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Credibility-enhancing displays promote the provision of non-normative public goods

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Promoting the adoption of public goods that are not yet widely accepted is particularly challenging. This is because most tools for increasing cooperation—such as reputation concerns[1] and information about social norms[2]—are typically effective only for behaviours that are commonly practiced, or at least generally agreed upon as being desirable. Here we examine how advocates can successfully promote non-normative (that is, rare or unpopular) public goods. We do so by applying the cultural evolutionary theory of credibility-enhancing displays[3], which argues that beliefs are spread more effectively by actions than by words alone—because actions provide information about the actor's true beliefs. Based on this logic, people who themselves engage in a given behaviour will be more effective advocates for that behaviour than people who merely extol its virtues—specifically because engaging in a behaviour credibly signals a belief in its value. As predicted, a field study of a programme that promotes residential solar panel installation in 58 towns in the United States—comprising 1.4 million residents in total—found that community organizers who themselves installed through the programme recruited 62.8% more residents to install solar panels than community organizers who did not. This effect was replicated in three pre-registered randomized survey experiments (total $n = 1{,}805$). These experiments also support the theoretical prediction that this effect is specifically driven by subjects' beliefs about what the community organizer believes about solar panels (that is, second-order beliefs), and demonstrate generalizability to four other highly non-normative behaviours. Our findings shed light on how to spread non-normative prosocial behaviours, offer an empirical demonstration of credibility-enhancing displays and have substantial implications for practitioners and policy-makers.

Public goods are crucial to human welfare but pose a challenge when contributing is costly to the individual. Field experiments—which verify the conclusions of countless models and laboratory experiments—have demonstrated the power of reputation concerns and social norms for promoting contributions to public goods[4]. However, such interventions are typically only effective when most people already contribute to the public good in question (a descriptive social norm exists) or at least believe that people should contribute to it (an injunctive social norm exists)[2].

Here we investigate how to promote public goods that are not already normative—that is, how new prosocial norms can be spread. We focus on 'bottom-up' approaches in which individuals influence those around them, rather than 'top-down' approaches based on institutional sanctions or policies[5], and ask why some individuals are more successful than others in promoting the adoption of new prosocial norms. To shed light on this question, we leverage a theory from the study of cultural evolution that has primarily been used to explain religious commitment[6–8]: credibility-enhancing displays (CREDs)[3].

The essence of this theory is that your actions help to shape my second-order beliefs (that is, what I believe about what you believe), which in turn influences my adoption of your beliefs. In particular, the theory of CREDs focuses on actions that are expected to be beneficial to people who hold the belief but expected to be costly to people who do not hold the belief. If I see you engage in such an action, it provides a credible signal that you actually hold the belief and thus think the action is beneficial—a much stronger signal than if you simply say that you believe it. The canonical example involves assessing the edibility of a mushroom. If the mushroom is inedible, eating it can be extremely costly. Thus, seeing someone eat the mushroom after they say it is edible gives you much greater confidence that they truly believe it is safe to eat, relative to someone who merely says that the mushroom is edible—and this, in turn, makes you more likely to believe the mushroom is edible.

The logic of CREDs generates a clear prediction regarding bottom-up attempts to promote public goods: advocates who themselves engage in a given behaviour should be more effective at convincing others to also adopt that behaviour—specifically because they are perceived as believing that the behaviour is more beneficial. To test these predictions, we focus on one particular public-goods problem: the installation of residential solar panels. The use of residential solar panels helps to reduce carbon dioxide emissions and resultant climate change, and thereby benefits society at large. But the immediate financial cost of installation, combined with the search cost of learning about solar panels and suitable installers, may outweigh any personal benefit to the homeowner who chooses to install. The installation of solar panels remains descriptively non-normative (only 0.4% of American households had solar panels in 2014, during data collection; see Supplementary Information section 4.4 for details)[9] and—as shown by a norming study (see Methods and Extended Data Figs. 1, 2 for details)—there is also not currently a strong injunctive norm that stipulates that people should be installing solar panels.

We examine the role of CREDs in the ability of community organizers to promote the installation of solar panels by using data from a series of 'Solarize Connecticut' campaigns[10–12], run in 58 towns (with a total population size of 1.4 million) in the state of Connecticut by the non-profit organization SmartPower from 2012 to 2015. These campaigns promoted the Solarize programme, which included a volunteer 'solar ambassador' in each town who encouraged other residents to install solar panels through the programme (see Supplementary Information section 4 and Extended Data Fig. 3 for further details).

Ambassadors were recruited on the basis of their centrality in the community social network rather than their own solar installation choices. As a result, only a minority of the ambassadors (32.7%) themselves installed solar panels through the Solarize programme. Examining the number of Solarize installations achieved in each town confirms the key prediction of CREDs: more people installed solar panels through the Solarize programme in towns in which the ambassador also installed through Solarize, compared to towns in which the ambassador did not install through Solarize (Fig. 1; linear regression including controls for the type and timing of the Solarize campaign, $b = 17.89$, 95% confidence interval (CI) = 3.36–32.41, $P = 0.017$). This result is robust to controlling for important characteristics of the towns and the ambassadors: the number of residential solar panel installations

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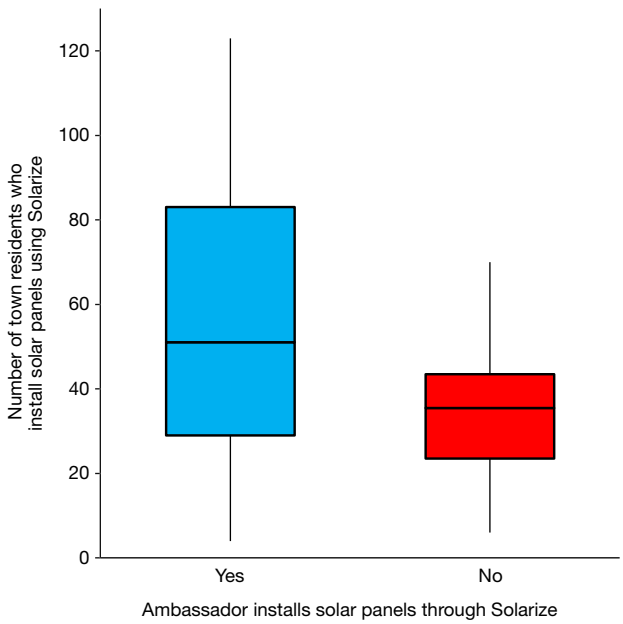

Fig. 1 | Ambassadors who install solar panels through the Solarize programme are more successful at convincing others to participate than ambassadors who do not. The number of people per town who installed solar panels using the Solarize programme is shown as a function of whether the solar ambassador of that town installed through the Solarize programme (blue, $n = 18$) or did not (red, $n = 40$). Box-and-whiskers plot indicates the minimum, 25th percentile, 50th percentile (median), 75th percentile and maximum values.

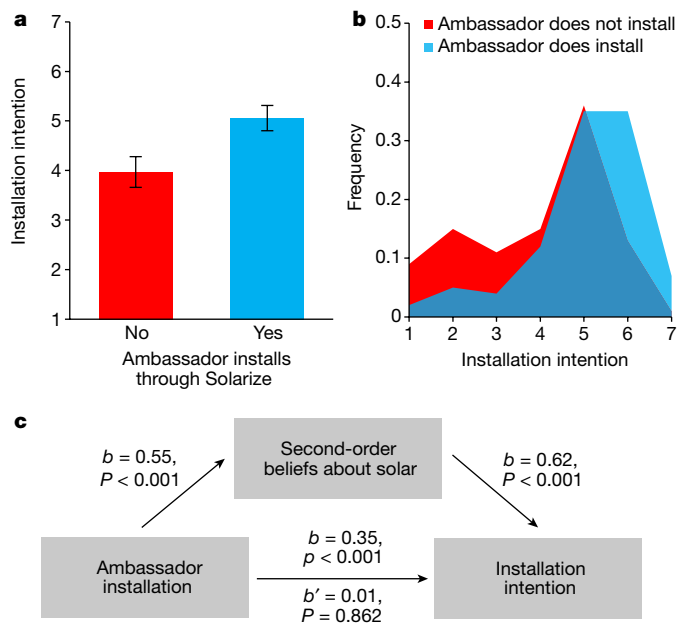

Fig. 2 | Ambassador installation influences subjects' intentions to install through the Solarize programme. a, **b**, Means (with 95% CIs) (**a**) and distributions (**b**) of intentions to install (1–7 Likert scale), as a function of whether the ambassador installed solar panels through the Solarize programme (blue, $n = 100$) or did not (red, $n = 100$). **c**, Subjects' second-order beliefs fully mediate the effect of ambassador installation on subjects' installation intentions. All variables are standardized for this analysis. The correlations between ambassador installation and second-order beliefs, second-order beliefs and subjects' intentions to install, and ambassador installation and subjects' intentions to install (without (b) and with (b') second-order beliefs as a covariate) are shown.

in the town before the Solarize campaign, the number of homes suitable for solar panel installation in the town, the gender of the ambassador, whether the ambassador served in an official town government role and whether the ambassador had already installed solar panels before the Solarize campaign (see Supplementary Information section 2 for further details).

To help to support a causal interpretation of this correlational finding, we perform an instrumental variable regression, which is a standard econometric technique for inferring causality from observational data (for details, see Supplementary Information section 3). We instrument for whether the ambassadors installed through the Solarize programme with a variable for whether the ambassador's home was suitable for solar installation. Given that ambassadors could only install through the Solarize programme if their house was suitable, suitability is a useful instrument: a test of suitability demonstrates that it is not a weak instrument (F ratio of 25.23) and it significantly predicts whether the ambassador installed using the Solarize programme ($b = 0.58$, 95% CI = 0.34–0.82, $P < 0.001$). We believe that suitability is a valid instrument because it is highly unlikely that suitability is correlated with potential unobserved confounding variables—such as ambassador motivation or installer quality—because suitability is based on predetermined features of the roof structure and shading of ambassadors' houses (for further discussion of validity, see Supplementary Information section 3). In our instrumented regression, we continue to find a significant positive effect of ambassador installation on the number of townsperson installations ($b = 23.82$, 95% CI = 1.77–45.88, $P = 0.034$), supporting our causal interpretation.

Thus, this field study supports our hypothesis based on CREDs: ambassadors who installed solar panels through the Solarize programme were more effective at convincing others to perform similar installations, when compared to those ambassadors who advocated installation without any accompanying action.

We complement this field study with three pre-registered experiments that were run using the online labour market Amazon Mechanical Turk, which is substantially more demographically diverse than undergraduate subject pools[13]. Of particular relevance, 52% of our subjects indicated that they were past or current homeowners, and all of our results replicate when restricting to these subjects (see Supplementary Information section 5.2).

Experiments 1 and 2 recreated the main contrast of the field study: subjects were presented with a description of the Solarize campaign, a description of a solar ambassador who did or did not choose to install solar panels through Solarize and an appeal from the solar ambassador that detailed the benefits of the programme. Subjects then indicated how likely they would be to install solar panels through the Solarize programme.

As in the field study, experiment 1 ($n = 200$ individuals) finds a significant effect of ambassador installation: subjects reported a higher likelihood of installing through the Solarize programme if the ambassador installed through Solarize ($m = 5.06$, 95% CI = 4.80–5.32) than if the ambassador did not ($m = 3.97$, 95% CI = 3.66–4.28, $t_{198} = 5.31$, $d = 0.75$, $P < 0.001$; Fig. 2a, b). Experiment 1 also provides an initial test of our prediction that this effect is driven by subjects' second-order beliefs (that is, their beliefs about what the ambassador believes about solar panels). To do so, we developed a 12-item second-order-belief scale ($\alpha = 0.96$), in which subjects indicated their beliefs about the ambassador's beliefs about the benefits of the Solarize campaign. As predicted, responses to the second-order-belief scale significantly and fully mediate the effect of ambassador installation on the subjects' intentions to install (97% of the effect; Fig. 2c) (see Methods and Supplementary Information section 5.1 for details).

Experiment 2 ($n = 399$ individuals) used an experimental mediation design[14] to provide further support for the key role of second-order beliefs in driving the effect of ambassador installation, and to rule out two competing explanations. We implemented a 2 × 2 between-subjects design that crossed the manipulation of whether the ambassador installed through the Solarize programme (from experiment 1) with a direct manipulation of second-order beliefs regarding the benefits of residential solar panels: subjects were informed about accidentally

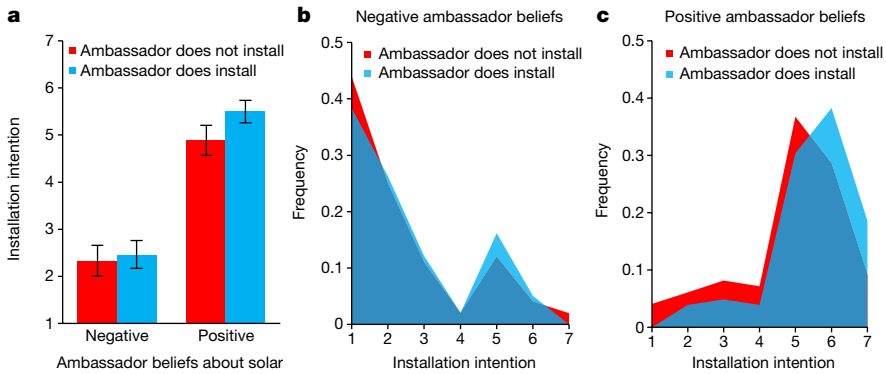

Fig. 3 | Second-order beliefs explain the effect of ambassador installation on the installation intentions of subjects. a, Mean (with 95% CI) installation intentions (1–7 Likert scale) as a function of whether the ambassador did not install solar panels through the Solarize programme and did not believe in the benefits of solar panels (red, left; $n = 100$), did install but did not believe in the benefits (blue, left; $n = 98$), did not install but believed in the benefits (red, right; $n = 99$), or did install and believed in the benefits (blue, right; $n = 102$). **b**, **c**, The distributions of installation intentions in each condition are shown. We see that when information about the ambassador's beliefs is provided directly in experiment 2, there is little effect of whether the ambassador installed on installation intentions of the subjects. This result is in contrast to experiment 1, providing evidence for the mediating role of second-order beliefs.

overhearing the ambassador express, in confidence, either a positive or negative view of residential solar panels. Thus, in experiment 2 subjects did not need to rely on the installation behaviour of the ambassador to gain insight into what the ambassador truly believed about solar panels—and thus, the CREDs account predicts that ambassador installation should have much less effect in experiment 2 compared to experiment 1. A two-way ANOVA finds a significant main effect of second-order beliefs such that subjects reported a higher likelihood of installing when the ambassador expressed a belief that residential solar panels are beneficial ($m = 5.20$, 95% CI = 5.01–5.39), compared to when the ambassador expressed a belief that residential solar panels are not beneficial ($m = 2.40$, 95% CI = 2.17–2.63, $F_{1,395} = 340.79$, $d = 1.83$, $P < 0.001$). There was also a significant, but small, main effect of ambassador installation such that subjects reported a slightly higher likelihood of installing if the ambassador also installed ($m = 4.00$, 95% CI = 3.71–4.30) than if the ambassador did not ($m = 3.60$, 95% CI = 3.31–3.88, $F_{1,395} = 6.08$, $d = 0.17$, $P = 0.014$) (Fig. 3). There was no significant interaction between ambassador installation and second-order beliefs ($F_{1,395} = 2.48$, $P = 0.116$). Critically, the coefficient on ambassador installation is 62% smaller in experiment 2 than in experiment 1, which provides causal evidence that second-order beliefs mediate the effect of ambassador installation (subjects were randomly assigned simultaneously across experiments 1 and 2 to enable this comparison; see Methods and Supplementary Information section 5.1 for details).

Experiment 2 also provides evidence for CREDs over two alternative explanations of the effect of ambassador installation that are based directly on actions rather than second-order beliefs. First, subjects might dislike or distrust the non-installing ambassador because their behaviour is hypocritical[15] and therefore ignore their recommendation to install through the Solarize programme[16]. Second, the ambassador's installation decision might directly influence subjects' count of the number of people who install and therefore influence their intention to install via perceived descriptive normativity[2]. Contrary to these accounts, however, subjects reported a higher likelihood of installing in the condition with an ambassador who did not install—and therefore was hypocritical and projected a norm of non-installation—but was overheard expressing a belief in the benefits of solar panels ($m = 4.89$, 95% CI = 4.59–5.19) than in the condition with an ambassador who installed—and was therefore not a hypocrite and projected a norm of installation—but was overheard to not truly believe in the benefits of solar panels ($m = 2.46$, 95% CI = 2.14–2.79, $t_{195} = 10.83$, $d = 1.54$, $P < 0.001$). Thus, when put in conflict, information about the beliefs of ambassadors overrides their actions.

Finally, experiment 3 ($n = 1{,}206$ individuals) replicated the design of experiment 1; however, solar panel installation was replaced by one of four other behaviours that are strongly non-normative from both a descriptive and injunctive perspective (see Methods and Extended Data Figs. 2, 3 for details). For each behaviour, we compare subjects' intention to engage in the behaviour across conditions in which the ambassador does versus does not engage in the behaviour. A random-effects meta-analysis on the four effect sizes reveals a significant positive effect of the ambassador engaging in the behaviour ($d = 0.33$, 95% CI = 0.21–0.44, $Z = 5.63$, $P < 0.001$), and no evidence of heterogeneity in effect size across behaviours ($\chi^2_3 = 2.37$, $P = 0.499$) (Fig. 4). Finally, aggregating over the four behaviours we find that the second-order-belief scale ($\alpha = 0.91$) significantly and fully mediates the effect of ambassador engagement (89% of the effect) (see Methods, Supplementary Information section 5 and Extended Data Fig. 4 for details). Thus, experiment 3 shows that the CREDs-based effect documented in the field study and experiments 1 and 2 can promote a range of highly non-normative public goods other than solar panels.

These results are of substantial importance for theories of cultural evolution, in which CREDs have a major role: despite the influence

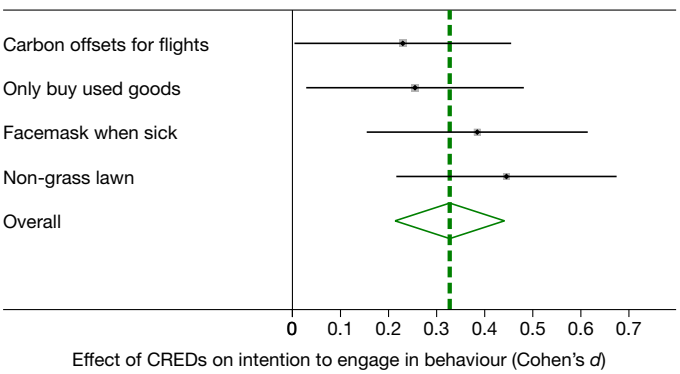

Fig. 4 | Ambassador engagement promotes contribution to highly non-normative public goods. Random-effects meta-analysis of the effect of the ambassador engaging in the behaviour they are advocating on subjects' intentions to engage in that behaviour (1–7 Likert scale). This was performed across four highly non-normative public goods: purchasing carbon offsets for flights ($n = 305$), only buying used goods ($n = 303$), wearing a facemask in public when sick ($n = 297$) and replacing grass lawns with more sustainable ground cover ($n = 301$). Effect sizes are shown as Cohen's d; error bars indicate 95% CI. The relative sizes of the grey boxes indicate the weighting assigned to the studies by the meta-analysis. ANOVA produces equivalent results (see Supplementary Information section 5.3).

that this theory has had, there has previously been little empirical evidence that directly supports CREDs—notable exceptions include two previous studies[7,17]—or that specifically demonstrates the mechanism of second-order beliefs. Our experiments provide such support. Furthermore, although it has previously been theorized that CREDs may help to explain prosocial behaviour more broadly[18], we apply the logic of CREDs to the spread of non-normative public goods in particular, which demonstrates an important role for this theory in solving one of the major outstanding challenges in cooperation research. Finally, we present an experimental methodology that can be used to empirically investigate the effects of CREDs in a wide range of contexts beyond that of public goods.

Our results also contribute to the literature on influence, persuasion and attitudes[19,20], as well as community organizing and the diffusion of solar panels in particular[10], by empirically demonstrating the importance of 'practicing what you preach'. Although this result might seem obvious in retrospect, the data suggest that it was not in fact self-evident in prospect: only 32.7% of solar ambassadors recruited as Solarize Connecticut community organizers were people who themselves installed residential solar panels through the programme.

Problems of cooperation and the provision of public goods are becoming increasingly important and urgent. The results presented here suggest that whether we are advocating for residential solar panels, public transportation, supporting local businesses or civil liberties, our campaigns will be more effective if they are built on a foundation not only of words but also of action.

Online content

Any methods, additional references, Nature Research reporting summaries, source data, statements of data availability and associated accession codes are available at https://doi.org/10.1038/s41586-018-0647-4.

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Author contributions K.G. and B.B. designed and conducted the field study. G.T.K.-T. and S.L. analysed the results of the field study. G.T.K.-T. and D.G.R. designed the online experiments. G.T.K.-T. conducted the online experiments and analysed the results. G.T.K.-T., B.B., K.G., S.L. and D.G.R. wrote the paper.

Competing interests The authors declare no competing interests.

Additional information
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METHODS

No statistical methods were used to predetermine sample size. The field study was not randomized, and the experiments were randomized. Investigators were not blinded to allocation during experiments and outcome assessment.

Field study. The Solarize Connecticut campaign was run in five rounds over 2012–2015 in 58 towns in Connecticut (see Supplementary Information section 4 for more detail on the rounds and round types). Solarize Connecticut was an initiative of the Connecticut Green Bank administered by the non-profit organization SmartPower (similar campaigns have also been conducted in New York, Massachusetts and Washington[12], and are underway in several additional states including North Carolina, South Carolina, Pennsylvania, Montana and California). There were four main marketing principles of the campaign: town-supported outreach and education, pre-selected solar installers, discount pricing through a tiered pricing structure and a clear termination date to the campaign. Solarize programmes were designed to limit the time and number of approved installers of residential photovoltaic systems or solar panels within a town.

The campaigns were organized by local volunteers—'solar ambassadors'—who were primarily recruited from town selectpeople, town managers and members of the town clean-energy task forces, because they were expected to be key nodes in the community social network, and therefore more influential. Before the campaigns, solar installation rates were extremely low among the towns studied ($m < 1\%$), so it is not surprising that most ambassadors did not have solar panels themselves before the campaign. Although many towns had multiple ambassadors, there was always a point person for SmartPower, who was the 'primary' ambassador. In this study, we focus on the one primary ambassador (identified by SmartPower, who were blind to the hypotheses tested here). In the field study, ambassadors in $n = 18$ towns installed solar through the Solarize programme and ambassadors in $n = 40$ towns did not. Because the ambassadors were recruited based on their centrality in the community social network rather than their own solar installation choices, it is not surprising that a majority of the ambassadors (67.3%) did not themselves participate in the Solarize programme.

Data from this field experiment originate from three sources: (1) Connecticut Green Bank recorded installations and their timing; and after the conclusion of the Solarize campaign, solar ambassadors were (2) sent an online survey and (3) interviewed in person. Connecticut Green Bank data were used to ascertain installations as the dependent variable in the analysis of the field study, and ambassador interviews and surveys were used to derive the individual difference measures of ambassadors that were the predictors in the field study. We obtained county-level data on the suitability of rooftops for solar photovoltaics from the company GeoStellar. Yale University's Institutional Review Boards reviewed the use of this data and approved it under protocol 1303011727.

Experiments. Our experiments were conducted using Qualtrics survey software and subjects were recruited using the crowdsourcing tool Amazon Mechanical Turk. Informed consent was obtained from all subjects and was approved by Yale University's Institutional Review Boards protocol 1307012383.

Experiments 1 and 2 were designed to capture the key features of the field study in a vignette context (see Supplementary Information section 6 for full experimental materials for each experiment). The vignettes began on screen 1 with an initial description that provided basic information about the Solarize Connecticut campaign. Then, screen 2 provided information about a hypothetical solar ambassador (for maximal similarity to the field study, subjects were only provided with information about the ambassador that would have been evident to a community member in conversation with an ambassador). At the end of screen 2, subjects in experiment 1 and experiment 2 were randomized to receive information that indicated that the ambassador did or did not install solar panels through the Solarize programme. Then, on screen 3 the ambassador gave subjects the 'pitch' for Solarize, which was copied from the Solarize Connecticut website (http://solarizect.com/about-solarize/). In experiment 2, subjects next saw an additional screen in which they learned about the ambassador's private beliefs as a result of being behind the ambassador in the checkout line at the grocery store and overhearing him speaking on the phone to his wife (this screen was not included in experiment 1). Subjects in experiment 2 were randomized to either learn that the ambassador's beliefs regarding solar panels were positive or negative. On the next screen, subjects in both experiments indicated their likelihood of participating in the Solarize programme using a seven-point Likert scale. This question is our dependent variable. Subjects in experiment 1 were then shown an additional screen with the second-order-belief scale that we developed for this study ($\alpha = 0.96$). The scale consisted of twelve items, each of which assessed different aspects of subjects' second-order beliefs regarding Solarize (see Supplementary Information section 6.1.2 for the full scale). Finally, subjects in both experiments completed comprehension and demographic questions—including whether they were a homeowner—and were thanked for their participation.

In summary, experiment 1 had two conditions (ambassador installed versus ambassador did not install) and experiment 2 had four conditions, in a 2 (ambassador installed versus ambassador did not install) × 2 (ambassador had positive beliefs about solar panels versus ambassador had negative beliefs about solar panels) design. To enable comparison of behaviour across experiment 1 and experiment 2, subjects for both experiments were recruited at the same time and were randomly assigned to one of the six total conditions. Our pre-registered target sample size was 100 subjects per condition, and we recruited $n = 200$ (64.5% female, average age = 34.6 years) for experiment 1 and $n = 399$ (61.1% female, average age = 34.6 years) for experiment 2. Subjects completed the survey in 7 min on average and were compensated 0.50 US dollars (US$).

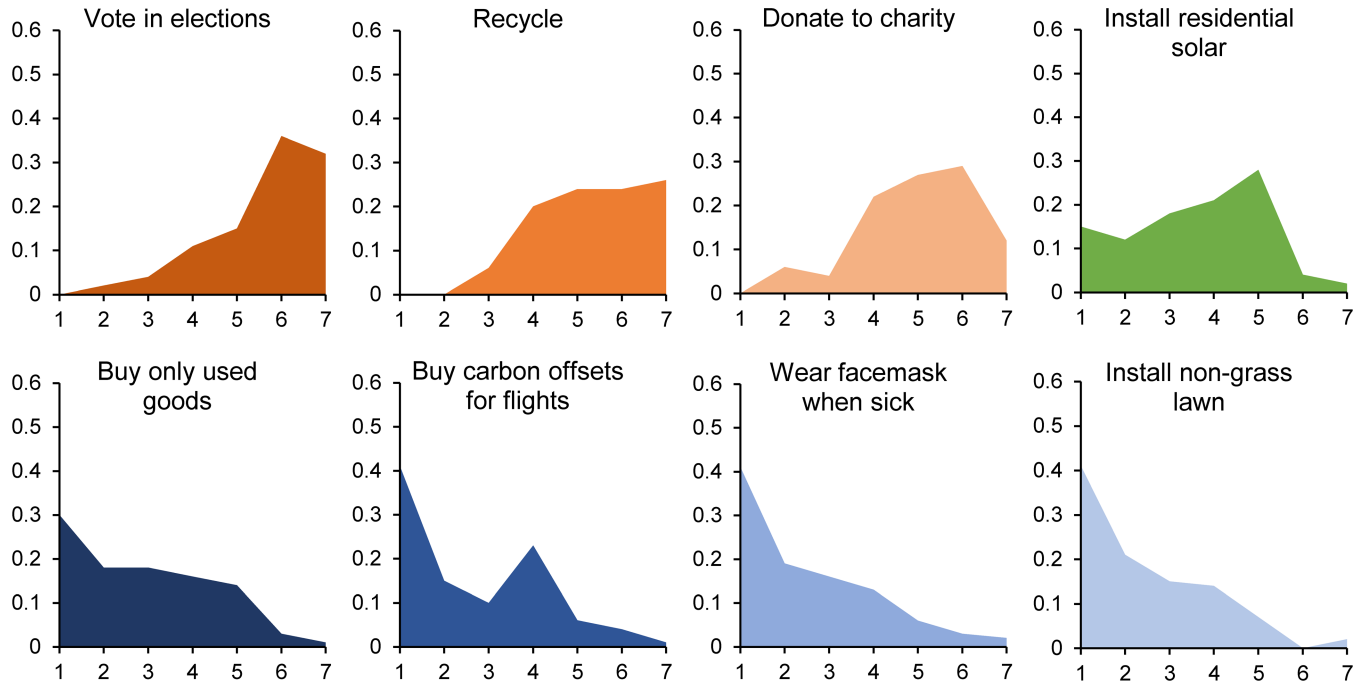
Experiment 3 aimed to test whether the results of the two-condition study of experiment 1 generalized to four highly non-normative behaviours. Thus, subjects were randomly assigned to one of eight between-subjects conditions in a 2 (ambassador engaged versus ambassador did not engage in the behaviour they were advocating) × 4 (behaviour being advocated: purchasing carbon offsets for flights, only buying used goods, wearing a facemask in public when sick and replacing grass lawns with more sustainable ground cover) design. The format of experiment 3 was identical to experiment 1, except that all text related to installing solar panels though Solarize was replaced with relevant text about one of the four behaviours. Full experimental materials are shown in Supplementary Information section 6.4. Our pre-registered sample size was $n = 150$ subjects per condition, and we recruited $n = 1{,}206$ subjects (63.3% female, average age = 34.8 years), who completed the survey in 5 min on average and were compensated US$0.50.

Subjects in the norming study were asked for their normativity judgments on various behaviours associated with contributions to public goods (presented in randomized order): wearing a face mask in public when sick with the flu or a cough; replacing grass lawns with more sustainable ground cover; buying carbon offsets for flights; buying only used consumer goods; installing residential solar panels; donating to charity; recycling; and voting in elections. For each behaviour (presented in randomized order), subjects were asked for their judgments of injunctive normativity, with the question: 'in your opinion, how much do people in your community think this behaviour is what you are supposed to do?'. Responses were given on a seven-point scale from 1 (very little) to 7 (very much). The same was performed for descriptive normativity, with the question: 'in your opinion, how many people in your community do this behaviour?' (responses from 1 (very few) to 7 (very many)). Our pre-registered sample size was $n = 100$, and we recruited $n = 100$ subjects (45% female, average age = 36.2 years), who completed the study in 4 min on average and were compensated US$0.50.

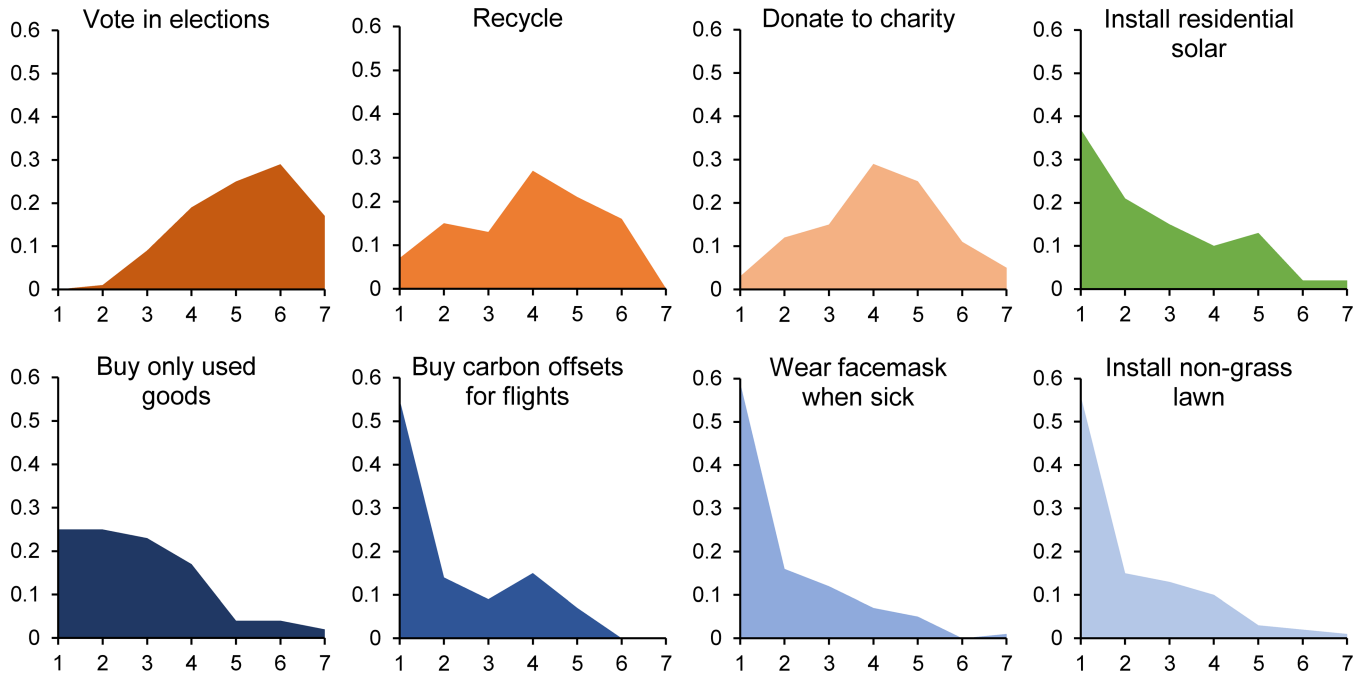
Reporting summary. Further information on research design is available in the Nature Research Reporting Summary linked to this paper.

Data availability

All data are publicly available at: http://osf.io/wbmjc.

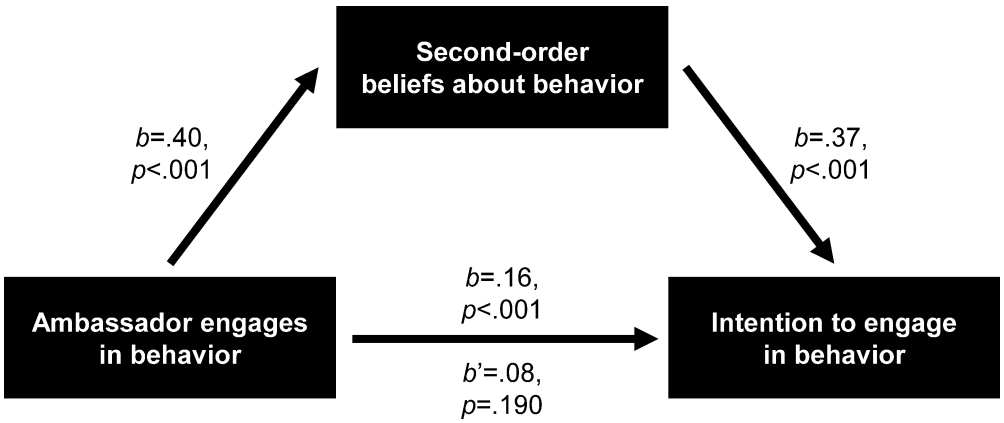


Extended Data Fig. 1 | Results of the norming study regarding injunctive norms. Distributions are shown of subjects' ($n = 100$) responses to the question: 'in your opinion, how much do people in your community think doing this behaviour is what you are supposed to do?'. Responses were given on a Likert scale between 1 ('very little') and 7 ('very much').



Extended Data Fig. 2 | Results of the norming study regarding descriptive norms. Distributions are shown of subjects' ($n = 100$) responses to the question: 'in your opinion, how many people in your community do this behaviour?'. Responses were given on a Likert scale between 1 ('very few') and 7 ('very many').

Extended Data Fig. 3 | Example photographs from Solarize campaigns. a–c, A live installation event (**a**), a campaign kick-off event (**b**) and flyers and signs for an informational event (**c**) are shown. Photographs courtesy of SmartPower.



Extended Data Fig. 4 | Mediation analysis for experiment 3. Because there is no evidence of heterogeneity in the effect of CREDs across non-normative public-good scenarios, we collapse across scenario (total $n = 1{,}206$) and see that subjects' second-order beliefs fully mediate the effect of ambassador engagement on subject intentions to engage in the behaviour in question. All variables are standardized for this analysis. The correlations between ambassador engagement and second-order beliefs, second-order beliefs and subjects' intentions to engage in the behaviour, and ambassador engagement and subjects' intentions to engage in the behaviour (without (b) and with (b') second-order beliefs as a covariate) are shown.

nature research

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When statistical analyses are reported, confirm that the following items are present in the relevant location (e.g. figure legend, table legend, main text, or Methods section).

n/a	Confirmed	
☐	☒	The exact sample size (*n*) for each experimental group/condition, given as a discrete number and unit of measurement
☐	☒	An indication of whether measurements were taken from distinct samples or whether the same sample was measured repeatedly
☐	☒	The statistical test(s) used AND whether they are one- or two-sided *Only common tests should be described solely by name; describe more complex techniques in the Methods section.*
☐	☒	A description of all covariates tested
☒	☐	A description of any assumptions or corrections, such as tests of normality and adjustment for multiple comparisons
☐	☒	A full description of the statistics including central tendency (e.g. means) or other basic estimates (e.g. regression coefficient) AND variation (e.g. standard deviation) or associated estimates of uncertainty (e.g. confidence intervals)
☐	☒	For null hypothesis testing, the test statistic (e.g. *F*, *t*, *r*) with confidence intervals, effect sizes, degrees of freedom and *P* value noted *Give P values as exact values whenever suitable.*
☒	☐	For Bayesian analysis, information on the choice of priors and Markov chain Monte Carlo settings
☒	☐	For hierarchical and complex designs, identification of the appropriate level for tests and full reporting of outcomes
☐	☒	Estimates of effect sizes (e.g. Cohen's *d*, Pearson's *r*), indicating how they were calculated
☐	☒	Clearly defined error bars *State explicitly what error bars represent (e.g. SD, SE, CI)*

Our web collection on statistics for biologists may be useful.

Software and code

Policy information about availability of computer code

Data collection	none
Data analysis	STATA 13

For manuscripts utilizing custom algorithms or software that are central to the research but not yet described in published literature, software must be made available to editors/reviewers upon request. We strongly encourage code deposition in a community repository (e.g. GitHub). See the Nature Research guidelines for submitting code & software for further information.

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Policy information about availability of data

All manuscripts must include a data availability statement. This statement should provide the following information, where applicable:

- Accession codes, unique identifiers, or web links for publicly available datasets
- A list of figures that have associated raw data
- A description of any restrictions on data availability

All data are publicly available at: http://osf.io/wbmjc

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☐ Life sciences ☒ Behavioural & social sciences ☐ Ecological, evolutionary & environmental sciences

For a reference copy of the document with all sections, see nature.com/authors/policies/ReportingSummary-flat.pdf

Behavioural & social sciences study design

All studies must disclose on these points even when the disclosure is negative.

Study description	Quantitative (all): observational (field study) and experimental (experiments)
Research sample	CT Town residents recruited via Solarize campaign (field study); adults recruited from Amazon Mechanical Turk (experiments) for convenience
Sampling strategy	Convenience, no sample size calculation was performed (field study); convenience, sample size calculation performed using pilot data (experiments) using the "power" command in STATA 13
Data collection	In-person interviews, Qualtrics survey administered via email, data recorded by CT Green Bank (field study); Qualtrics survey administered via Amazon Mechanical Turk (experiments)
Timing	2012-2015 (field study); 2017-2018 (experiments)
Data exclusions	None
Non-participation	No participants dropped out/declined participation
Randomization	Participants were randomly assigned to condition within each experiment.

Reporting for specific materials, systems and methods

Materials & experimental systems

n/a	Involved in the study
☒	☐ Unique biological materials
☒	☐ Antibodies
☒	☐ Eukaryotic cell lines
☒	☐ Palaeontology
☒	☐ Animals and other organisms
☐	☒ Human research participants

Methods

n/a	Involved in the study
☒	☐ ChIP-seq
☒	☐ Flow cytometry
☒	☐ MRI-based neuroimaging

Human research participants

Policy information about studies involving human research participants

Population characteristics	Field study (43.1% female), experiments (62.0% female, average age 34.8)
Recruitment	CT Town residents recruited via Solarize campaign (field study); adults recruited from Amazon Mechanical Turk (experiments)