

Educational intervention can rewire village social networks

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Social networks shape behaviour, affect information dissemination and undergird collective action. And they can be exploited in the design of interventions in these domains^{1–7}. Although social networks obey fundamental principles, such as the degree distribution^{8,9}, they can nevertheless evolve as ties form and break. Here, through a randomized controlled trial involving a total of 8,331 people in 110 remote Honduran villages, we evaluate the secondary effects of a 22-month public-health intervention¹ on pre-existing social-network structure. We used a design in which a varying fraction of village households received the intervention, and villages in which no one received the intervention were used as the primary reference group. In ‘low-dosage’ villages (in which 5–30% of households received the intervention), people who received the intervention tended to sever ties with untreated individuals to whom they previously went for health advice. Conversely, in high-dosage villages (50–100%), treated individuals increased their ties. These average changes are explained mainly by tie dynamics; that is, treated–treated pairs persist and form new links, whereas treated–untreated ties dissolve. A mediation analysis indicates that absorption of health information increases the likelihood of tie formation and persistence. The rewiring patterns generally lasted for three years after the intervention ceased. Moreover, although the intervention delivered health information, it reshaped friendship and financial ties, not just health-advice connections. In aggregate, the introduction of a new information regime acted as a kind of social institution, rewiring individuals’ connections even though the overall degree distribution of village-wide networks remained relatively stable.

In many small-scale traditional or rural societies, social connections provide key benefits that would typically be available from formal institutions in large-scale or more developed societies^{10,11}. In such environments, people rely on their friends and family for loans, rather than on banks; for security, rather than on police or courts; and for health care, rather than on doctors or clinics. For instance, in addition to seeking information from providers in the formal health-care sector, women might turn to their social networks (for example, to older female friends or relatives residing in their village) for information about, and assistance with, the delivery and care of newborns.

This raises an important issue, which is that if formal health care or official sources of health information were to be made available, and to be used, one motivation that people had for maintaining social connections to people who previously filled such a need might fall away. That is, the introduction of formal health-care institutions (which might take the form of, for example, monthly educational sessions offered by visiting outsiders) might modify or replace traditional close-knit social connections, leading to potential declines in face-to-face interactions or to a rewiring of social ties within the population. People who

previously relied on friends and kin for health information might cut ties to some of their contacts or form new ties to people who are newly in possession of the information provided by the visiting health-care workers.

When formal institutions are present, in other words, an individual might turn for information and assistance to different community members who have been invested with a possibly new role by the formal institution, even if the individual’s number of connections stays the same. Indeed, many public-health and development initiatives that are ostensibly of a purely informational nature might be inadvertently rewiring social connection in locales around the world, for better or worse. Here, we sought to quantify such possible changes in several ways using data from a large-scale experiment.

Honduras village cohort

We investigated the possible transformative effects of exogenous education interventions on social interactions through a secondary analysis of a randomized trial, examining the connections that individuals

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keep, drop or form, as well as the overall structure of village-wide networks that might arise. Our design improves on that of a few previous studies that might support such a research objective^{2,5,6,12–15}. We studied 110 remote villages in an isolated region of Honduras to which a health-education intervention was delivered over 22 months. We performed a two-stage randomized experiment in which we modulated the fraction of households (the ‘dosage’) that received the intervention in each village (0%, 5%, 10%, 20%, 30%, 50%, 75% and 100%). Among other things, this allows us to probe deeply the particular types of rewiring taking place.

The village populations in our data ranged from 11 to 313 individuals, and the average village size was 75.7 (s.d. = 50). At baseline, the average age of the 8,331 participants was 37 (s.d. = 18; range: 12–91), 63% were women and 63.8% were married or in a civil union.

The 22-month health-education intervention was delivered from November 2016 to August 2018. We mapped the village-wide social networks before and after the intervention¹⁶. That is, we measured them immediately before the intervention in 2016 (wave 1; October 2015 to June 2016) and immediately afterwards in 2019 (wave 3; January 2019 to December 2019). Changes in these social networks are the main outcomes of interest. We discerned health-related connections, financial connections and friendship connections (Methods) for all village residents. On average, at baseline, when we include intra-household social connections (for example, when a person says they get health advice from their spouse), individuals had 2.9 (s.d. = 3.1, range: 0–66) health-advice connections, 5.5 (s.d. = 3.5, range: 0–30) friendship connections, and 2.7 (s.d. = 2.5, range: 0–26) financial connections.

In addition, between January and August 2018 (wave 2), we measured 62 knowledge outcomes part way through the intervention. A summary measure of these outcomes serves in a causal mediation analysis connecting wave-1 intervention assignment to wave-3 network structure outcomes. Finally, we verified persistence in network rewiring at wave 4 (August 2022 to July 2023) for an exogenously chosen subset of 56 of the 110 villages. Extended Data Figure 1 summarizes the study timeline and data collection.

Statistical approach

We estimated the average causal effects of the intervention on network properties as they changed between wave 1 (2016) and wave 3 (2019) (including overall, total, spillover and direct effects^{17–22}; Methods), and used randomization-based permutation tests for experiments with interference^{21,22}, with a studentized village-level difference in degree change test statistic (see Methods for the estimands, test statistics and comparison groups). In parallel, we performed a link-level analysis, assessing the extent to which directed dyads (treated–treated (TT), treated–untreated (TU and UT) and untreated–untreated (UU)) are more likely to persist, break or form in treated villages, compared to untreated dyads in 0% villages (U_0U_0) (Methods and Supplementary Information section 12), and we performed a mediation analysis^{23,24} linking wave-1 intervention assignment to wave-3 network outcomes through a wave-2 summary measure of health knowledge (Methods and Supplementary Information section 18). We also analysed the persistence of the effects at wave 4 in the subset of 56 villages. Because effects might vary with village dosage, we present pooled results and also stratified analyses for low (5–30%) and high (50–100%) dosage villages, with the 0% villages as the primary reference group (see Extended Data Fig. 3 for further justification of this subgroup analysis). Robustness checks related to sample inclusion, modelling approach and computation of *P* values appear in the Methods.

Effects on health-advice networks

We first estimate the average causal effects of the intervention on node-level properties of the health-advice network between waves 1 and 3.

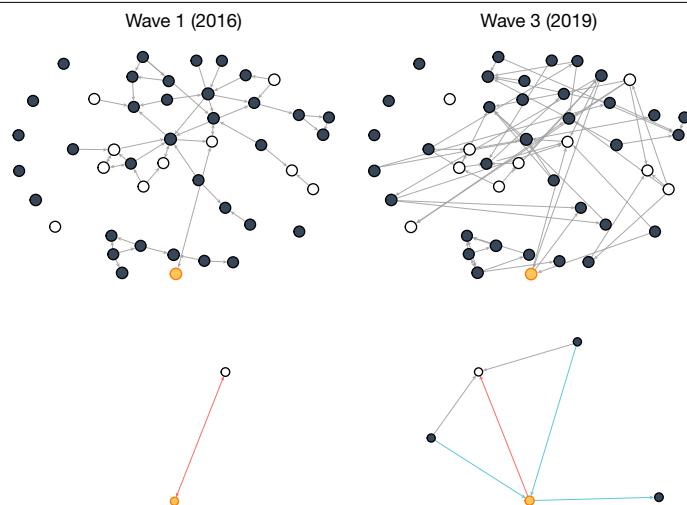


Fig. 1 | Illustrative health-advice network relationships for a village with 45 households. A total of 75% of households were treated (black nodes) versus untreated (open nodes) in wave 1 (left) and are also marked in wave 3 (right). In the bottom panels, we zoom in on the connections of a single treated individual, who is highlighted in yellow in both waves. Connections from the yellow ego to its alters are colour-coded: to or from treated individuals in light blue, and to or from untreated individuals in red. Connections among the alters themselves are shown in grey. The effect of the intervention on the social structure of the yellow node is apparent in this instance. Visually speaking, the overall connectivity in this village receiving a 75% dosage also increases (comparing top left to top right).

Figure 1 illustrates such changes for an example village at village-wide and individual levels. Although global village-wide statistics such as the shape of the degree distribution remain stable in the villages (Extended Data Fig. 2), the intervention affects individual connectivity (and also has other effects on overall network structure).

Here, we focus on changes in mean total degree, in-degree and out-degree (with other metrics, such as clustering, closeness and betweenness, reported in Extended Data Tables 1–3). Pooling all dosage arms, the overall effect of being in a fully or partially treated village versus an untreated village on mean degree is negligible, with the relative difference in the mean change being estimated at 0.06% ($P = 0.977$; Fig. 2a and Extended Data Table 1).

However, at the individual level, treated and untreated people in treated villages move in opposite directions. Treated individuals seem to gain higher connectivity on average (4.18% total degree, 4.77% in-degree and 3.59% out-degree; $P = 0.143$, 0.226 and 0.295, respectively; Fig. 2d and Extended Data Table 1), whereas untreated individuals in treated villages seem to exhibit lower connectivity (–3.62%, –4.15% and –3.09%; $P = 0.228$, 0.338 and 0.363; Fig. 2g and Extended Data Table 1). The resulting direct effect (treated versus untreated in treated villages) is estimated at 7.78% ($P < 0.001$) (Fig. 2j and Extended Data Table 1). These pooled averages, however, mask heterogeneity by dosage, motivating the stratified analyses that follow.

Health-advice networks at a low dosage

In low-dosage villages, the intervention is associated with declines in connectivity. Overall, total, spillover and direct effects all point in the same (negative) direction (Fig. 2, middle column and Extended Data Fig. 3). Relative to 0% villages, the overall change in total degree, in-degree and out-degree is estimated at –6.36% for each measure ($P = 0.042$, 0.149 and 0.072, respectively) (in a directed network, average in-degree equals average out-degree, so percentage changes in all three measures coincide). This overall, node-level effect holds under the false discovery rate (FDR) criterion (see Supplementary Information section 22 for other robustness checks).

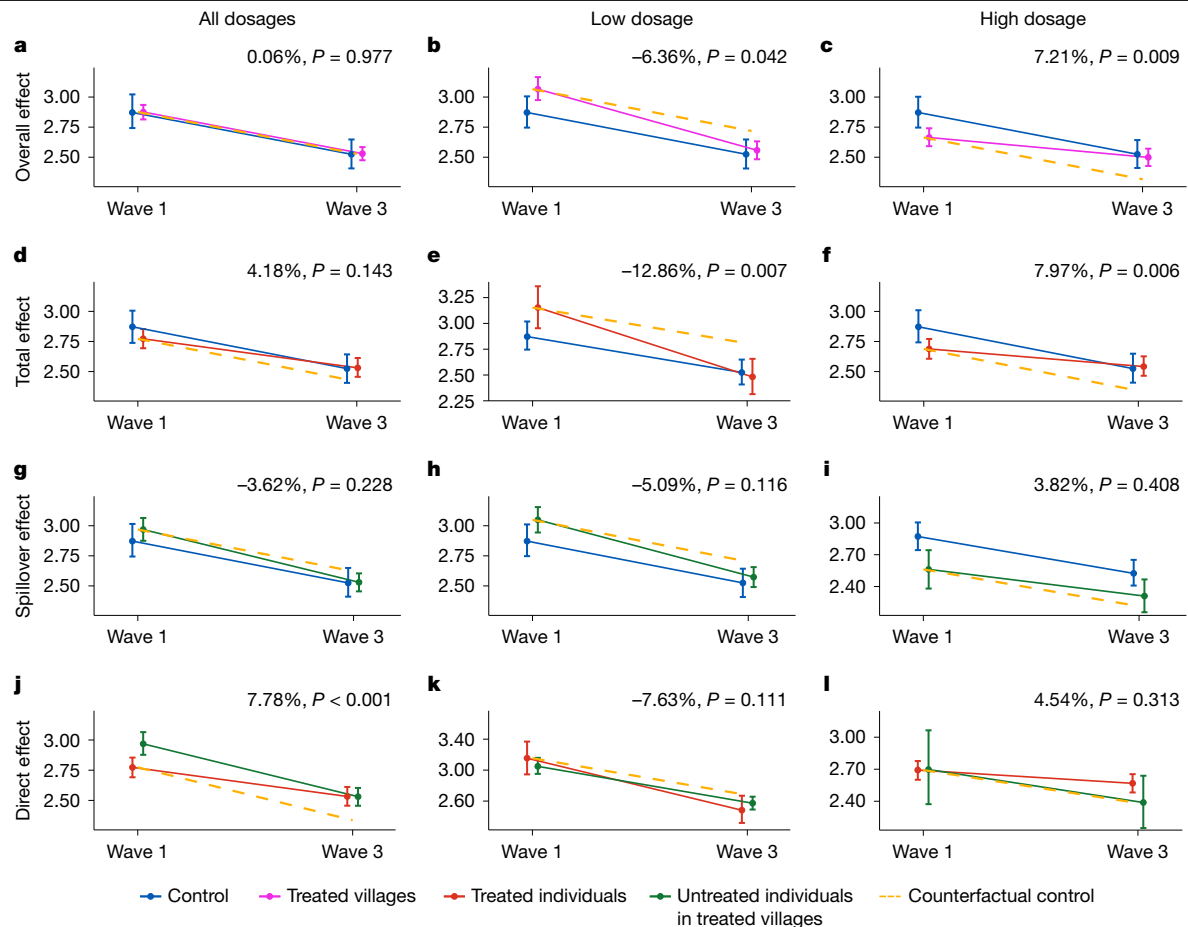


Fig. 2 | Overall, total, spillover and direct effects on mean degree changes, assessed in all villages combined and in low-dosage and high-dosage villages separately. The x axis shows time (waves 1 and 3) and the y axis shows the mean degree per group. Purple, all individuals in fully or partially treated villages (treated villages); red, treated individuals in partially and fully treated villages (treated individuals); green, untreated individuals in partially treated villages (untreated in treated villages); blue, untreated individuals in fully untreated villages (control). Dashed yellow lines (counterfactual control) show the expected mean degree without treatment under a parallel-trends assumption. Each panel reports the estimated effect and its *P* value (two-sided permutation tests with a studentized village-level difference in degree change under the two-stage design; 10,000 permutations); 95% confidence intervals are shown around the wave-1 to wave-3 points. Effects compare the change in mean degree from wave 1 to wave 3 between the group of interest and the control group. *P* values are unadjusted, single-test values; adjustments for multiple comparisons are reported in the Methods and Supplementary

For treated individuals in low-dosage villages, the degree measures decline by 12.86% for total degree ($P = 0.007$), 13.35% for in-degree ($P = 0.042$) and 12.38% for out-degree ($P = 0.0287$), versus untreated individuals in 0% villages. For untreated individuals in low-dosage villages, the corresponding changes are -5.09% , -5.00% and -5.18% ($P = 0.116$, 0.266 and 0.123). The direct effects (treated versus untreated in treated villages) are -7.63% , -8.18% and -7.07% ($P = 0.111$, 0.245 and 0.170). These effects (for total degree) are shown in Fig. 2 (middle column); in-degree and out-degree appear in Extended Data Table 2.

To understand who drives these declines, we next examine one-hop-away spillovers, comparing untreated individuals who have at least one treated network neighbour at wave 1 with untreated individuals in 0% villages (whose neighbours are necessarily untreated) (Methods and Supplementary Table S1). Among 661 such untreated one-hop-away neighbours, total degree, in-degree and out-degree fall by 54.09%, 57.32% and 50.85% (all $P < 0.001$) relative to untreated people in 0%

Information section 22. Exact *P* values for all panels are provided in the source data for this figure; in **j** the value is smaller than 0.001 and is shown as $P < 0.001$. For reference, the control-village change in mean degree from wave 1 to wave 3 is -12.09% (Supplementary Information). Rows show overall (**a–c**), total (**d–f**), spillover (**g–i**) and direct (**j–l**) effects (the direct-effect curves pool treated individuals in all treated villages; the direct-effect *P* values come from the within-village comparison in partially treated villages); columns show all dosages (**a, d, g, j**), low dosage (**b, e, h, k**) and high dosage (**c, f, i, l**). Sample *n*: 6,672 (**a, j**), 3,514 (**b, k**) and 3,158 (**c, l**) individuals in treated villages; 3,149 (**d**), 573 (**e**) and 2,576 (**f**) treated individuals; 3,523 (**g**), 2,941 (**h**) and 582 (**i**) untreated individuals in treated villages; and 1,659 control individuals. Villages: **a**, 88 treated and 22 control; **d**, 87 treated and 22 control; **b** and **h**, 44 low-dosage and 22 control; **e**, 43 low-dosage and 22 control; **c** and **f**, 44 high-dosage and 22 control; **g**, 66 partially treated and 22 control; **i**, 22 high-dosage partially treated and 22 control; **j**, 65 partially treated; **k**, 43 low-dosage partially treated; **l**, 22 high-dosage partially treated.

villages, indicating a substantial loss of ties around treated individuals. This observed reduction can be explained by an observed drop in connectivity for untreated–treated and treated–untreated dyads, which is examined using the link-formation analysis further below.

A speculative explanation for this greater reduction is that untreated one-hop-away neighbours lost inbound connections because treated individuals became self-reliant or more selective, choosing not to ‘trust’ their untreated neighbours for health advice after the intervention. They also lost outbound connections. Perhaps, through within-village diffusion of information, they became more self-reliant themselves (see Supplementary Table S4 for further explanation).

We next examine the higher-degree spillover effect, comparing 2,280 untreated individuals who were only indirectly connected to treated individuals in wave 1 (that is, through more than one degree of separation) with untreated individuals in control villages. For these individuals with a more-than-one-hop-away connection to treated individuals, we

see an increase of 9.11% in total degree ($P = 0.002$), 10.17% in in-degree ($P = 0.006$) and 8.05% in out-degree ($P = 0.026$), compared with control villages. This increase reflects the fact that such individuals have ties exclusively to other untreated villagers at the start of the study—ties that, as the link analysis below shows, dissolve less in partially treated villages than in control villages—but remain free to add new ties to both treated and untreated individuals after the intervention. The observed growth is thus an outcome of stable untreated–untreated links plus newly possible links to treated people, and is not likely to be due to a substantive spillover effect of being indirectly connected to a treated individual (see Supplementary Table 84 for further explanation).

We performed a link-formation analysis in low-dosage villages to examine which types of links already existing in wave 1 are more likely to be maintained or broken in wave 3, compared with fully untreated villages (Extended Data Table 4). Directed links from treated to untreated individuals (TU links) and directed links from untreated to treated individuals (UT links) break more often than do links between untreated individuals in untreated villages (U_0U_0 links). Relative to U_0U_0 ties (reference group; 33.7% persistence), TU and UT ties are estimated to be less likely to persist, with 24.4% and 35.6% lower odds ($P = 0.059$ and $P = 0.005$). Moreover, TT ties are observed to have 52.6% higher odds of persisting ($P = 0.009$), and UU links in treated villages also show slightly higher odds of persisting (12.6%; $P = 0.29$), but these do not reach significance so cannot be interpreted.

As mentioned earlier, the drop in UT and TU ties could be due to a loss of credibility among untreated individuals and self-reliance among both treated and untreated individuals, thanks to the training received and diffusion of knowledge, respectively; and the persistence of TT links (and possibly UU links) might reflect greater cohesion and trust between pairs with a similar treatment status. This stability of UU links in treated villages, a spillover effect of being in a partially treated village, could explain why one-hop-away untreated neighbours show an average loss in degree, whereas higher-order untreated individuals, who by definition hold only UU ties at first, show an average increase.

Next, we investigated the effect of low-dosage treatment on link formation by analysing the probability that individuals who are unconnected in wave 1 establish new ties by wave 3 (Extended Data Table 5). Compared with U_0U_0 dyads (0.89% formation rate in low-dosage villages), UU, TU and UT ties are observed to be less likely to form (−6.36%, −32.01% and −31.71% in odds; $P = 0.738$, 0.061 and 0.071, respectively), whereas TT ties have a higher likelihood of forming (63.35%; $P = 0.025$). The reduction of TU ties arises because treated individuals dissolve ties from wave 1 with previously trusted untreated alters (members of their personal network), perhaps adopting self-reliance or preferentially forming ties with other treated individuals.

In summary, in low-dosage villages, we observe a reduction in social connections, compared with fully untreated villages. This is driven mostly by the spillover effects on untreated individuals, especially those who are directly connected to treated neighbours in wave 1 (see Supplementary Table 51 for detailed analyses). In general, the overall effects are weighted averages of total and spillover effects, which are in turn weighted averages of dyadic effects. Total effects reflect the effects on TU and TT links, and spillover effects reflect those on UT and UU links (Supplementary Information section 14). Given the negative effects on TU and UT links for both tie persistence and formation, the negative effect on UU tie formation, the relatively low density of initial networks and the high prevalence of untreated individuals in low-dosage villages, the negative effects follow for treated individuals, untreated individuals and overall.

Health-advice networks at a high dosage

In high-dosage villages, we observe similar patterns at the dyadic level. However, the larger number of treated individuals produces more TT

links and fewer UT and TU links overall, and this leads to an overall increase in average degree.

Specifically, in high-dosage villages, being in a partially or fully treated village results in an average relative increase of total degree, in-degree and out-degree by 7.21% for all three metrics, with $P = 0.009$, compared with an untreated village (Extended Data Table 3 and right column in Fig. 2). For treated individuals, there is a relative increase in total degree, in-degree and out-degree of 7.97%, 8.80% and 7.14% ($P = 0.006$, 0.025 and 0.038), compared with untreated individuals in fully untreated villages. By contrast, for untreated individuals, the spillover effect shows a negligible change in total degree, in-degree and out-degree of 3.82%, 0.14% and 7.49% ($P = 0.408$, 0.976 and 0.149), compared with untreated individuals in untreated villages. Overall, the treatment encourages treated individuals to expand their networks.

One-hop-away spillovers remain negative but smaller in magnitude than they are in low-dosage villages. Among 314 untreated one-hop neighbours of treated alters, total degree, in-degree and out-degree fall by 19.05%, 20.06% and 18.04% ($P = 0.002$, 0.018 and 0.011) more than they do for untreated individuals in fully untreated villages (Supplementary Table 52). Thus, even in high-dosage villages, untreated individuals adjacent to treated ones tend to lose both incoming and outgoing ties—probably through the same mechanisms as those at low dosage (decreased credibility of the untreated and increased self-reliance among the treated). Higher-order untreated individuals ($n = 268$) exhibit a positive difference in the change in total degree, in-degree and out-degree of 30.60%, 23.80% and 37.40% ($P < 0.001$, 0.004 and <0.001), compared with the control group. Similar to low-dosage villages, this lower reduction is likely to be due to the persistence of untreated–untreated ties.

We again examined which pre-existing (wave 1) connections are more likely to break in the high-dosage villages (Extended Data Table 6). As in the low-dosage situation, TU and UT ties are less likely to persist (−40.90% and −32.87% in odds; $P = 0.0016$ and 0.0138) relative to U_0U_0 links (33.6%). TT and UU ties show 14.05% and 15.89% higher odds of persistence, respectively, although statistical support for this is relatively low ($P = 0.164$ and $P = 0.315$). Furthermore, we consider nodes that were not connected in wave 1, and we find null effects (Supplementary Table 18).

Summary of health-advice effects

In summary, the foregoing analyses reveal noteworthy trends in connection behaviour (Fig. 3). In villages overall (with either low or high fractions of households treated), we observe an increase in connectivity among treated–treated individuals and among untreated–untreated individuals, along with a reduction in connectivity among treated–untreated individuals, compared with untreated individuals in control villages. The increased connectivity among matched pairs could be a result of cohesion, trust and perceived similarity in knowledge, whereas the reduced connectivity among mismatched pairs could be explained by a decrease in perceived reliability among the untreated and an increased self-reliance among the treated. The treated individuals tend to cut connections with their untreated neighbours whom they previously trusted for health advice, and so opt for self-reliance. As a result of these dyadic tie dynamics, we see a heightened reduction in connectivity among first-degree untreated neighbours of treated individuals, compared with untreated individuals in control villages. We also see an increase in degree among higher-order untreated neighbours (those who are more than one hop away from treated individuals, and not otherwise connected to any treated person) owing to their enduring UU ties.

In low-dosage villages, because so few individuals are treated, the treated individuals have more limited opportunities to connect with others who have similar knowledge (acquired either from the intervention or from other treated individuals through diffusion¹), and this leads

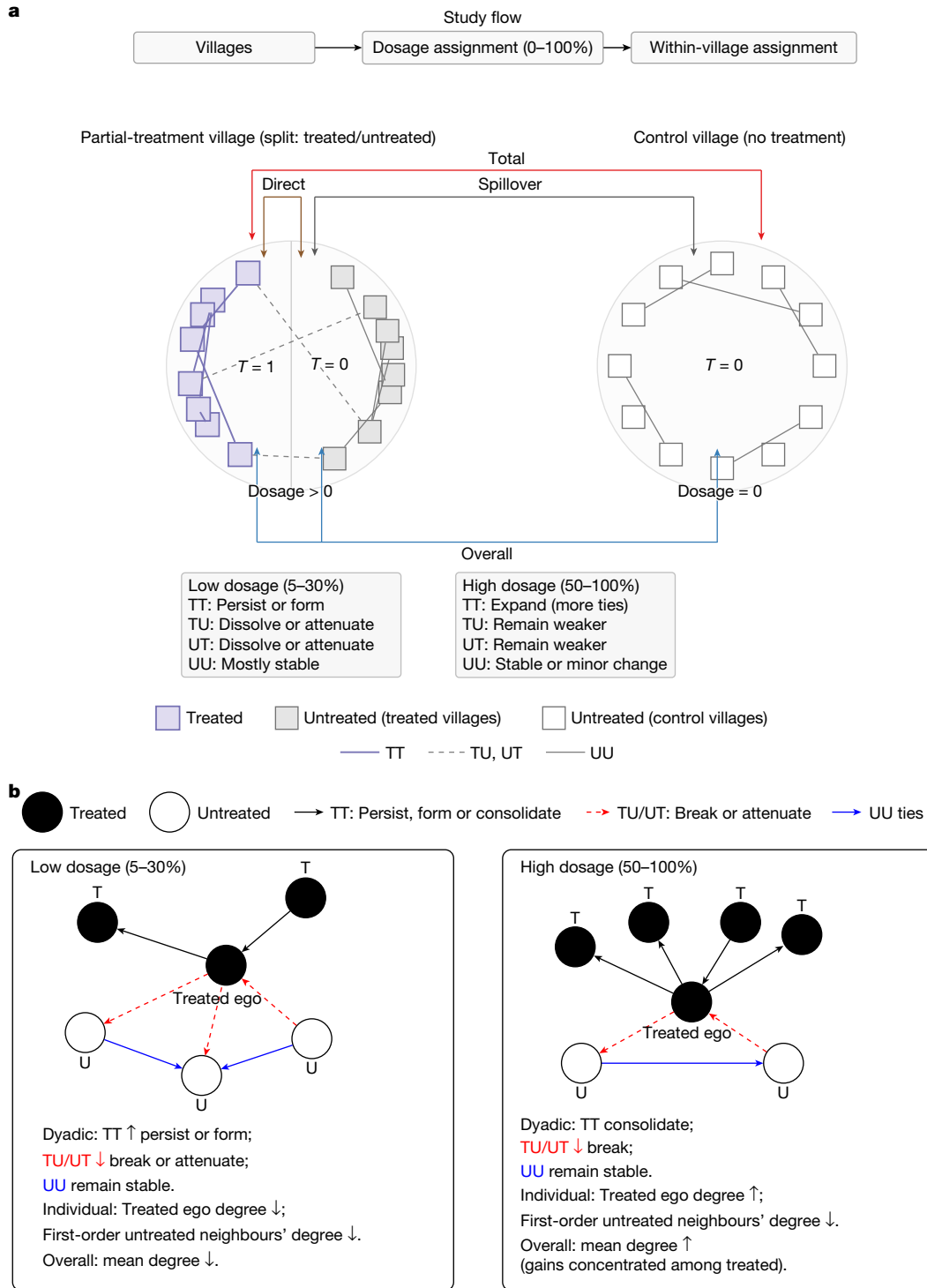


Fig. 3 | Summary of approach and main results. a, Conceptual schematic. Village randomization and within-village assignment is shown, along with comparisons for total, spillover and direct effects (red, black and brown, respectively) and for the overall effect (blue). T indicates treated individuals. Treated individuals are shown in light blue ($T = 1$), untreated individuals in treated villages ($T = 0$ and dosage > 0) are grey, and untreated individuals in

control villages are white ($T = 0$ and dosage = 0). **b**, Results by dosage. Dyadic-level tie dynamics are shown, with TT ties in black, TU and UT ties in red, and UU ties in blue. TT ties are more likely to persist or form in low dosage and consolidate in high dosage; UT and TU ties are likely to weaken or dissolve in both low and high dosages; and UU ties tend to remain more stable in both low and high dosages (compared with U_0U_0 ties in 0% villages).

to a decline in their network ties. However, once enough people (around 30% of households) receive the educational intervention, the treated group seems to pass a critical threshold, which makes it much easier for them to form or reinforce connections with individuals who were also treated—increasing overall connections among the treated group and even yielding more cohesive networks for the treated.

The foregoing effects do not vary consistently by demographic attributes of the villagers across a variety of specifications (see Supplementary Information section 13 for full heterogeneity analysis). By contrast, the interaction between treatment status and baseline health-advice degree is consistently negative for overall effects (where, for overall effects, ‘treatment’ equals 1 for all individuals in partially or fully treated

villages, and 0 for individuals in 0% villages). In low-dosage villages, the treatment $\times$ baseline degree terms are -0.171 for total degree and -0.194 for in-degree (both $P < 0.001$). In high-dosage villages, the corresponding coefficients are -0.124 for total degree and -0.096 for in-degree (both $P < 0.001$) (Supplementary Tables 23 and 27). In other words, people with many health-advice ties at baseline are relatively more likely to lose ties when they are given the educational intervention, compared with people who have relatively few ties at baseline. Finally, robustness tests that exclude intra-household connections remain consistent with most of the main results (Supplementary Tables 12 and 13).

Knowledge as a mediator of rewiring

We assessed the extent to which tie rewiring is a result of the acquisition of health knowledge (which was the objective of the education intervention). We estimated mediation effects at the individual and dyadic levels, using the change from wave 1 to wave 2 in a summary score of 62 knowledge items as the ‘mediator’ with respect to change of network degree between wave 1 and wave 3 (see Methods for the construction of the knowledge score and the mediation models). For every effect (overall, total, spillover and direct), we report the natural indirect effect (NIE) operating through the change of knowledge, and the proportion of the total effect that is mediated (Methods and Supplementary Information section 18).

At the individual level, at a low dosage, the total effect shows NIE = 0.030 ($P < 0.001$; mediated share -8.3%). The direct effect shows NIE = 0.031 ($P < 0.001$; -14.2%). Overall and spillover NIEs are small and not significant. At a high dosage, NIE is significant for all four estimated effects: overall 0.036 ($P < 0.001$; 19.5%), total 0.038 ($P = 0.004$; 18.3%), spillover 0.016 ($P = 0.042$; 18.8%) and direct 0.025 ($P = 0.028$; 20.3%).

Thus, at a high dosage, knowledge mediates around 20% of the effects on tie rewiring (at a low dosage the mediated shares are smaller, between -2.3% and -14.2%), and, in both dosage strata, the mediated pathway operates to increase degree. In low-dosage villages, in which effects reduce degree, the mediation effects (through knowledge) move in the opposite direction, yielding a larger decrease when controlling for knowledge. In high-dosage villages, in which effects raise degree, they move in the same direction, partially explaining the increase. Nevertheless, most of the effect of treatment on network rewiring in our experiment (about 80%) is not mediated through knowledge.

We also estimated mediation effects at the dyadic level, finding that a gain in knowledge by one or both individuals in a dyad, for any type of dyad (TT, UT, TU or UU) in partially or fully treated villages, shows a small increase for the TT ties in the likelihood of tie formation and tie persistence, especially in high-dosage villages (11%; Extended Data Fig. 4). Given the positive direct effect of the intervention on knowledge for the treated and the positive spillover effect for the untreated, the intervention can raise average degree through a change in knowledge for any dyad type.

Persistence of rewiring effects

A follow-up wave of network mapping (wave 4) was collected in a subset of 56 villages in 2022–23. We used these data to re-estimate the effects of the intervention on network changes from wave 1 to wave 4 in a panel of 3,732 stable residents from these villages (1,790 residents from 26 low-dosage villages, 1,063 from 17 high-dosage villages and 879 from 13 control villages).

In low-dosage villages (Supplementary Table 62), wave-4 results replicate wave-3 tie dynamics. The overall effect of the intervention comparing low-dosage villages with control villages on the average change in health-advice degree remains negative (average overall effect on change in total degree: -0.321 , $P = 0.005$; in-degree: -0.160 , $P = 0.049$; and out-degree: -0.160 , $P = 0.019$). Losses are concentrated among

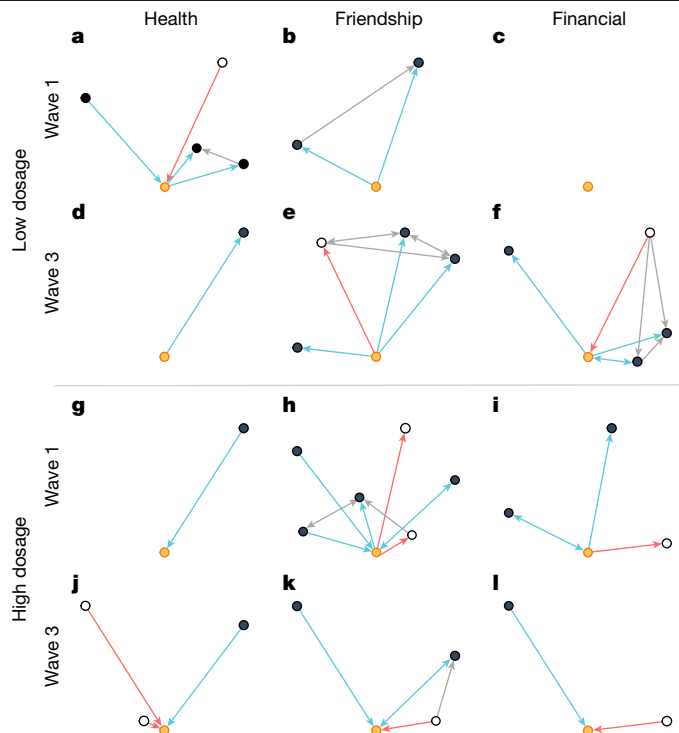


Fig. 4 | Three-layer multiplex network with health, friendship and financial layers shown at waves 1 and 3. **a–l**, Two focal treated nodes (yellow), one in a low-dosage (30%) village (**a–f**) and one in a high-dosage (75%) village (**g–l**), at wave 1 and wave 3, across the health (**a, d, g, j**), friendship (**b, e, h, k**) and financial (**c, f, i, l**) layers. Edges from the yellow ego are light blue to treated alters and red to untreated alters; ties among alters are grey. The yellow nodes keep the same positions across waves and layers. In the low-dosage example, the focal health ties start high and decrease by wave 3, whereas friendship and financial ties start low and increase. In the high-dosage example, the focal health-advice ties start low and increase by wave 3, whereas friendship and financial ties start high and decrease. This illustrates some of the patterns observed across both low and high dosages in the relationship dynamics across the three layers.

untreated neighbours of the treated (average spillover effect on change in degree: -0.333 , $P = 0.005$; in-degree: -0.170 , $P = 0.044$; and out-degree: -0.163 , $P = 0.022$). In high-dosage villages, we do not observe any statistically significant changes in network wiring compared with wave 1 (although, owing to a smaller sample of 17 high-dosage villages, we have limited statistical power; Supplementary Table 63). Finally, in the analysis of heterogeneity, we find consistent effects with respect to the relevance of baseline health-advice degree for both low- and high dosage villages (Supplementary Tables 64–71).

Friendship and financial networks

The intrinsic multiplexity of village social networks²⁵, in which, for instance, an ego can have several kinds of ties to the same alter (for example, health advice, friendship and financial), means that some of the friendship and financial ties might be rewired because of the health-advice tie rewiring. See Fig. 4 for illustrative results.

At a low dosage (5–30%), we find that the main effects on friendship degree are modest, but the multiplex moderators are informative. For the overall effect, the treatment $\times$ baseline health-advice degree interaction is positive (0.067 ; $P = 0.018$), and it occurs also for the spillover effect (0.068 ; $P = 0.019$), indicating that villagers who started as health-advice hubs tend to preserve or grow friendship ties under exposure, consistent with a kind of cross-layer buffering. At a high dosage (50–100%), the same multiplex lever strengthens: treatment $\times$ baseline health-advice degree is strongly positive for overall (0.164 ; $P < 2 \times 10^{-7}$)

and total ($0.168; P < 4 \times 10^{-7}$) effects, and remains positive for the spillover effect ($0.134; P = 0.018$). In other words, initially health-central actors expand friendship connectivity the most in high-dosage villages. Together, friendship rewiring tracks the health-advice layer, in that low dosage yields buffered contraction around health-central egos and high dosage yields a consolidation of friendship ties around such egos (Supplementary Tables 35–42).

For financial ties, low dosage aligns with pruning among baseline financial hubs. In overall and spillover effects, treatment $\times$ baseline financial degree is negative (-0.087 and $-0.095; P = 0.001$ and $P < 0.001$), implying that each extra baseline lending or borrowing tie above the village mean predicts more ties lost by wave 3 under exposure, especially around untreated neighbours of the treated. At the same time, multiplex tie overlap buffers this loss. Treatment $\times$ baseline health-advice degree is modestly positive at a low dosage (0.041 in overall; $P = 0.047$), partially offsetting financial pruning for health-central actors. In high-dosage villages, the cross-layer consolidation becomes clearer, where treatment $\times$ baseline health-advice degree is positive in overall and total effects (0.086 – $0.098; P \leq 0.001$). This indicates that health-central egos also gain financial ties when treatment is widespread. Supplementary Tables 43–50 provide heterogeneity analysis for these effects.

Overall, at a low dosage, friendship changes are modest but buffered among health-central egos, whereas financial ties show clear pruning for financially central actors, partly offset where health centrality is high. At high dosage, both layers consolidate around health-central actors. In short, initial health-network connections, which change because of the intervention, affect changes in both the financial and the friendship networks.

Discussion

We have shown that a health-education intervention delivered in person for a sustained period in isolated Honduran villages affected the dynamics of community social interactions. Changes in the wiring patterns within networks, relating specifically to who is connected to whom, resulted in various structural properties of the health network being altered. In villages in which a low fraction of households is treated, ties are rewired and there is some erosion of traditional social structures. But when a high fraction of the households is treated (above a certain threshold), and villages are saturated with the exogenous institution, the intervention seems to bolster local networks, enhancing community-based knowledge-sharing. In short, we observe a treatment-induced phase transition in network connectivity, in which low saturation weakens certain existing ties and high saturation reinforces them. There was a scale-dependent rewiring of social ties.

This nuanced effect is particularly evident in the differential effects on treated individuals and their untreated one-hop-away neighbours. Treated individuals tended to sever connections with untreated individuals to whom they were initially linked and to form new links to one another. That is, the mechanism is dyadic, with links among treated individuals persisting or forming, and links among treated and untreated individuals dissolving. Moreover, these effects generally persisted after three years, indicating durable intervention-induced rewiring. Friendship and financial ties also shifted in parallel with health-advice ties, reflecting the broader, multiplex reshaping of village social life by the intervention. Nevertheless, at both low and high dosages, although changes to the internal wiring within the villages are observed, there is, in general, no statistical evidence that certain overall, village-wide network properties (such as the degree distribution) change.

The structure of a social network clearly has relevance to its collective properties, as indicated by both mathematical models^{26–32} and previous empirical work with online networks^{33–35}. Still, situations in which ensembles of many face-to-face networks have been ascertained (say, in collections of villages, firms, or schools) remain rare^{1–7,36,37}, and

interventions to explicitly foster the rewiring of naturally occurring face-to-face networks are rarer still. But whether advertently or inadvertently targeted at modifying network structure, it is clear that some interventions could have such an effect. Nevertheless, the fundamental structure of human social networks is not coincidental, and is likely to have evolutionary antecedents and implications^{8,9,38–40}. Hence, social networks can be structurally consistent even when perturbed, to some extent. Such an equilibrium perspective suggests that despite a local churning of social ties under new influences, certain aspects of overall network structure can also remain remarkably stable⁴¹.

Many interventions seek to exploit social influence to enhance their effect. For instance, in face-to-face networks, behaviours as diverse as microfinance uptake in India⁷, iron supplementation in India², vitamin use in Honduras⁵, agricultural technology adoption in Malawi³ and school bullying in the USA⁶ have been shown experimentally to spread by social contagion. But such interventions, especially if substantial or sustained, are likely to also affect the underlying social ties in the populations at hand. Consequently, our study sheds light not only on the transformative effect of a health-education intervention in rural Honduran villages, but also on development interventions in other locales or domains.

A previous study of microcredit showed that although microcredit increased borrowing, its effect on profits and development outcomes was not significant. Notably, this intervention altered network dynamics by influencing social relationships and financial interactions within communities, with a reduction of informal borrowing and gift exchanges¹³. Similarly, an intervention promoting eco-friendly practices or providing agricultural subsidies not only targeted specific outcomes, such as the adoption of particular pesticides or improved production, but also reshaped the underlying social networks by influencing community interactions¹⁵. These effects on network structure underscore the broader potential and impact of community-based interventions in ways that can enhance or degrade the desired effect of the intervention on its targeted outcomes. This is especially likely to be the case for interventions that use social connections to accelerate behaviour change in the first place. Our experimental design provides clarity regarding such effects, because we varied the dosage of the treatment and studied detailed interactions among treated and untreated individuals, and because we examined several types of social connections over a sustained period of time.

Although treatment assignment in the underlying randomized controlled trial was randomized, and the analysis uses rigorous causal inference methods accounting for interference and the study design, network rewiring was not among the pre-planned primary outcomes for waves 1 and 3. However, network rewiring was a primary objective for the prospectively planned wave-4 data. Accordingly, our estimates are best interpreted as indicative evidence of causal effects. Nonetheless, the risk of false positives is limited by our suite of rigorous tests and robustness checks (Methods and Supplementary Information). Even under various strict corrections, the dyadic mechanism—that is, the dissolution of treated–untreated ties and the persistence of treated–treated ties—survives.

Another limitation is that we conducted this work in one setting, although our setting is a fairly typical example of village social life around the world and of places in which public-health and development interventions are undertaken. The effects on social connection of other types of intervention, with varied durations, topics or delivery means, might be different. Still, our work indicates that, in principle, face-to-face ties can be rewired by an informational intervention. In addition, we were limited in our ability to draw mechanistic conclusions about why the social ties were rewired. We had good measures of the knowledge that could have been fostered by the intervention, but not of other effects that it might have had, such as making particular, exogenously chosen villagers seem more trustworthy, for instance. Similarly, we can say that people became more ‘self-reliant’ (in the

structural sense of having fewer ties), but we cannot be sure how they themselves felt about this. Finally, we cannot know whether the rewiring we observe is beneficial or harmful to the village residents as a whole or to particular individuals. Future work could investigate whether the effects we report can be generalized to other settings and topics, or could examine the attitudes of villagers to the sorts of rewiring that we observe when educational interventions are implemented or when network effects are exploited for social contagion^{1,3,6,7,42}.

The intricate dynamics of social networks and the cascading effects of information provision that we observe offer the prospect of creating more effective interventions that might foster both positive change and social resilience within communities of diverse sorts. Underneath the enduring stability of social networks lies ongoing churn in who is connected to whom.

Online content

Any methods, additional references, Nature Portfolio reporting summaries, source data, extended data, supplementary information, acknowledgements, peer review information; details of author contributions and competing interests; and statements of data and code availability are available at <https://doi.org/10.1038/s41586-026-11154-7>.

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Methods

Honduras village cohort

We performed our trial in Honduras's Copán Department, covering more than 500 km² of challenging mountainous terrain. We started with a census in 176 villages (Extended Data Fig. 1). Recruitment was strong, with government data indicating that approximately 32,800 individuals were eligible. A total of $n = 30,422$ (93%) of these individuals participated in our census. From this population, 24,702 (81.2%) individuals (in 10,031 households in the 176 villages) enrolled in our longitudinal study. They committed to randomization, a 22-month intervention (with monthly visits from health workers, if selected), annual or biannual surveys and other data collection.

In the parent randomized controlled trial with all 176 villages, the primary objective was to evaluate a strategy to foster social contagion based on the 'friendship paradox', to which 66 villages were assigned¹. In the present study on network structural dynamics, we use data from the 110 villages that were assigned to the random-targeting arms of the randomized controlled trial, including data from 66 villages that were 'partially treated', at dosages of 5%, 10%, 20%, 30%, 50% and 75%; 22 villages that were fully treated (100% dosage); and 22 villages that were assigned to a control arm of no treatment (0% dosage).

From the individuals in these 110 villages, all residents aged 12 years or older in the study villages at baseline were eligible. No statistical methods were used to predetermine sample size; the analysis includes all eligible residents of the 110 villages in the random-targeting arms of the trial. The community health workers who delivered the intervention belonged to a separate organization and were blinded to the purpose of the study and to its randomization scheme; participants could not be blinded to receiving home visits. For the main analyses, we included respondents who (1) were enrolled and surveyed in both wave 1 and wave 3, and (2) did not change household or village between these waves. As a result, we analysed 8,331 individuals who resided in 4,513 households (for robustness checks, see Supplementary Information). For the mediation analyses that use wave 2, we also required survey completion at wave 2. For the wave-4 persistence analyses, we required eligibility and completion at wave 4 in 56 exogenously chosen villages. Full details are provided in the Supplementary Information.

Exogenous intervention

The maternal–neonatal–child health (MNCH) educational intervention was delivered by trained community health workers. The MNCH intervention included 15 educational modules aimed at maternal, child and neonatal practices that are typical of public-health or development-economics interventions, and it was delivered through the 'Timed and Targeted Counselling' method over 22 months (November 2016–August 2018). Content spanned folic acid use, prenatal care, birth planning, immediate and exclusive breastfeeding, neonatal thermal and cord care, diarrhoeal and respiratory disease prevention and management, paternal involvement, and modern family planning and delaying first pregnancy, among other topics. There were up to 22 1–2-h home visits tailored to family circumstances. Overall, 83% of targeted households received at least one visit. The median was 18 and the mean was 14 completed visits. Further details are provided in the Supplementary Information.

As part of the trial, the villages were randomized to several dosages (fractions of households receiving the educational intervention) and, according to the assigned dosage in each village, there was random assignment of households to the intervention. Here, exogenous intervention refers to treatment assignment being randomized and independent of existing social ties, which is essential for isolating causal effects. For the present analyses, we are explicitly contrasting low and high dosages (involving random targeting in 88 villages); that is, non-zero dosages are categorized into low (5%, 10%, 20% and 30%) and high (50%, 75% and 100%). Although the 100% dosage scenario could

be examined separately, because there are no untreated individuals, we have grouped it with the 50% and 75% dosage arm for two reasons: (1) the behavioural patterns in these high-dosage arms are similar, and (2) grouping them increases the statistical power of our results. This stratification is grounded in preliminary exploratory analysis using non-linear models (Loess regression and smoothing). A detailed explanation is provided in Extended Data Fig. 3.

For clarity, we categorize individuals on the basis of their treatment status and the treatment distribution in their villages. We merge treated individuals in fully treated villages (100% dosage) with treated individuals in partially treated villages (5%, 10%, 20%, 30%, 50% and 75% dosages) under the single term 'treated individuals'. We retain distinct categories for 'untreated individuals in partially treated villages' (5%, 10%, 20%, 30%, 50% and 75% dosages) and 'untreated individuals in fully untreated villages' (0%).

Network ascertainment

Using name-generator questions, we recorded diverse social relationships, including health advice, friendship, and financial relationships within each village in waves 1, 3 and 4 (the full list of name generators is provided in Supplementary Table 10). For the main analyses, we first analysed the health network (which consists of two questions: "Who would you ask for advice about health-related matters?" and "Who comes to you for health advice?"), and we allowed for directional ties.

In addition, we analysed multiplex layers, namely: (1) the friendship network (which consists of three questions: "Who do you trust to talk to about something personal or private?", "With whom do you spend free time?" and "Besides your partner, parents or siblings, who do you consider to be your closest friends?"); and (2) the financial network (which consists of two questions: "Who would you feel comfortable asking to borrow 200 lempiras from if you needed them for the day?" and "Who do you think would be comfortable asking you to borrow 200 lempiras for the day?"). The analysis allows us to attribute the changes of friendship and financial networks to the changes of health network. To ensure the validity and robustness of the changes in the friendship and financial networks, we also performed a complementary analysis examining the residual networks by removing the multiplex health ties from waves 1 and 3 from the friendship and financial networks (Supplementary Tables 57–60).

Permutation tests and causal effects

We performed a two-stage, cluster-randomized controlled trial. Villages were first assigned to dosage arms and, within treated villages, households were randomly selected for the 22-month health-education programme¹. We analyse our two-stage randomized saturation design under partial interference (interference allowed within, but not across, villages). An ego's network in our experiment can change because the ego was treated, or because others in their village (both near and far, geodesically speaking) were treated, or both. Disentangling these effects under the two-stage design depends on proper comparisons. 'Overall effects' are determined by comparing all individuals in the 88 partially and fully treated villages (5%, 10%, 20%, 30%, 50%, 75% and 100% dosages) with untreated individuals in the 22 fully untreated villages (dosage 0%). This captures the average effect of assigning different treatment dosages to people in a village on an individual's network structure, regardless of whether they or others in the village are treated. 'Total effects' are determined by comparing treated individuals in treated villages (dosage greater than 0%) with untreated individuals in the fully untreated villages, representing the average effect on changes in the network properties of being treated and being in a treated village versus being untreated in a fully untreated village (dosage 0%). 'Spillover effects' are established by comparing untreated individuals in treated villages with untreated individuals in untreated villages. These represent the average effect on network changes of being untreated and being exposed to a village dosage greater than

0%, versus being untreated in fully untreated villages (0%). Finally, the difference between the total effect and the spillover effect results in the ‘direct effect’, which compares treated individuals with untreated individuals in partially treated villages.

For overall, total and spillover effects, we first estimate the pooled effects of dosages higher than 0% versus 0%, and then we focus on the separate effects of low dosage (5–30%) versus 0% and high dosage (50–100%) versus 0%. Similarly, for direct effects, we first estimate the pooled direct effect of being treated versus untreated across all dosages, and then we investigate the direct effects within low-dosage villages and within high-dosage villages, separately. When the estimated total and spillover effects have the same sign as, and are similar in magnitude to, the overall effect, we focus on the overall effect for brevity.

To test whether the intervention altered node-level structure (total degree, in-degree and out-degree), we apply permutation tests designed for experiments with partial interference¹⁹, which permits the outcomes—that is, the network connections—for an individual to be affected by assignments within the same village, but not by assignments in other villages. This assumption is reasonable given the villages’ isolation and sparse intervillage ties. The test statistic is the studentized difference between village-level mean changes; effects are reported as percentage-scaled difference-in-differences. For each degree metric and for each causal effect, we take the change from wave 1 to wave 3 in the corresponding treated group, subtract the corresponding change in the corresponding control group and then divide this difference by the wave-3 mean of that metric in the comparison group. The estimated percentage-scaled effects, as illustrated in Fig. 2, are assessed against the randomization distribution using the permutation test. This framework provides exact *P* values under the sharp null of no treatment effect, while respecting the village-level randomization, the possibility of spillovers and the network’s interference structure⁴³.

The randomization distribution is generated by reassigning treatment labels according to the two-stage experimental design and assigning outcomes under the sharp null hypothesis of no effect, yielding design-based *P* values that respect interference and avoid the independence assumptions of standard parametric tests. The two-sided *P* values are computed on the basis of how extreme the observed statistics are relative to their null distribution. We use 10,000 permutations.

For the heterogeneity analysis, we use linear mixed-effects models with degree, in-degree or out-degree as the dependent variable and the treatment status as the key independent variable, including an interaction with the socio-demographic or wave-1 network property of interest. We also include household-level and village-level random effects. The treatment variable is defined as treated villages versus 0% villages (for overall effects); treated in treated villages versus untreated in 0% villages (for total effects); untreated in treated villages versus untreated in 0% villages (for spillover effects); and treated in treated villages versus untreated in treated villages (for direct effects).

We address the possibility of false discoveries using the Benjamini–Hochberg step-up procedure applied to pre-specified families⁴⁴. Following previous work^{45,46}, we also apply the Westfall–Young step-down procedure, which controls the family-wise error rate while accounting for the correlation among outcomes. We also report an Anderson summary-index analysis that consolidates the correlated graph statistics into one index per estimand ($m = 6$ within each estimand, and $m = 24$ across estimands)^{45,46}. The dyadic mechanism survives some of these (family-wise) corrections. Some node-level overall effects at low dosage hold under the FDR criterion but attenuate under family-wise control (Supplementary Information). For node-level and network-level tables, we adjust the six graph-metric *P* values (degree, in-degree, out-degree, betweenness, closeness and clustering) within each estimand (overall, total, spillover and direct; $m = 6$ per family). For the dyadic logistic models, each specification forms a family of the four exposure indicators {UU, TU, UT and TT} ($m = 4$). For the node-level tables, *P* values < 0.001 were set to 0.001 before adjustment; the dyadic tables use the exact

model *P* values. Adjusted *P* values are reported in Supplementary Information section 22, and do not alter our main conclusions.

Under multiple-testing correction, the dyadic mechanism is the most robust. The dissolution of treated–untreated ties and the persistence of treated–treated ties survive some family-wise (Westfall–Young) corrections: friendship persistence for UT and TU ties at both dosages and for TT ties at low dosage; health persistence for UT ties at low dosage and TU ties at high dosage; and friendship tie formation (TT) at low dosage. The node-level summary index confirms a dosage contrast—a selective loss of ties at a low dosage and a broad, spillover-driven increase in connectivity at a high dosage—whereas the low-dosage overall node-level effect nearly holds under the FDR criterion but does not survive family-wise control, consistent with the known conservatism of family-wise correction over mechanically related estimands.

Link analysis

Although we can show a one-to-one correspondence between dyadic effects and statistical causal effects on individual-level network properties (in particular, in-degree from treated individuals, in-degree from untreated individuals, out-degree to treated individuals and out-degree to untreated individuals), in the main text we report results for the link-formation analysis using dyadic mixed-effects logistic regressions, because they might be more intuitive (see Extended Data Tables 4–6 and Supplementary Tables 14–22 for methodological details). Specifically, to study tie persistence and formation, we fit mixed-effects logistic models on directed, within-village ego–alter pairs, with exposure encoded by dyad category $\in \{TT, TU, UT, UU, U_0U_0\}$ (reference category) and random intercepts for village, ego and alter to account for dependence:

$$\text{logit}(\Pr(Y_{ijv} = 1 | M_{ijv} = m)) = \gamma_{0m} + \gamma_{UU_m} UU + \gamma_{UT_m} UT + \gamma_{TU_m} TU + \gamma_{TT_m} TT + u_i + v_j + \xi_v,$$

where Y_{ijv} is the indicator for the existence of a tie between individuals i and j in village v in wave 3; UU, UT, TU and TT are the indicators for the treatment status of the dyad ijv ; u_i , v_j and ξ_v are ego-level, alter-level and village-level random effects; and $m = 1$ if the tie between individuals i and j in village v existed in the wave, and 0 otherwise.

These models provide odds-ratio estimates for each dyad category relative to U_0U_0 and align with node-level estimands through a mapping between dyadic flows and in-degree and out-degree components. Results from the corresponding permutation tests for the individual-level analysis show similar conclusions.

Mediation analysis

At wave 2, midway through intervention delivery, we surveyed 62 knowledge items (scored correct or incorrect; that is, 1 for correct and 0 for incorrect). For each respondent, we created baseline and midline knowledge scores by summing the items. The mediator is the change in knowledge, defined as the midline score minus the baseline score. Of the 8,331 individuals in the main analytic sample, 7,748 were present in all three waves (1, 2 and 3) and are included in the mediation analyses. The summed scores took values between 0 and 53 in wave 1 and between 0 and 58 in wave 2, with a potential minimum of 0 and maximum of 62.

For individual-level mediation, for each estimand (overall, total, spillover and direct), we estimate natural indirect (NIE) and natural direct effects with the *cmed* function of the R package CMAverse in a regression-based specification using imputation for the *g*-formula components and bootstrap inference²³. Outcomes and the mediator are modelled linearly, and the exposure corresponds to the individual-level and village-level treatment contrast appropriate for each estimand; for example, being treated versus being untreated in partially treated villages for direct effects. We report in the main text the NIE and the proportion-mediated effects; the natural direct effects are provided in the Supplementary Information. The models include the same

covariates as the primary analyses and omit exposure–mediator interaction in the main specification.

For dyadic mediation, with the dyad-level outcomes being the existence of a tie in wave 3 given its existence (tie persistence) or non-existence (tie formation) in wave 1, we specify a logistic model for the outcome and a multinomial model for the mediator. The dyadic mediator is a three-category variable; that is, none, one and both, indicating whether neither, exactly one or both members of the dyad increased knowledge between baseline and midline. The exposure is specified as the categorical variable defining the treatment status of the dyad; that is, TT, UT, TU, UU and U_0U_0 , with the latter being the reference category. We again estimate the natural direct and indirect effects using the `cmed` function of the R package `CMAverse`, using a regression-based approach and bootstrap inference, without exposure–mediator interaction. Dyad-level NIEs, summarized as a share of the total effects for TT, UT, TU, and UU links versus U_0U_0 , complement the individual-level results. In Supplementary Information section 18, we provide detailed information on the tuning of the functions and a description of the methods used for the mediation analysis.

Software

All analyses were performed in R (v.4.5.3) using Visual Studio Code (v.1.93.1); the complete package environment, with pinned versions, is specified in the deposited code.

Ethics statement

The underlying field trial was registered with ClinicalTrials.gov under the identifier NCT02694679. The Yale University institutional review board (IRB) and the Honduran Ministry of Health approved all procedures (protocol number 1506016012). The research was performed in accordance with all relevant guidelines and regulations, and all participants provided informed consent.

Reporting summary

Further information on research design is available in the Nature Portfolio Reporting Summary linked to this article.

Data availability

The data in this study were collected from identifiable human participants residing in small villages and include sensitive information about personal networks. The data are therefore subject to controlled access to comply with human-subject regulations and the confidentiality assurances given to participants. De-identified individual-level data, network edge list, and village-level metadata constituting the minimum dataset necessary to interpret, verify and extend the findings of this Article will be made available to qualified researchers upon reasonable request. Requests for access should be addressed to the corresponding

author (N.A.C.) and will be acknowledged within 14 days and receive a substantive response within 30 days. Access is conditional on the execution of a data-use agreement with Yale University that prohibits any attempt to re-identify participants, prohibits redistribution of the data and restricts use to the approved research purpose, and on IRB approval. There are no other restrictions on data use. Source data are provided with this paper.

Code availability

Source code for the data analysis and for reproducing the figures is available via Zenodo at <https://doi.org/10.5281/zenodo.22004783> (ref. 47).

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Author contributions Conceptualization: M.P., L.F., E.M.A. and N.A.C. Methodology: M.P., L.F., E.M.A. and N.A.C. Statistical analysis: M.P., L.F. and E.M.A. Writing: M.P., L.F., E.M.A. and N.A.C. Funding acquisition: L.F., E.M.A. and N.A.C.

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Competing interests E.M.A. is a Technical Fellow at Susquehanna International Group. The remaining authors declare no competing interests.

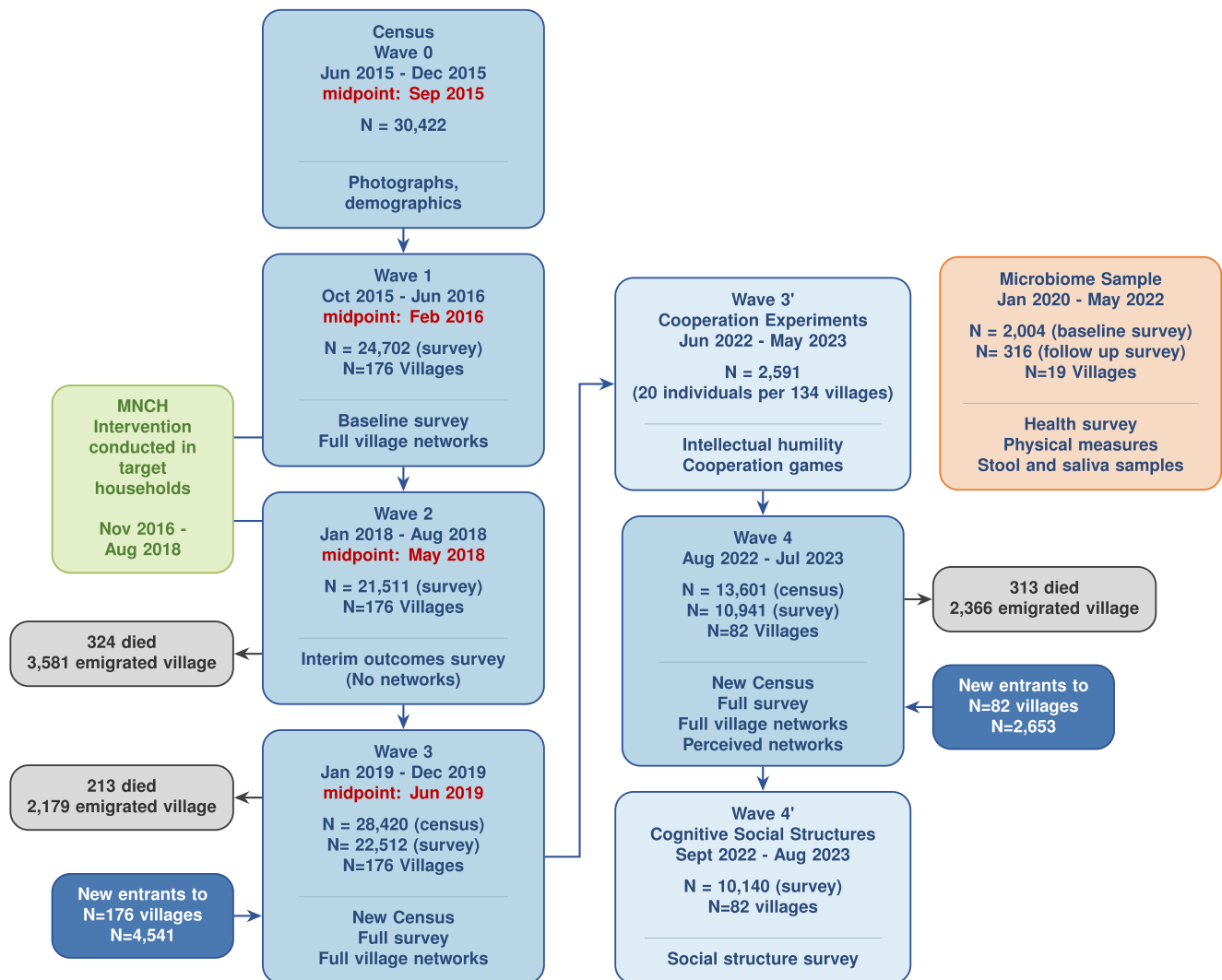
Additional information

Supplementary information The online version contains supplementary material available at <https://doi.org/10.1038/s41586-026-11154-7>.

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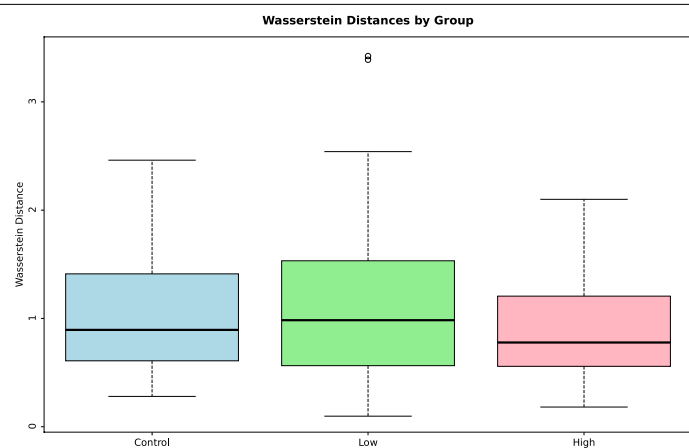
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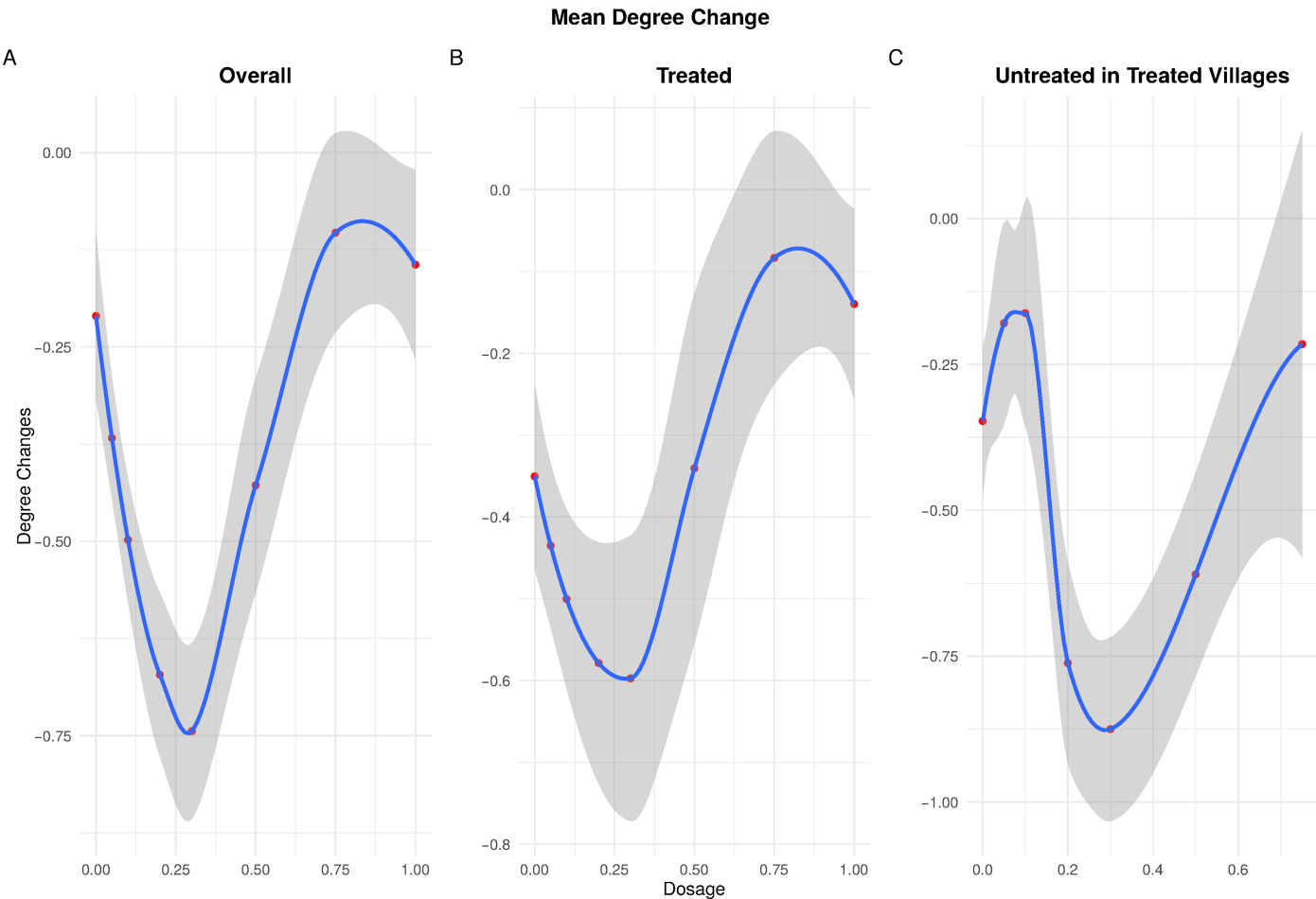


Extended Data Fig. 1 | Overview of data collection. Waves, dates, sample sizes, and variable types of the underlying cohort: wave 0 photographic census (Jun–Dec 2015; N = 30,422); wave-1 baseline sociocentric network survey (Oct 2015–Jun 2016; N = 24,702); wave-2 interim survey (Jan–Aug 2018; N = 21,511; no network mapping); wave-3 endline census and sociocentric network survey (Jan–Dec 2019; census N = 28,420; survey N = 22,512); wave 4 follow-up census

and survey in a subset of villages (Aug 2022–Jul 2023; N = 10,941; 56 villages from the random-targeting arms used here). Retention exceeded 80% across waves, with tracking of new entrants, births, deaths, and emigration. The MNCH intervention (November 2016–August 2018) began after baseline; waves 3 and 4 were conducted after the intervention.

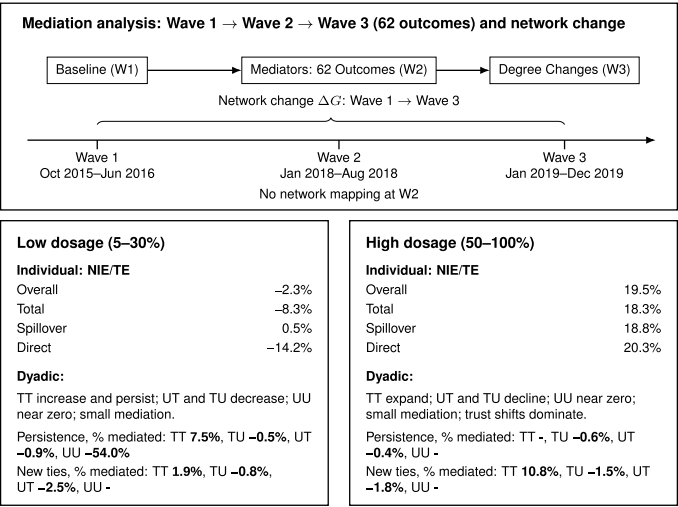


Extended Data Fig. 2 | Stability of village degree distributions between waves 1 and 3. Box plots of the 1-Wasserstein distance between the wave-1 (pre-treatment) and wave 3 (post-treatment) degree distributions of each village ($n = 22$ control villages, 44 low-dosage villages, and 44 high-dosage villages; boxes show the median and interquartile range, whiskers extend to $1.5 \times \text{IQR}$). Two-sided Welch two-sample t-tests found no significant differences between arms: control vs. low dosage, $t = -0.712$, $df = 52.6$, $p = 0.479$, 95% CI $[-0.472, 0.225]$; control vs. high dosage, $t = 1.113$, $df = 33.8$, $p = 0.274$, 95% CI $[-0.134, 0.460]$ – suggesting that there is no evidence that the degree-distribution change differs across treatment arms.



Extended Data Fig. 3 | Relationship between dosage and mean degree change in health-advice networks. a, Overall mean degree change. **b,** Mean degree change for treated individuals; **c,** Mean degree change for untreated individuals in treated villages (n = 8,331 individuals in 110 villages; control

group n = 1,659 individuals in 22 villages with 0% dosage). LOESS dose-response curves illustrate bimodal patterns, justifying the categorization into high (50–100%) and low (5–30%) dosages to reflect distinct behavioural responses across dosage ranges.



Extended Data Fig. 4 | Mediation analysis through wave-2 knowledge and results. Schematic and results of the causal mediation analysis linking wave-1 intervention assignment to wave-3 tie formation and persistence via the change in a summary score of 62 health-knowledge items between waves 1 and 2 (n = 7,748 individuals present in waves 1, 2 and 3; regression-based estimation with bootstrap inference, 1,000 resamples; dyadic NIE on the odds-ratio scale).

Extended Data Table 1 | Effects on node-level network properties in the health network: all dosages

	Overall	Total	Spillover	Direct
Degree	0.06% (0.977)	4.18% (0.143)	-3.62% (0.228)	7.78% (< 0.001)
In Degree	0.06% (0.983)	4.77% (0.226)	-4.15% (0.338)	8.92% (0.010)
Out Degree	0.06% (0.974)	3.59% (0.295)	-3.09% (0.363)	6.65% (0.017)
Betweenness	-22.51% (0.201)	0.43% (0.976)	-43.01% (0.026)	54.83% (0.005)
Closeness	-8.18% (0.005)	-8.30% (0.006)	-8.07% (0.005)	-0.22% (0.927)
Clustering	19.80% (0.010)	24.84% (0.004)	15.30% (0.065)	9.05% (0.155)

Each cell presents the percentage change and *P*values in treated groups (all individuals in fully and partially treated villages for the overall effect, treated individuals in fully and partially treated villages for the total and direct effects, untreated individuals in fully and partially treated villages for the spillover effects) relative to control groups (all individuals in untreated villages for the overall, total and spillover effects, and untreated individuals in fully and partially treated villages for the direct effects) at wave 3 within the health all-dosages network, including intra-household connections, calculated using two-sided permutation tests of a studentized village-level difference in mean change (10,000 permutations); percentages are relative to the wave-3 mean of the comparison group.

Extended Data Table 2 | Effects on node-level network properties in the health network: low dosages

	Overall	Total	Spillover	Direct
Degree	-6.36% (0.042)	-12.86% (0.007)	-5.09% (0.116)	-7.63% (0.111)
In Degree	-6.36% (0.149)	-13.35% (0.042)	-5.00% (0.266)	-8.18% (0.245)
Out Degree	-6.36% (0.072)	-12.38% (0.0287)	-5.18% (0.123)	-7.07% (0.170)
Betweenness	29.92% (<0.001)	32.18% (0.013)	29.48% (<0.001)	2.72% (0.795)
Closeness	-11.99% (<0.001)	-22.48% (<0.001)	-9.94% (<0.001)	-12.69% (0.004)
Clustering	7.24% (0.386)	-10.20% (0.456)	10.64% (0.211)	-19.43% (0.105)

Percentage changes and *P*values, defined as in Extended Data Table 1, for low-dosage (5–30%) villages, including intra-household connections.

Extended Data Table 3 | Effects on node-level network properties in the health network: high dosages

	Overall	Total	Spillover	Direct
Degree	7.21% (0.009)	7.97% (0.006)	3.82% (0.408)	4.54% (0.313)
In Degree	7.21% (0.063)	8.80% (0.025)	0.14% (0.976)	9.68% (0.137)
Out Degree	7.21% (0.028)	7.14% (0.038)	7.49% (0.149)	-0.37% (0.945)
Betweenness	22.92% (0.004)	21.87% (0.009)	27.58% (0.034)	-5.51% (0.626)
Closeness	-3.94% (0.176)	-5.14% (0.087)	1.37% (0.770)	-6.43% (0.154)
Clustering	33.83% (<0.001)	32.81% (<0.001)	38.35% (0.005)	-5.81% (0.654)

Percentage changes and *P*values, defined as in Extended Data Table 1, for high-dosage (50–100%) villages, including intra-household connections.

Extended Data Table 4 | Link dissolution in the health network: low dosages

Coefficient				
t	Estimate	Std. Error	z value	Pr(> z)
(Intercept)	-0.6756	0.0898	-7.524	5.31e-14 ***
UU	0.1183	0.1116	1.060	0.2891
TU	-0.2797	0.1479	-1.891	0.0586 .
UT	-0.4396	0.1581	-2.781	0.0054 **
TT	0.4228	0.1615	2.618	0.0089 **

Mixed-effects logistic regression (log-odds) for the persistence of directed ties existing at wave 1, with dyad categories UU, TU, UT and TT versus U_oU_o (reference) and random intercepts for village, ego and alter.

Extended Data Table 5 | Link formation in the health network:
low dosages

Coefficient	Estimate	Std. Error	z value	Pr(> z)
(Intercept)	-4.71550	0.15914	-29.631	< 2e-16 ***
UU	-0.06573	0.19609	-0.335	0.7375
TU	-0.38578	0.20588	-1.874	0.0610 .
UT	-0.38141	0.21134	-1.805	0.0711 .
TT	0.49074	0.21894	2.241	0.0250 *

Mixed-effects logistic regression (log-odds) for the formation by wave 3 of directed ties absent at wave 1, with dyad categories UU, TU, UT and TT versus U₀U₀ (reference) and random intercepts for village, ego and alter.

Extended Data Table 6 | Link dissolution in the health network: high dosages

Coefficient	Estimate	Std. Error	z value	Pr(> z)
(Intercept)	-0.67896	0.07357	-9.229	< 2e-16 ***
UU	0.14751	0.14681	1.005	0.3150
TU	-0.52599	0.16668	-3.156	0.0016 **
UT	-0.39848	0.16185	-2.462	0.0138 *
TT	0.13149	0.09450	1.391	0.1641

Mixed-effects logistic regression (log-odds) for the persistence of directed ties existing at wave 1, defined as in Extended Data Table 4, in high-dosage (50–100%) villages.