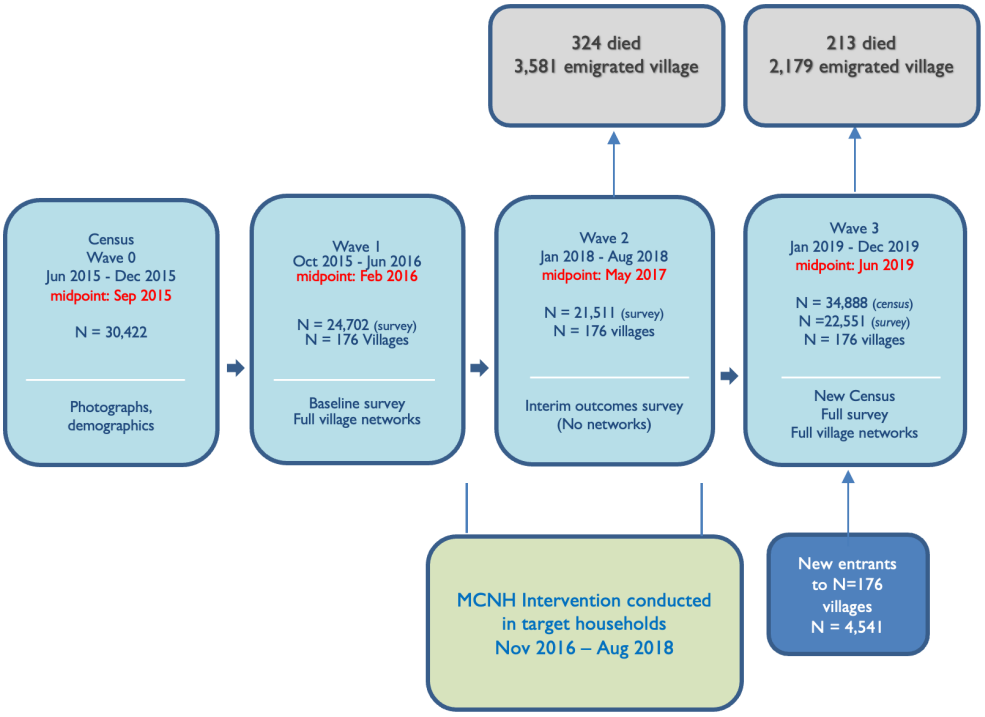

Figure S3. Outline of data collection waves, dates, sample sizes, and variable types. Sample retention is high, and >80% of subjects complete each wave. We also impanel new entrants into the villages and track births, deaths, and emigration. Note that the 22-month MCNH (maternal, child, and neonatal health) intervention was always delivered at a point starting after the baseline survey (wave 1) was completed in the particular village. And the follow-up survey (wave 3) was always collected fully after the intervention was complete.

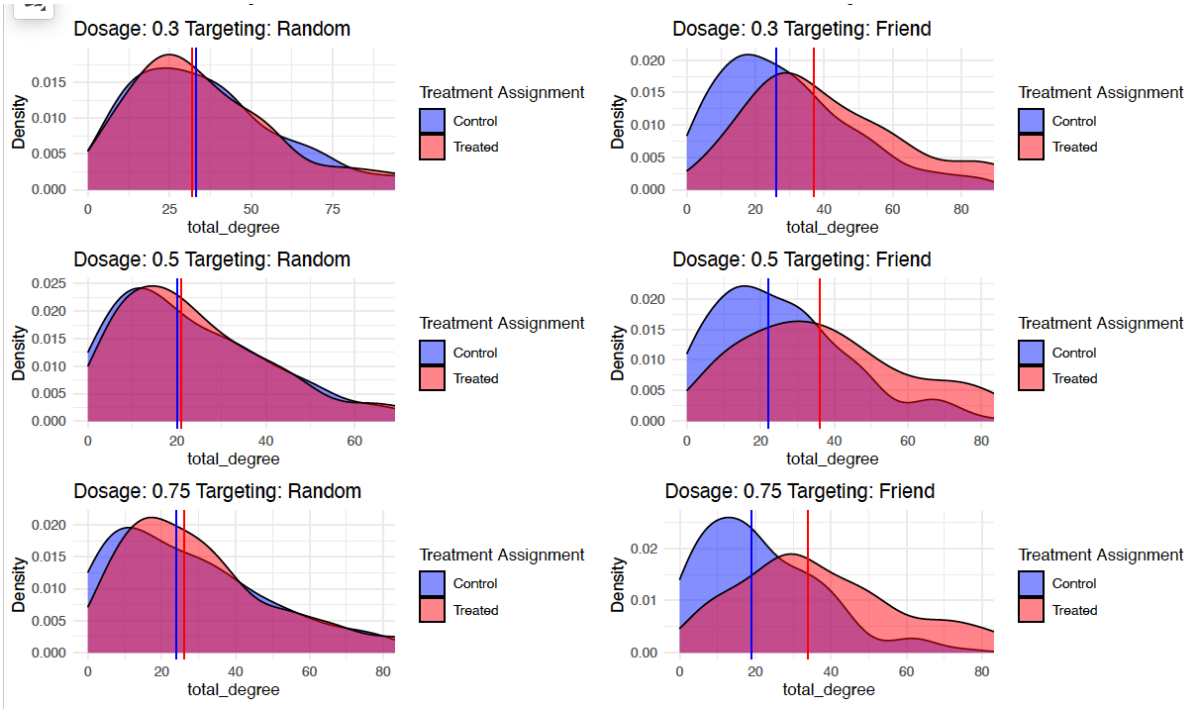

Figure S4: Each panel illustrates the empirical distributions of the "total degree" centrality measure, separately, for the intervened-upon and non-intervened-upon households within villages with the specified fraction of targeted households and targeting nomination method.

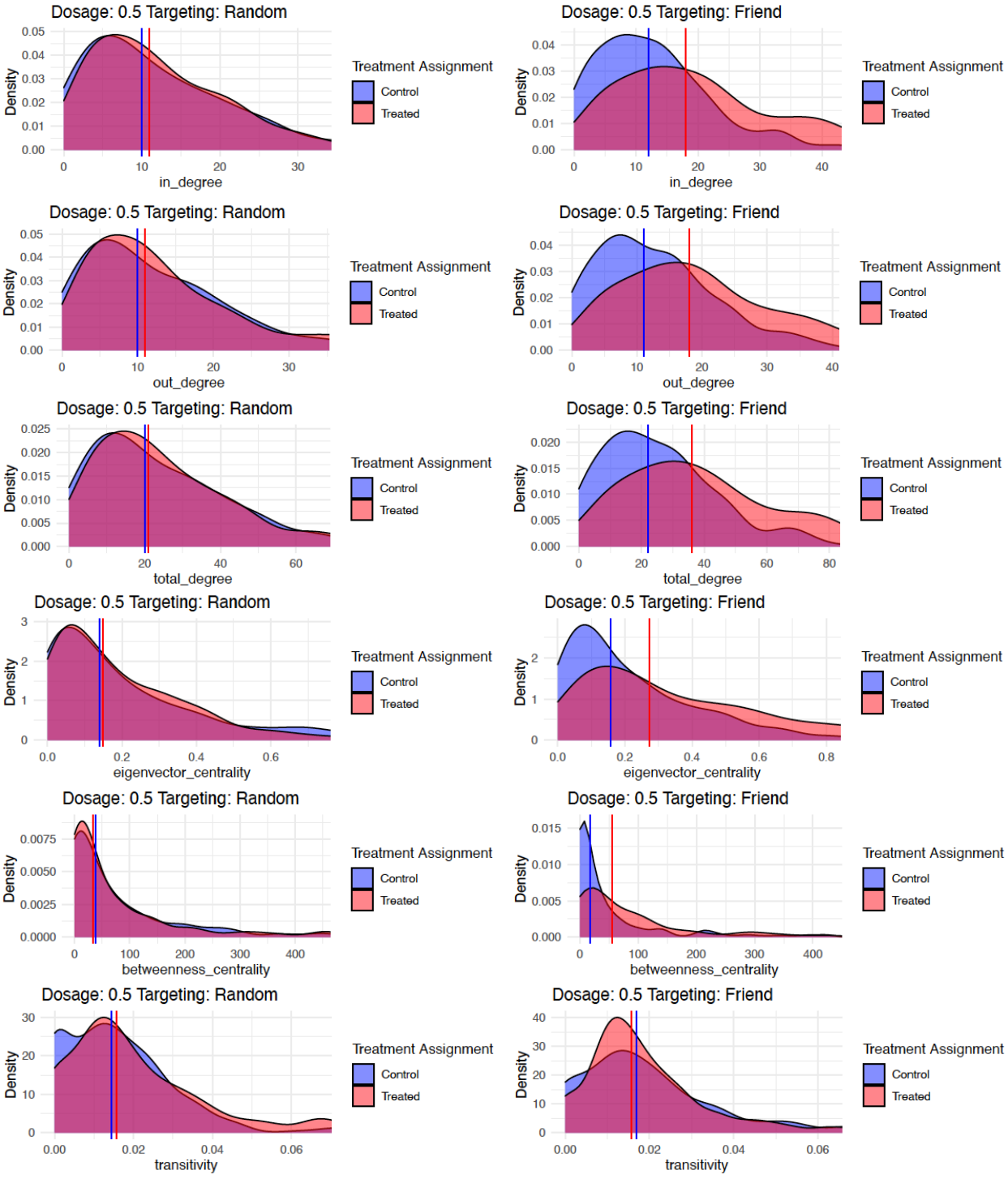

Figure S5: Each panel illustrates the empirical distributions of the corresponding centrality measures, separately, for the intervened-upon and non-intervened-upon households within villages with 50% targeted households and the specified targeting nomination method.

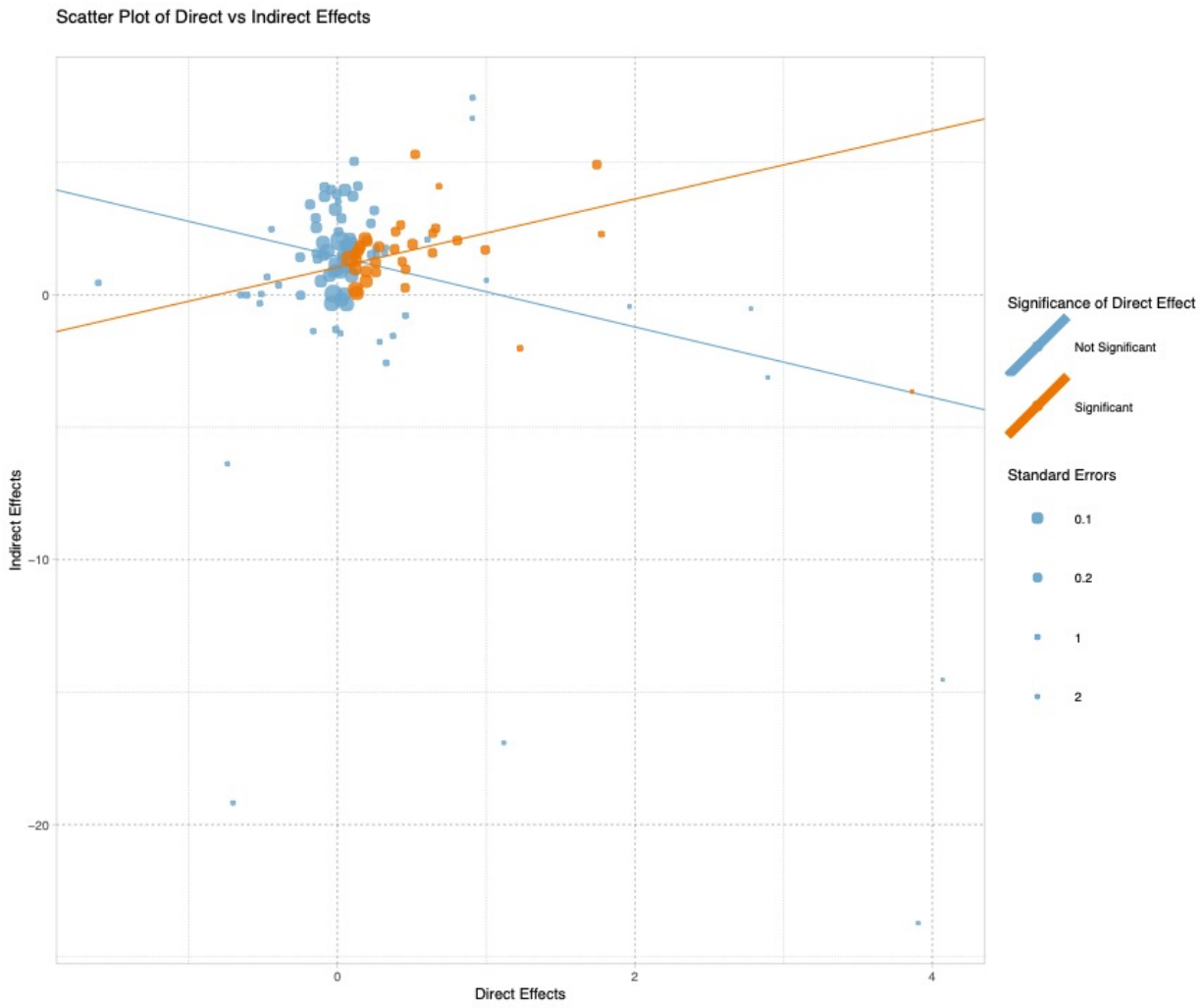

Figure S6: An illustration of the empirical relation between direct and indirect effects for each outcome, using a regression analysis with errors in variables. Each dot in this scatter plot illustrates direct effects and indirect effects for a single outcome. Blue dots represent outcomes for which the direct effect is not significant, whereas orange dots represent outcomes for which the direct effect is significant. Dot sizes are proportional to the estimation error relevant to the corresponding outcome. We also show the errors-in-variables regression lines corresponding to outcomes for which the direct effect was significant (orange) and not significant (blue).

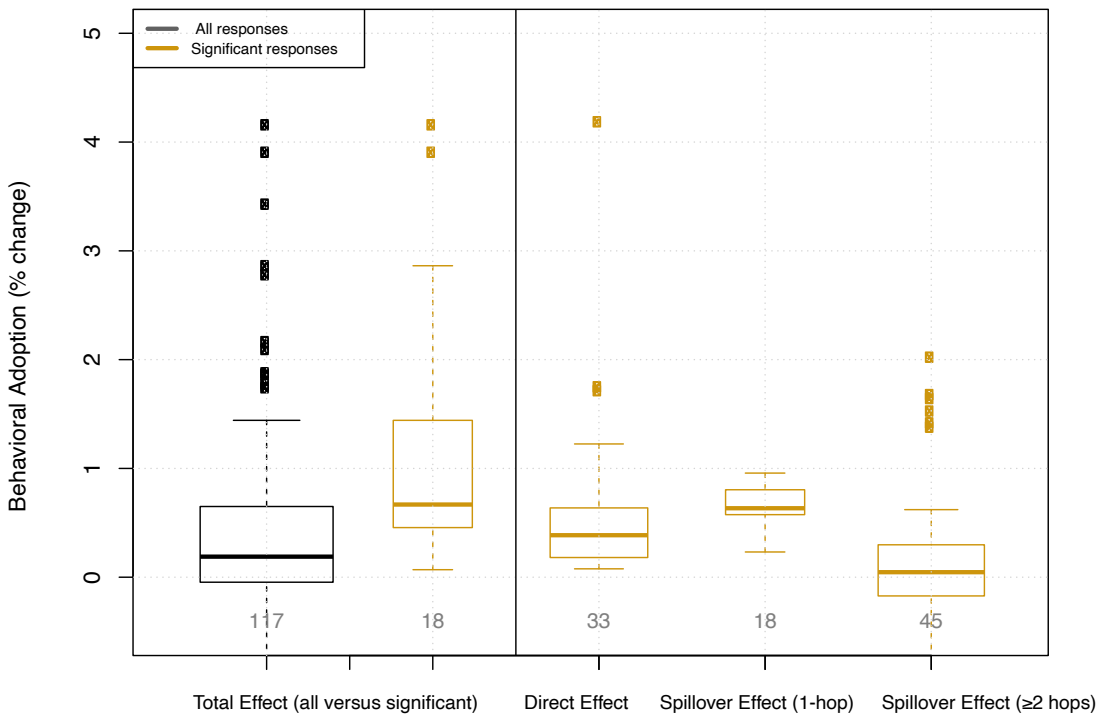

Figure S7. Magnitude of the causal effects of intervention at various geodesic distances. The Y axis is the change in adoption due to targeting expressed as the log odds of a change in outcomes over time. The leftmost section details the total effect of the intervention; within that section, the left boxplot (in black) summarizes the effects of all the outcomes, while the other boxplot (in orange) summarizes the distribution of the effects for the practice outcomes with a significant total effect (N=10). In the panel at right, we then consider three components of the total effect of treatment, namely, the direct effect (seen in N=33 outcomes), the indirect effect from neighboring households one-hop away in the network (seen in N=18 outcomes), and the indirect effect from neighboring households more than one-hop away in the network (seen in N=45 outcomes).

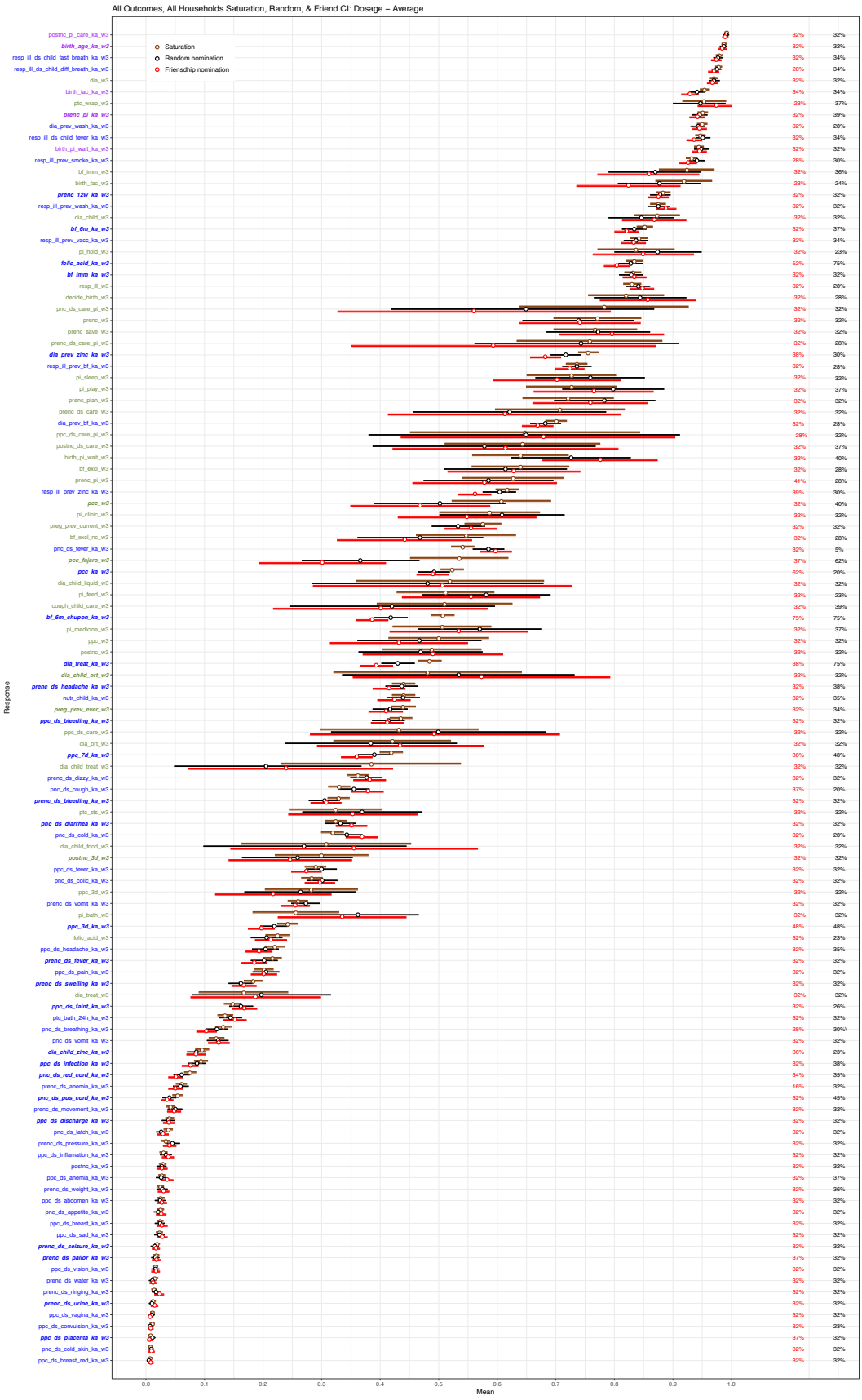
All Outcomes, All Households Saturation, Random, & Friend CI: Dosage – Average
Saturation
Random nomination
Friendship nomination
Response
Mean
0.0
0.1
0.2
0.3
0.4
0.5
0.6
0.7
0.8
0.9
1.0

Figure S8. Effect of nomination targeting on outcome adoption. (a) The X axis measures behavioral adoption at follow-up at wave 3, in terms of the percentage of correct responses at follow-up of all individuals in three sets of households: households in 100%-treated villages (saturation, brown); households in villages with random targeting (black); and households in villages with nomination targeting (red). This is shown for all 117 outcome measures arranged in order of mean response for the outcome in the Wave 3 survey in the 100%-treated villages. The colors of the variable names indicate knowledge (blue), attitude (purple), and practice (green) outcomes. The percentages of households that have to be targeted in the random (black) and nomination (red) strategies to achieve the same ultimate percentage as achieved in the saturation targeting (whatever that percentage might be) are on the far right. Table S6 lists the long descriptions for each of the variable names on the left.

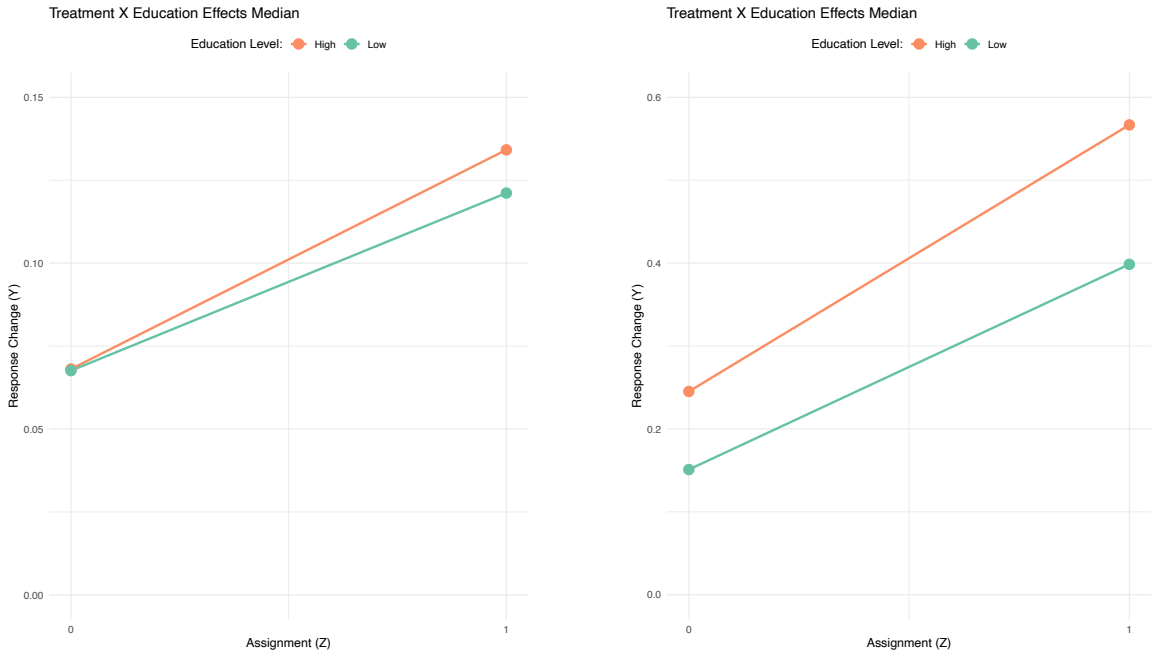

Figure S9. Behavioral change as a function of household education, for all 117 outcomes (left) and for the 33 outcomes with a direct effect (right).

Kin Ties	General Ties (Kin and Non-Kin)
- Does your mother live in this town? What is the name of your mother? - Does your father live in this town? What is the name of your father? - How many siblings do you have? How many are brothers? How many are sisters? - How many of these siblings are over the age of 12 and currently living or working in this village? 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? What are their names? - Are you married or living in a civil union? What is the name of your partner?	- Who do you trust to talk to about something personal or private? - With whom do you spend free time? - Who would you feel comfortable asking to borrow 200 lempiras from if you needed them for the day? - Who do you think would be comfortable asking you to borrow 200 lempiras for the day? - Who would you ask for advice about health related matters? - Who comes to you for health advice? - Besides your partner, parents or siblings, who do you consider to be your closest friends? - What are the names of this town's leaders? - Who are the people in this town with whom you do not get along well? - How many friends or relatives that you see more than once a month live in other villages? What is the name of the village where the majority of these friends/relations live?

Table S1. List of name generators.

Dosage (fraction targeted)	Mean number of visits
0%	0
5%	14.4
10%	14.6
20%	14.8
30%	13.1
50%	13.6
75%	13.9
100%	15.0

Table S2. Mean household visits made by the team delivering the educational intervention (out of a maximum of 22), by fraction targeted. There was no significant difference in visit completion mean by dosage or treatment, but slightly lower mean visits completed in the villages with 30%, 50%, and 75% targeting dosages.

Geodesic Distance	0	1	2	3
Random	2	17	24	2
FNT	0	37	15	0
Centrality	0	35	0	0

Table S3. Average minimum geodesic distances among sets of nodes targeted using random nomination, friendship nomination (FNT), and (degree) centrality. We give the number of nodes at any (minimum) given geodesic distance from chosen targets among the 22 villages with 10% targeting. For instance, among the set of the most central nodes, each node is connected directly to at least one other member of the set (and the geodesic distance in the table is 1.0). Nodes with zero geodesic distance are isolates.

Effect Name	# sig	Test Model/Effect
mf ate	40	$\frac{\sum_i Y_i(Z_i = 1)}{\sum_i (Z_i = 1)} - \frac{\sum_i Y_i(Z_i = 0)}{\sum_i (Z_i = 0)}$
mf ase	23	$\frac{\sum_i Y_i(MinES_i = 1,\ Z_i = 0)}{\sum_i (MinES_i = 1,\ Z_i = 0)} - \frac{\sum_i Y_i(MinES_i = 0,\ Z_i = 0)}{\sum_i (MinES_i = 0,\ Z_i = 0)}$
mf tte	40	$\frac{\sum_i Y_i(Dose_i = 1)}{\sum_i (Dose_i = 1)} - \frac{\sum_i Y_i(Dose_i = 0)}{\sum_i (Dose_i = 0)}$
mb total direct	33	$Y = \alpha + \tau Z + \beta S + \delta D + \epsilon$
mb total spillover	18	$Y = \alpha + \tau Z + \beta S + \delta D + \epsilon$
mb direct	28	$Y_i = \alpha + \tau Z_i + \psi S_i + \delta D_i + \sum_{k \in K} \phi^k\ I(dose_i = k) + \epsilon_i$
mb 1-hop spillover	12	$Y_i = \alpha + \tau Z_i + \psi S_i + \delta D_i + \sum_{k \in K} \phi^k\ I(dose_i = k) + \epsilon_i$
mb multi hops spillover	45	$Y_i = \alpha + \tau Z_i + \psi S_i + \delta D_i + \sum_{k \in K} \phi^k\ I(dose_i = k) + \epsilon_i$
mb contagion	70	$Y_i = \alpha + \tau\, Z_i + \delta\, D_i + \varphi C_i + \sum_{k \in K} \phi^k\ I(dose_i = k) + \epsilon_i$
mb education		$Y = \alpha + \tau(Z * E) + \beta S + \delta D + \epsilon$

Table S4. Equations for models used. Note the following abbreviations; **mf** stands for model-free and **mb** for model-based; **ate** stands for average treatment effects; **ase** stands for average spillover effects**; tte** stands for total treatment effects. Significance was ascertained using FDR correction procedures. We do not report results for the mb contagion models here. The education model was not used to select significant outcomes (for subset analysis).

Outcome Type Subsets
Pearson's Correlation Coefficient for
100%d - 0%d vs. **Random Dosage - Friend Dosage**
Fisher Transformation for two-sided p-value

Group	Outcome Type	n	Empirical	Empirical p-value	Nominal	Nominal p-value
All Households	All	117	**0.238**	0.009454844	**0.196**	0.033562672
	Knowledge	68	0.233	0.055165593	0.113	0.3619969
	Attitude	5	0.395	0.555092166	0.412	0.535493007
	Practice	44	0.370	0.012889961	0.359	0.016153377
Untreated Households	All	117	0.194	0.036350758	0.201	0.02920725
	Knowledge	68	0.197	1.08E-01	0.129	0.295957722
	Attitude	5	-0.285	1.321330104	-0.299	1.337544065
	Practice	44	0.376	0.01142848	0.438	0.002630505
Direct Treatment Effect Outcomes: All Households	All	33	**0.319**	0.070097089	0.270	0.129725392
	Knowledge	26	0.171	0.406683528	0.032	0.87987049
	Attitude	2	1.00	*N/A*	1.00	*N/A*
	Practice	5	**0.897**	0.039532382	0.891	0.043653864
Direct Treatment Effect Outcomes: Untreated Households	All	33	0.236	0.187271976	0.257	0.149950753
	Knowledge	26	0.264	0.194492694	0.195	0.34225737
	Attitude	2	1.00	*N/A*	1.00	*N/A*
	Practice	5	0.835	0.088484966	0.908	0.03184556

Table S5. Correlation between the efficiency of network targeting and the difficulty of inducing outcome changes in a village. We compute the Fisher's Z transform between the ease of acquiring knowledge, attitudes, and practices, on the one hand, and the increase in efficiency due to network targeting, on the other hand. Please note that the p-values we report for the Fisher's Z transform do not account for uncertainty (namely, the estimation error) in the metrics on which the correlation is computed. The ease of acquiring is measured by the difference between the percentage of correct survey responses in the villages where 100% of the households were treated and the percentage of correct responses in the villages where none of the households were treated (the higher this difference, the easier it is to learn or adopt a target outcome). The increase in efficiency due to network targeting is measured by the difference between the fraction of households that need to be treated in the random arm and the smallest fraction of households that need to be treated in the friendship-nomination targeting arm in order to obtain the same percentage of learning and adoption of a target outcome. We repeat this analysis using both nominal fractions (ITT) and empirical (realized) fractions of households treated. We perform this analysis on all households and on the subgroup of untreated households. And we further focus on the subgroups of outcomes showing significant direct treatment effect, and the primary outcomes only. Overall, as outcomes become easier to acquire, we need to treat a lower the fraction of the households using friendship-nomination targeting, when compared to random targeting, to induce the same amount of behavioral changes in the population.

Variable	Variable description
folic_acid_w3	Currently taking folic acid tablets (daily in past 7 days)
prenc_plan_w3	Made a birth plan in preparation for birth
prenc_save_w3	Saved money in preparation for birth
prenc_sell_w3	Sold animal in preparation for birth
prenc_w3	Sought prenatal care within 12 weeks
folic_acid_ka_w3	Identifies that women should take folic acid before pregnancy
prenc_12w_ka_w3	Identifies that women should seek prenatal care first 12 weeks of pregnancy
prenc_support_accompany_ka_w3	Identifies accompanying woman to prenatal care visits as method of support during pregnancy
prenc_support_eat_ka_w3	Identifies ensuring that woman eats well as method of support during pregnancy
prenc_support_rest_ka_w3	Identifies ensuring that woman rests 1 hour per day well as method of support during pregnancy
prenc_support_violence_ka_w3	Identifies avoiding violence as method of support during pregnancy
prenc_support_vitamins_ka_w3	Identifies encouraging women to take vitamins as method of support during pregnancy
prenc_support_work_ka_w3	Identifies helping woman with house work/child care as method of support during pregnancy
prenc_expenses_animals_ka_w3	Identifies saving animals to sell as method of preparing for birth expenses
prenc_expenses_cost_ka_w3	Identifies knowing cost of trip to maternal clinic as method of preparing for birth expenses
prenc_expenses_savings_ka_w3	Identifies having a savings plan as method of preparing for birth expenses
q0300_correct_w3	Correctly answered prenatal care knowledge riddle
birth_fac_w3	Gave birth in health facility
birth_fac_ka_w3	Believes health facility is best place to give birth
prenc_ds_care_w3	Woman sought medical care for pregnancy danger sign
prenc_ds_bleeding_ka_w3	Identified bleeding as pregnancy d.s.
prenc_ds_seizure_ka_w3	Identified seizure as pregnancy d.s.
prenc_ds_headache_ka_w3	Identified headache as pregnancy d.s.
prenc_ds_ringing_ka_w3	Identified ringing in ears as pregnancy d.s.
prenc_ds_dizzy_ka_w3	Identified dizziness as pregnancy d.s.

prenc_ds_urine_ka_w3	Identified difficulty urinating as pregnancy d.s.
prenc_ds_movement_ka_w3	Identified reduced or absent fetal movement as pregnancy d.s.
prenc_ds_water_ka_w3	Identified water breaking as pregnancy d.s.
prenc_ds_fever_ka_w3	Identified fever as pregnancy d.s.
prenc_ds_swelling_ka_w3	Identified swelling of face/hands/feet as pregnancy d.s.
prenc_ds_pallor_ka_w3	Identified pallor as pregnancy d.s.
prenc_ds_weight_ka_w3	Identified difficulty gaining weight as pregnancy d.s.
prenc_ds_pressure_ka_w3	Identified high/low blood pressure as pregnancy d.s.
prenc_ds_vomit_ka_w3	Identified vomiting as pregnancy d.s.
prenc_ds_anemia_ka_w3	Identified anemia as pregnancy d.s.
ppc_3d_w3	Mother had health checked by professional within 3 days of birth
ppc_w3	Mother had health checked by professional within 7 days of birth
ppc_ds_care_w3	Mother sought medical care for postnatal danger sign
postnc_3d_w3	Newborn had health checked by professional within 3 days of birth
postnc_w3	Newborn had health checked by professional within 7 days of birth
postnc_ds_care_w3	Newborn experienced health problem in first month, care was sought
ppc_3d_ka_w3	Identifies that mother should receive postnatal medical check-up within 3 days of birth
ppc_7d_ka_w3	Identifies that mother should receive postnatal medical check-up within 7 days of birth
ppc_ds_bleeding_ka_w3	Identified heavy vaginal bleeding as postnatal d.s.
ppc_ds_pain_ka_w3	Identified lower abdomen pain as postnatal d.s.
ppc_ds_fever_ka_w3	Identified fever as postnatal d.s.
ppc_ds_discharge_ka_w3	Identified vaginal discharge as postnatal d.s.
ppc_ds_headache_ka_w3	Identified headache as postnatal d.s.
ppc_ds_vision_ka_w3	Identified blurred vision as postnatal d.s.
ppc_ds_convulsion_ka_w3	Identified convulsions or fits as postnatal d.s.
ppc_ds_breast_red_ka_w3	Identified red painful area or lump in breast as postnatal d.s.
ppc_ds_placenta_ka_w3	Identified retained placenta as postnatal d.s.
ppc_ds_vagina_ka_w3	Identified ruptured vagina as postnatal d.s.

ppc_ds_faint_ka_w3	Identified weakness or fainting as postnatal d.s.
ppc_ds_abdomen_ka_w3	Identified abdominal tenderness as postnatal d.s.
ppc_ds_breast_ka_w3	Identified breast pain/tenderness as postnatal d.s.
ppc_ds_sad_ka_w3	Identified sadness/depression as postnatal d.s.
ppc_ds_infection_ka_w3	Identified infection as postnatal d.s.
ppc_ds_inflammation_ka_w3	Identified inflammation as postnatal d.s.
ppc_ds_anemia_ka_w3	Identified anemia as postnatal d.s.
postnc_ka_w3	Identified correct ways to provide newborn care
pnc_ds_fever_ka_w3	Identified fever as postnatal d.s.
pnc_ds_diarrhea_ka_w3	Identified diarrhea as postnatal d.s.
pnc_ds_breathing_ka_w3	Identified difficulty breathing as postnatal d.s.
pnc_ds_vomit_ka_w3	Identified vomiting as postnatal d.s.
pnc_ds_appetite_ka_w3	Identified poor appetite as postnatal d.s.
pnc_ds_red_cord_ka_w3	Identified redness/bleeding around cord as postnatal d.s.
pnc_ds_pus_cord_ka_w3	Identified pus in cord as postnatal d.s.
pnc_ds_cold_skin_ka_w3	Identified cold skin as postnatal d.s.
pnc_ds_cough_ka_w3	Identified cough as postnatal d.s.
pnc_ds_cold_ka_w3	Identified cold as postnatal d.s.
pnc_ds_colic_ka_w3	Identified pain/colic as postnatal d.s.
pnc_ds_latch_ka_w3	Identified problems latching as postnatal d.s.
pnc_ds_pneumonia_ka_w3	Identified pneumonia as postnatal d.s.
pcc_w3	Did not use harmful substances to treat cord stump
pcc_fajero_w3	Did not wrap fajero around newborn in first 7 days after birth
pcc_ka_w3	Identified proper cord care methods
q0100_correct_w3	Correctly answered proper cord care riddle
ptc_wrap_w3	Kept newborn wrapped first 7 days after birth
ptc_sts_w3	Held newborn skin-to-skin during first month after birth
ptc_bath_24h_ka_w3	Identifies that newborn should be bathed >24 hours after birth
bf_excl_w3	Exclusively breastfed child first 6 months

bf_excl_nc_w3	Exclusively breastfed child first 6 months without giving chupón
bf_imm_w3	Breastfed immediately after birth
bf_imm_ka_w3	Identifies that newborn should be breastfed immediately after birth
bf_6m_ka_w3	Identifies that newborns should only be given breast milk during first 6 months
bf_6m_chupon_ka_w3	Believes newborns should not be given chupón during first 6 months
prenc_pi_w3	Father accompanied mother to clinic for prenatal care visit at least once
prenc_ds_care_pi_w3	Father accompanied mother to seek medical care for pregnancy danger sign
birth_pi_wait_w3	Father waited at birthplace during labor
ppc_ds_care_pi_w3	Father accompanied mother to seek medical care for postnatal danger sign
pnc_ds_care_pi_w3	Father sought medical care for newborn for postnatal danger sign
pi_hold_w3	Father held child
pi_feed_w3	Father fed child
pi_bath_w3	Father bathed child
pi_play_w3	Father played with child
pi_clinic_w3	Father took child to clinic when sick
pi_medicine_w3	Father gave child medicine
pi_sleep_w3	Father put child to sleep
prenc_pi_ka_w3	Believes father should accompany mother to prenatal care visits
birth_pi_wait_ka_w3	Believes father should wait at birth location while mother gives birth
postnc_pi_care_ka_w3	Believes father should care for children when sick
dia_w3	Did not report diarrhea in past 4 weeks
dia_ort_w3	Used ORT to treat diarrhea in past 4 weeks
dia_treat_w3	Used appropriate treatment for diarrhea in past 4 weeks
dia_child_w3	Child did not experience diarrhea past 4 weeks
dia_child_liquid_w3	Child experienced diarrhea past 4 weeks, was given appropriate amount of liquids
dia_child_food_w3	Child experienced diarrhea past 4 weeks, was given appropriate amount of food
dia_child_ort_w3	Child experienced diarrhea past 4 weeks, was given ORT
dia_child_treat_w3	Child experienced diarrhea past 4 weeks, was given appropriate treatment

dia_child_zinc_days_w3	Child experienced diarrhea past 4 weeks, was given zinc 10-14 days
dia_treat_ka_w3	Identified appropriate diarrhea treatment methods
dia_zinc_ka_w3	Identified zinc 10-14 days as diarrhea treatment
dia_prev_zinc_ka_w3	Identified zinc supplement as way to prevent diarrhea
dia_prev_wash_ka_w3	Identified washing hands with soap and water as way to prevent diarrhea
dia_prev_bf_ka_w3	Identified breastfeeding as way to prevent diarrhea
q0200_correct_w3	Correctly answered diarrhea treatment with zinc riddle
resp_ill_w3	Did not report respiratory illness (coughing) for 2 weeks
cough_child_w3	Child did not have cough past 4 weeks
cough_child_care_w3	Child had cough past 4 weeks, care was sought
resp_ill_prev_zinc_ka_w3	Identified zinc supplement as way to prevent respiratory illness
resp_ill_prev_wash_ka_w3	Identified washing hands with soap and water as way to prevent respiratory illness
resp_ill_prev_vacc_ka_w3	Identified vaccination as way to prevent respiratory illness
resp_ill_prev_smoke_ka_w3	Identified avoiding kitchen smoke as way to prevent respiratory illness
resp_ill_prev_bf_ka_w3	Identified breastfeeding as way to prevent respiratory illness
resp_ill_ds_child_fever_ka_w3	Identified fever as d.s. for children with respiratory illness
resp_ill_ds_child_diff_breath_ka_w3	Identified difficulty breathing as d.s. for children with respiratory illness
resp_ill_ds_child_fast_breath_ka_w3	Identified rapid breathing as d.s. for children with respiratory illness
preg_prev_ever_w3	Reported ever using birth control to delay or avoid pregnancy
preg_prev_current_w3	Reported currently using birth control
birth_age_ka_w3	Believes woman should be at least 18 years of age to have her first child
nutr_child_ka_w3	Identified appropriate dosage for micronutrient supplement
decide_birth_w3	Birth location chosen either jointly or by woman

Table S6. Full variable names for Figure S8.

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