Supporting Information

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SI Materials and Methods

Overview. In this study we used a generic field experimental design which we deployed in four distinct, ongoing, real-world social settings online: a crowd-funding website (kickstarter.com), a product review website (epinions.com), an open-source encyclopedia (wikipedia.org), and an online petitioning website (change.org). The experimental intervention involved the random allocation of one or multiple successes to individuals. In each setting the success was in a different form (a dollar amount, a positive rating, an award, or a signature), but the communality of design permits a unique comparative analysis across social systems. The key advantage of field experiments is that they combine the potential for causal inference found in laboratory experiments with the external validity typical of naturalistic observation, by studying people inside the social systems of interest without having to remove them into an artificial environment. This feature is particularly important for the focal phenomenon in this study—success breeds success—for which problems of confounding are very difficult to address in observational studies, as we elaborate in the main text.

Ethical Considerations. Intervention in ongoing social systems naturally raises ethical concerns, and throughout our experiments we navigated these issues with the utmost care. All experiments were approved by Stony Brook University's Human Subjects Committee (ID nos. 373335, 366647, 230771, and 442574) and had the additional backing of the National Science Foundation (Award nos. 1303522 and 1340122). We were careful to abide by the terms of use of the internet sites. The guiding principles of minimal harm and minimal disruption led us to restrict our samples by scale. For example, in the crowd-funding study we did not select projects with a funding goal of more than $5,000, and in the signatures study we selected only small petition campaigns with low signature goals. Additionally, as a research team we always acted in ways we could easily see ourselves act outside the experiments: We gave editing awards only to people who belonged to the 1% most prolific editors, we gave positive ratings only to good reviews and negative ratings only to bad reviews, and we sampled only petitions that sought no harm against a person or group. Finally, in the follow-up studies that increased the magnitude of the intervention, we refrained from intervening in two of the four settings (wikipedia.org and change.org) in which we thought that such intervention would lead to too severe a disruption. On wikipedia.org, we felt that we would not act within the spirit of the project if we gave editors more than one award within a short period, because receiving an award is a relatively rare event. On change.org a noticeable increase in our original intervention of 12 signatures by distinct signatories would have required many signatures per person and thereby would have violated the site's terms of use. For these reasons we increased the strength of the treatment in a second round of data collection only on kickstarter.com, where we increased the number of donors from one to four, and on epinions.com, where we increased the number of ratings from one to four.

SI Results

Site 1: Kickstarter.com. *Descriptive statistics.* There were two rounds of data collection. Round 1 focused on identifying the main effect of a donation on subsequent donations by third parties. The treatment involved a single donor donating a percentage of the funding goal to projects that had not raised any dollars 24 d before the funding deadline. Round 2 focused on identifying the relative effects of different numbers of donors. Here the treatment was either a donation of 1% by one donor or a total donation of 4% by four separate donors. For realism, in the case of four donors, we introduced a moderate variation in the size of donations across donors (e.g., one $15 donation, two $20 donations, and one $25 donation for a funding goal of $2,000). Table S1 shows the mean and median number of donations received and total dollar amounts raised during the 24-d observation period for both the experiment and the control conditions of both rounds, as well as the number of cases in each. The mean dollar amounts and mean number of donations in round 2 correspond to the averages reported in Fig. 3 *A* and *B*, respectively, in the main text.

Effect of treatment on subsequent fundraising (round 1). To produce the binary measure reported in Fig. 1 in the main text (which measures the immediate effect of the treatment on the rate of third-party giving, excluding effects between subsequent donations), we determined for the experimental and control condition of both rounds the number of cases in which no donations were made and the number of cases in which some additional donations were made by third parties. During the 24 d between the treatment and the deadline, 70% of subjects in the experimental condition and 39% of subjects in the control group received one or more third-party donations.. A χ^2 test shows a significant difference between conditions in the number of subjects receiving additional funding before the deadline ($\chi^2 = 19.4$; $P = 0.000$).

We also analyzed the effect of the treatment on cumulative measures of success that include possible second-order interdependencies among third-party donating events. Fig. S1*A* shows the distribution of the total number of dollars raised from third parties after the treatment, by condition, in round 1. A signed-rank test shows a significant difference between conditions in round 1 ($z = 4.13$; $P = 0.000$). Fig. S1*C* shows the distribution of the number of donations received from third parties after the treatment, by condition, in round 2. Again, a clear difference is visible between the experimental and control conditions ($z = 3.95$; $P = 0.000$).

Effect of treatment strength on subsequent fundraising (round 2). In the second round there also was a significant treatment effect on the percentage of cases experiencing at least one additional third-party donation during the 24-d observation period, but the strength of the treatment, which was varied in round 2, did not impact this percentage significantly. The percentages were 32%, 74%, and 87%, respectively, in the zero-donors, one-donor, and four-donors conditions. The difference between the zero-donors and one-donor conditions is significant ($\chi^2 = 11.0$; $P = 0.000$), as is the difference between the zero-donors and four-donors conditions ($\chi^2 = 19.4$; $P = 0.000$), but the difference between the one-donor and four-donors conditions is not significant ($\chi^2 = 1.65$; $P = 0.190$).

Fig. S1*B* shows the distribution of total numbers of dollars raised from third parties after the treatment, by condition, in round 2. The figure shows a large gap between the zero-donations and one-donation conditions, whereas the one-donation and four-donations conditions appear closer together. Signed-rank tests show that the difference in the total amount raised between the zero-donations and one-donation conditions in round 2 was statistically significant ($z = 3.02$; $P = 0.003$), as was the difference between the zero-donations and four-donations conditions ($z = 3.61$; $P = 0.000$), but the difference between the one-donation and four-donations conditions was not ($z = 1.70$; $P = 0.090$). Fig. S1*D* shows the distribution of the number of third-party donations after the treatment, by condition, in round 2. The number of donations clearly differs between the zero-donations and one-donation conditions, but the one-donation and four-donation conditions lie closer together. Indeed, the signed-rank tests find a difference between the zero-donors and one-donor conditions (z = 3.20; P = 0.001) and between the zero-donors and four-donors conditions (z = 4.16; P = 0.000) but not between the one-donor and four-donors conditions (z = 1.95; P = 0.051).

Goal amounts by round. Fig. S2*A* shows the distributions of funding goal amounts for rounds 1 and 2, respectively. In round 1 we set a maximum goal amount of $1,000, and in round 2 we increased the maximum goal amount to $5,000. Because in round 1 the pairs of treatment/control cases were matched in goal amount, the distribution of goal amounts is identical across the two conditions. Similarly, because in round 2 trios of cases were matched in goal amount, the distribution of goal amounts is identical across the three conditions.

Treatment effects on public enthusiasm. We measured expressions of enthusiasm through the number of Facebook likes accumulated at the end of each fundraising campaign. Fig. S2 *B* and *C* shows the distribution of these expressions in rounds 1 and 2 of the study, respectively, by condition. Large differences are visible in both graphs. In round 1 the difference between treatment and control is statistically significant (signed-rank test; z = 4.191; P = 0.000). In round 2, the difference between the one-donor condition and the zero-donors condition is significant (z = 3.35; P = 0.001), as is the difference between the four-donor condition and the zero-donors condition (z = 4.06; P = 0.000), but the difference between the one-donor and four-donors conditions is not (z = 1.59; P = 0.112). The significant treatment effects indicate that, despite the equal quality of projects across condition, random donations increased the level of enthusiasm for target projects, thus disconnecting public support from intrinsic merit.

Relationship between donations and public enthusiasm. We then predicted the incidence of donations from the treatment, controlling for the logarithm of the number of Facebook likes posted after the treatment, using negative binomial regression. Results are shown in Table S2. In models 2 and 4 the treatment effect loses a portion of the original effect size it had in models 1 and 3. In both round 1 and round 2, the treatment effect remains strong after controlling for the number of Facebook likes. These results suggest that a part of the treatment effect on subsequent donations was mediated by a greater level of public enthusiasm for treatment projects triggered through the intervention. We emphasize that the possibility of relevant unobservables prevents any hard conclusions about causal pathways and social mechanisms.

Effects of contribution percentage. In the first round, in which the treatment involved a single donor, we varied the size of this donor's contribution, donating either 1% of the funding goal (on average $6.77) or 10% of the funding goal (on average $66.76). Table S3 displays posttreatment third-party funding by the percentage donated. The percentage of projects with at least one posttreatment donation is significantly higher (χ^2 = 11.2; P = 0.001) when 1% is donated (68%) than when 0% is donated (39%) and also is significantly higher (χ^2 = 14.5; P = 0.000) when 10% is donated (72%) vis-à-vis the 0% condition but does not differ significantly between positive donations of varying magnitude (χ^2 = 0.191; P = 0.663). Similarly, the median number of third-party donations is significantly higher in projects that received donations of 1% than in projects that received no donation (rank-sum test; z = 3.42; P = 0.001) and in projects that received donations of 10% than in projects that received no donations (z = 4.02; P = 0.000) but does not differ between projects that receive donations of 1% and those that received a donation of 10% through the treatment (z = 0.583; P = 0.560). Also, the median number of dollars raised from third parties increases significantly in projects that received donations of 1% as compared with projects that received no donation (z = 3.30; P = 0.001) and in projects that received donations of 10% as compared with projects that received no donations (z = 3.74; P = 0.000) but again does not differ between projects that receive a 1% donation and those that receive a 10% donation through the treatment (z = 0.583; P = 0.672). These results indicate that potential third-party donors are sensitive to the presence of an initial donation but not to its size.

Site 2: Epinions.com. ***Descriptive statistics.*** We assessed the quality of new product reviews on epinions.com that as yet had received no ratings and categorized them as either "high quality" or "low quality." In case of a high-quality review, the treatment involved the application of one or more positive "very helpful" ratings to the review; in case of a low-quality review, the treatment involved the application of one or more negative "not helpful" ratings. Because only high-quality ratings are a form of success, the main text reports the results only for these cases. The data reported here come from two rounds of data collection. In the first round cases were assigned randomly to the zero-ratings or the one-rating condition; in the second round cases were assigned randomly to the zero-ratings, the one-rating, or the four-ratings condition. Below we combine these rounds of data collection for easier presentation, because we maintained the same rating procedure in the second round and find no differences between rounds. Table S4 shows the mean and median number of positive ratings received by high-quality reviews and the mean and median number of negative ratings received by low-quality reviews for the two experimental conditions and the control condition during the 14-d observation period as well as the number of cases in each condition.

Effect of treatment on subsequent ratings received. To produce the binary measure reported in Fig. 1 in the main text (which measures the immediate effect of the treatment on the rate of third-party ratings, excluding effects between subsequent ratings), we determined the number of cases in the one-rating and zero-ratings conditions in which no positive ratings were given after the treatment and the cases in which additional positive third-party ratings were given. During the 14 d immediately following the treatment, 90% of subjects in the one-rating condition and 77% of subjects in the zero-ratings condition received one or more positive third-party ratings (Fig. 1). A χ^2 test shows a significant difference between conditions in the percentage of subjects receiving one or more positive ratings 14 d after the treatment (χ^2 = 9.54; P = 0.002). In the negative-rating experiment, the percentage of cases with one or more negative third-party ratings was 50% in the experimental conditions and 16% in the control condition (χ^2 = 11.5; P = 0.001).

We also analyzed the effect of both treatment conditions on total positive and total negative ratings received (a measure that includes possible second-order interdependencies among third-party rating events). As we report in the main text, we find that the treatment had a significant effect on the number of positive ratings accumulated after 14 d by high-quality reviews (z = 3.21; P = 0.001), which were 11.4 in the zero-ratings condition and 14.9 in the one-rating condition. Analogously, we find that the treatment also had a significant effect on the number of negative ratings accumulated after 14 d by low-quality reviews (z = 3.44; P = 0.001), which were 0.581 in the zero-ratings condition and 2.40 in the one-rating condition.

Effect of treatment strength on subsequent ratings received. Fig. S3*A* combines these response percentages for high-quality reviews in the positive rating experiment with those found for low-quality reviews in the negative rating experiment. The difference in the percentage of high-quality reviews that received one or more positive ratings between the two treatment conditions was not significant (χ^2 = 0.304; P = 0.582), nor was there a significant difference in the percentage of low-quality reviews that received one or more negative ratings (χ^2 = 0.181; P = 0.670). Fig. S3*B* shows for each condition the change in the average number of positive ratings given to high-quality reviews in the positive ratings study and the average number of negative ratings given to low-quality reviews in the negative ratings study. Fig. S4 *A* and *B* shows the distribution of total numbers of positive and negative ratings received from third parties after the treatment, by condition. The high-quality reviews that received no positive ratings from us received significantly fewer third-party positive ratings during the observation period than did the reviews that received one positive rating during treatment ($z = 3.21$; $P = 0.001$) but not significantly fewer than those reviews that received four positive ratings during treatment ($z = 1.83$; $P = 0.067$). The reviews to which we gave four positive ratings and those to which we gave one positive rating do not differ significantly ($z = 1.07$; $P = 0.283$) in the number of subsequent positive third-party ratings. The low-quality reviews to which we gave no negative ratings received significantly fewer negative third-party ratings during the observation period than did the reviews to which we gave one negative rating ($z = 3.44$; $P = 0.001$) and also received fewer negative ratings than the reviews to which we gave four negative ratings ($z = 4.14$; $P = 0.000$). There is no difference in the number of negative third-party ratings between the reviews to which we gave four negative ratings and those to which we gave one negative rating ($z = 1.06$; $P = 0.288$).

Effect of treatment on positivity level of subsequent ratings. It is important to consider the possibility that, instead of encouraging third parties to give a review a rating similar to ours, our treatment simply increased the overall number of ratings given by third parties. In the latter case, the volume of ratings would have increased, but not necessarily how positive the ratings are. Accordingly, we calculated total number of ratings that were dissimilar to the rating applied through the treatment. For the high-quality reviews, where we applied no, one, or four "very helpful" ratings, we look at effects on the number of ratings worse than "very helpful" (i.e., the number of "helpful," "somewhat helpful," and "not helpful" ratings) received after the treatment. For the low-quality reviews, to which we applied no, one, or four "not helpful" ratings, we examine effects on the number of ratings better than "not helpful" (i.e., "very helpful,, "helpful," and "somewhat helpful" ratings) received after the treatment. If our treatment had merely increased the incidence of ratings of any kind without increasing the overall rating level, we would find increased frequencies of these other ratings in the experimental conditions vis-à-vis the control condition. Table S5 shows that, instead, the treatment consistently reduced the number of other ratings given by third parties. For high-quality reviews, the difference in the number of other ratings is significant between the zero-ratings and the one-rating conditions (rank-sum test; $z = 2.71$; $P = 0.007$) and between the zero-ratings and the four-ratings conditions ($z = 2.56$; $P = 0.009$) but is not significant between the one-rating and the four-ratings conditions ($z = 0.417$; $P = 0.677$). Similarly, for low-quality reviews, the difference in the number of other ratings is significant between the zero-ratings and the one-rating conditions ($z = 2.001$; $P = 0.045$) and between the zero-ratings and the four-ratings conditions ($z = 2.76$; $P = 0.006$) but is not significant between the one-rating and the four-ratings conditions ($z = 1.32$; $P = 0.186$). Taken together, Tables S4 and S5 thus show that one or more initial positive ratings increased the number of subsequent positive ratings and reduced the number of negative ratings given to high-quality reviews and, analogously, that one or more initial negative ratings increased the number of subsequent negative ratings and reduced the number of positive ratings given to low-quality reviews.

Site 3: Wikipedia.org. ***Descriptive statistics.*** Table S6 shows the mean and median number of awards received during the 90-d observation period in both the experiment and the control condition as well as the number of cases in each. We provide data for both 30 d and 90 d after the treatment to give the reader a sense of how effects changed with time over the course of the much longer observation period in this study.

Effect of treatment on awards received. To produce the binary measure reported in Fig. 1 in the main text (which measures the immediate effect of the treatment on the rate of success, excluding effects between subsequent awards), we determined the number of cases for each condition in which no additional awards were given and the remaining cases in which some additional awards were given by third parties. During the first 30 d after the treatment, 22% of editors in the experimental condition and 13% of editors in the control condition were subsequently given one or more awards from other users not involved in the experiment. A χ^2 test shows that this difference is significant ($\chi^2 = 7.18$; $P = 0.007$). After 90 d, these percentages had risen to 40% in the experimental condition and 31% in the control condition ($\chi^2 = 4.72$; $P = 0.030$); these are the percentages and significance levels reported in Fig. 1.

We also analyzed the effect of the treatment on the total number of awards received (a measure that includes possible second-order interdependencies among awarding events). Fig. S5*A* shows the distribution of awards received from third parties 90 d after the treatment, by condition. A rank-sum test shows a significant difference between the distributions ($z = 1.982$; $P = 0.048$).

Pretreatment awards by condition. Fig. S5*B* shows the cumulative distribution of awards received during the 30 d before the treatment. The minor differences between the curves indicate that randomization succeeded in balancing the two conditions reasonably in terms of pretreatment awarding. A rank-sum test shows no significant difference between the distributions of awards before treatment ($z = 0.991$; $P = 0.322$). Table S7 shows the results of logistic regression models predicting the likelihood of posttreatment awards, with and without controlling for pretreatment awards, 30 and 90 d after the treatment. The treatment effect is significantly positive throughout the four models shown, demonstrating that random differences in pretreatment awards across conditions do not impact the conclusion with respect to a cumulative advantage effect in award accumulation. Because 13% of editors in the control condition received more than one award after the treatment, Table S7 also shows the results of negative binomial regression models predicting the total number of posttreatment awards. The significant effect of the treatment in model 1 continues to be statistically significant in model 2 once pretreatment awards are controlled. In model 3 the treatment effect falls short of significance, but once pretreatment awards are controlled in model 4, the treatment effect becomes significant.

Effect of treatment on productivity. We measured posttreatment productivity as the number of edits to Wikipedia article pages made by editors during the 90-d observation period following the treatment and measured pretreatment productivity as the number of edits during the 30 d preceding the treatment. Fig. S5*C* shows the distribution of posttreatment productivity, by condition. The difference in posttreatment productivity is significant (rank-sum test; $z = 2.91$; $P = 0.004$), indicating that the treatment raised editors' productivity levels. Fig. S5*D* shows the distribution of pretreatment productivity, by condition. There is no significant difference in productivity before the treatment ($z = 0.516$; $P = 0.606$), indicating that randomization succeeded in balancing the two conditions in terms of productivity.

Relationship between awards and productivity. The positive effect of the treatment on productivity found in Fig. S5*C* raises the possibility that the mechanism driving the feedback effect in awarding was an intensification of editing behavior by award recipients, which in turn may have generated a merit-based response in awarding by third parties. To evaluate whether the treatment directly produced the observed increase in the likelihood of another award, rather than generating it indirectly through an increase in productivity, we predicted the probability of a posttreatment award from the treatment, controlling for posttreatment productivity, using logistic regression. Posttreatment productivity is measured in thousands of edits so that effect size and SE can be reported in regular decimal representation. Results are shown in Table S8. The significantly positive effect of productivity on awards in models 2 and 4 reflects the natural correlation between productivity and awarding that one would expect in a meritocratic system. The significantly positive effect of the treatment in models 1 and 3 reflects the main treatment effect identified earlier. In both models 2 and 4 the treatment effect maintains most of the original effect size it had in models 1 and 3, respectively, although in model 4 the significance level drops below the 95% confidence level. To evaluate if a treatment effect on the number of awards exists net of posttreatment productivity, we predicted the number of posttreatment awards 30 and 90 d after the treatment using negative binomial regression. The results are also shown in Table S8. We find that the treatment maintains most of its effect after controlling for productivity, both 30 d after the treatment (model 2) and 90 d after the treatment (model 4). Treatment effects after 30 d are significant (models 1 and 2), whereas after 90 d the effects fall short of significance, with or without the controlling effect of productivity. These results indicate that in large part the treatment directly affected award-giving behavior by third parties through the increased productivity of recipients. However, the possibility of relevant unobservables prevents any hard conclusions about causal pathways and social mechanisms.

Site 4: Change.org. ***Descriptive statistics.*** Table S9 shows the mean and median number of signatures received during the observation period of 2 wk in both the experiment and the control condition and the number of cases in each.

Effect of treatment on signatures received. The binary measure reported in Fig. 1 in the main text (which measures the immediate effect of the treatment on the rate of signatures, excluding effects between subsequent signatures) is based on a dichotomization of the posttreatment signature count contrasting cases in which some additional signatures were given and the remaining cases in which no additional signatures were given by third parties. During the 14 d after the treatment, 66% of subjects in the experimental condition subsequently received one or more signatures from third-party signatories, compared with 52% of subjects in the control group. A Pearson χ^2 test for independence shows a significant difference between conditions in the number of subjects receiving signatures 14 d after the treatment ($\chi^2 = 4.05$; $P = 0.044$).

We also analyzed the full effect of the treatment on total signatures received (a measure that includes possible second-order interdependencies among signatures). Fig. S6*A* shows the distribution of signatures solicited from third parties after the treatment, by condition. Fig. S6*A* indicates that signature totals in the experimental conditions were higher, but a rank-sum test shows that this difference falls short of statistical significance ($z = 1.76$; $P = 0.079$). Table S10 reports results from negative binomial regression models predicting total posttreatment signature counts from the treatment, controlling for pretreatment signature counts. The significantly positive effect of prior signatures on subsequent signatures in model 2 reflects the uncontrolled relationship between past and future support, which need not reflect a success-breeds-success effect because it may be produced spuriously by differences between campaigns in natural support base. The regression models find a positive treatment effect in model 1 which becomes statistically significant once orthogonal variance from pretreatment signatures is controlled in model 2, confirming the presence of a success-breeds-success effect on signature totals.

Goal amount. Fig. S6*B* shows the distribution of signature goal amounts by condition. Some imbalance is visible, with more petitions in the experiment condition than in the control condition having the typical, low goal amount of 100 signatures, although this difference is not statistically significant ($\chi^2 = 2.32$; $P = 0.128$). We found that higher goal amounts were associated significantly with greater numbers of posttreatment signatures (rank-sum test; $z = 2.00$; $P = 0.046$), suggesting that the slightly lower goal amounts in the treatment condition may have had a potential suppressing effect on signature totals. We explored the impact of this difference in goals on the treatment effect through negative binomial regression, reported in Table S10, model 3. A comparison with model 2 in Table S10 shows that the treatment effect indeed increases somewhat once the goal amount is controlled, thus confirming that the modest imbalance in the goal variable across conditions does not affect the conclusion about the presence of a success-breeds-success effect in signature accumulation.

Effect of treatment on public enthusiasm. To investigate whether the success effect in change.org produced differential expressions of enthusiasm about the campaigns across conditions, despite equivalence of expected project quality (because of randomization), we counted the number of supporting comments left on each campaign page. Fig. S6*C* shows the distribution of the number of supportive comments by condition. It shows that the number of comments in the treatment condition tended to be higher, but a rank-sum test identifies no significant difference ($z = 0.184$; $P = 0.854$).

Relationship between signatures and public enthusiasm. We then predicted the incidence of posttreatment signatures from the treatment, controlling for the number of supportive comments posted after the treatment, using negative binomial regression. The number of supportive comments was logged to correct for extreme variable skew. Results are shown in Table S10, model 4. The significantly positive effect of supportive comments on signatures in model 4 reflects the natural correlation between support in words and support in action. In model 4, the treatment effect maintains most of its original size in model 3 and remains significant. These results suggest that subsequent signatures were added in part because of increased public enthusiasm stemming from our intervention but that mostly it was the actual signatures we added that directly triggered subsequent signatures. We emphasize once more that the possibility of relevant unobservables prevents any hard conclusions about causal pathways and social mechanisms.

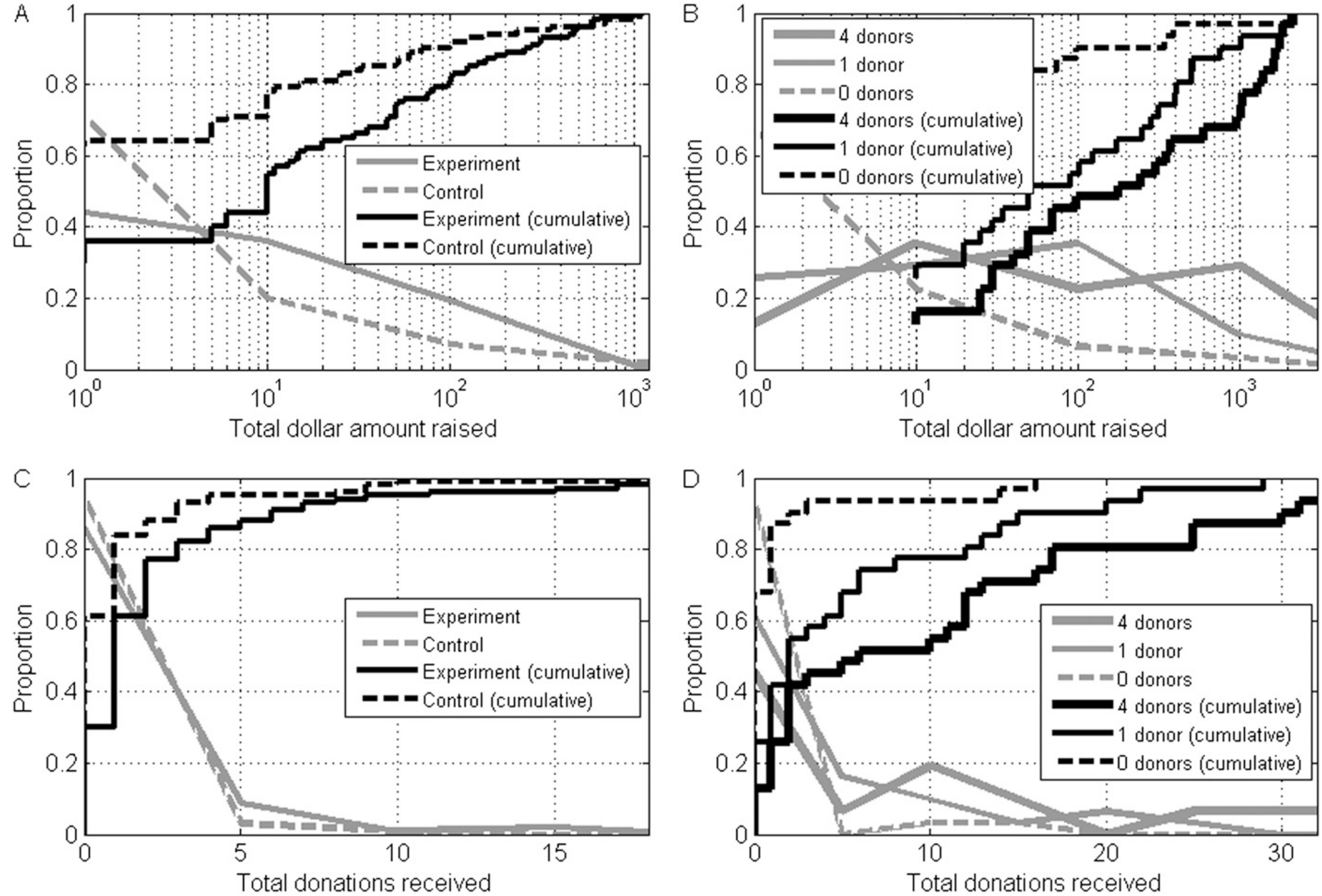

Fig. S1. Distributions of dollars and donations in the crowd-funding study. (*A*) Distribution of dollars raised in round 1, by condition. (*B*) Distribution of dollars raised in round 2, by condition. (*C*) Distribution of donations received in round 1, by condition. (*D*) Distribution of donations received in round 2, by condition.

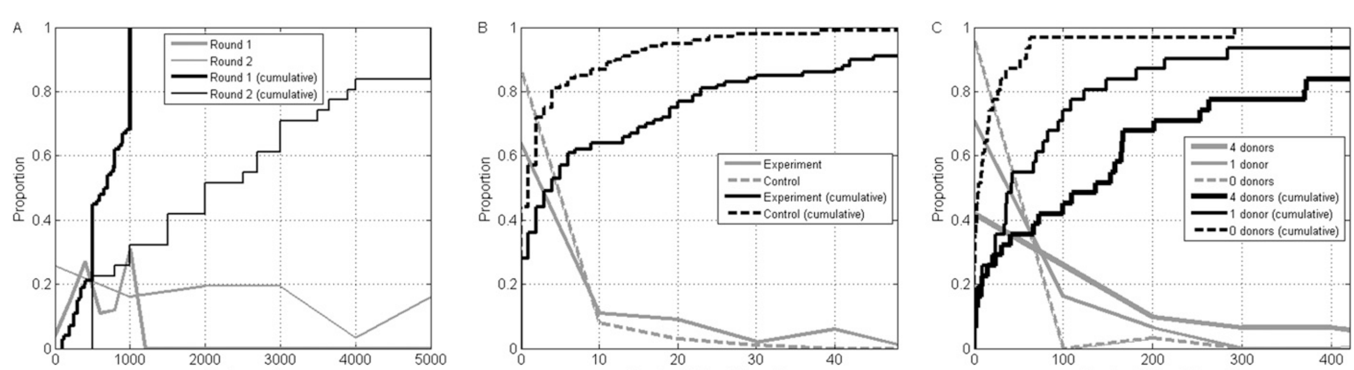

Fig. S2. Distribution of funding goal amount and Facebook likes in the crowd-funding study. (*A*) Distribution of funding goal amount in rounds 1 and 2. (*B*) Distribution of Facebook likes in round 1, by condition. (*C*) Distribution of Facebook likes in round 2, by condition.

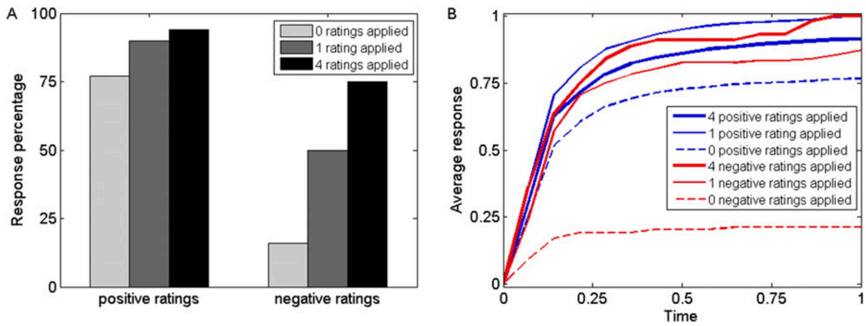

Fig. S3. Percentage and average running number of positive and negative ratings. (*A*) Percentage of high-quality reviews with one or more positive posttreatment ratings and percentage of low-quality reviews with one or more negative posttreatment ratings, by condition. (*B*) Average running number of positive ratings given to high-quality reviews and average running number of negative ratings given to low-quality reviews, over time and by condition. The horizontal axis is normalized so that 0 marks the time of experimental intervention, and 1 marks the end of the observation period. The vertical axis is normalized so that a value of 1 equals the maximum for each system across time and conditions.

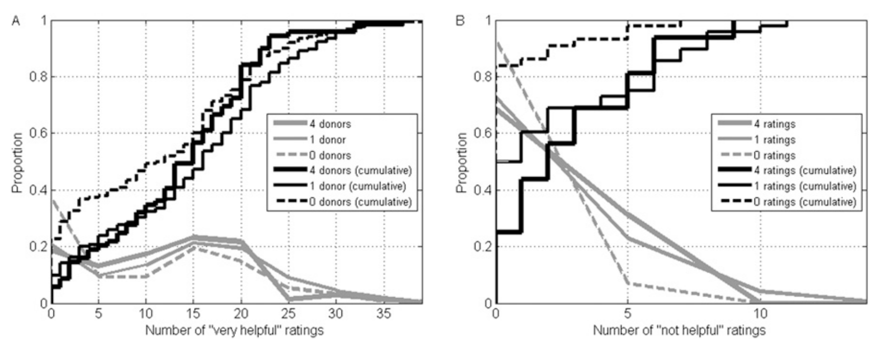

Fig. S4. Distribution of positive and negative ratings. (*A*) Distribution of positive ratings, by condition. (*B*) Distribution of negative ratings, by condition.

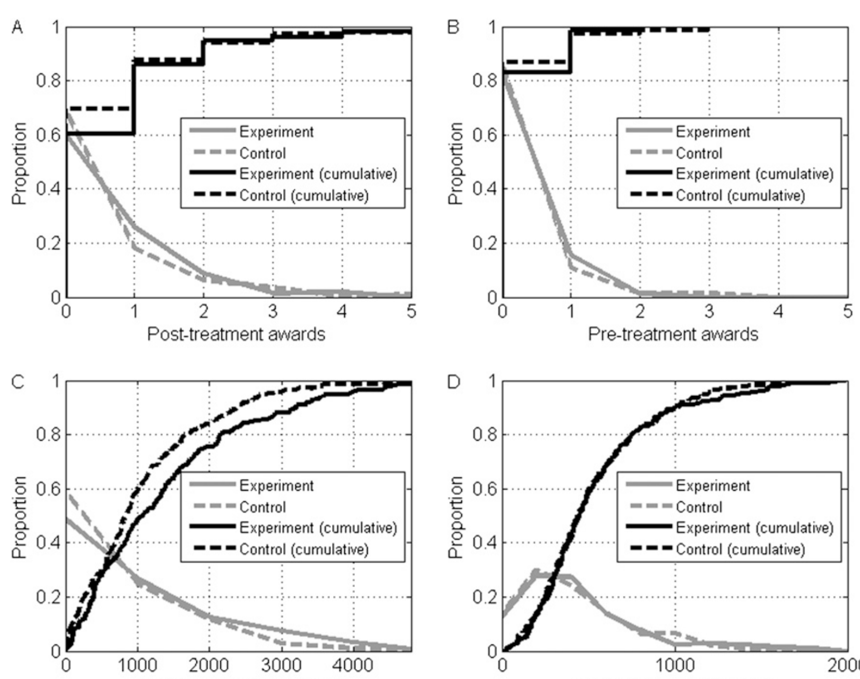

Fig. S5. Distribution of awards and productivity in the awards study. (*A*) Distribution of posttreatment awards received, by condition. (*B*) Distribution of pretreatment awards received, by condition. (*C*) Distribution of editors' posttreatment productivity, by condition. (*D*) Distribution of editors' pretreatment productivity, by condition.

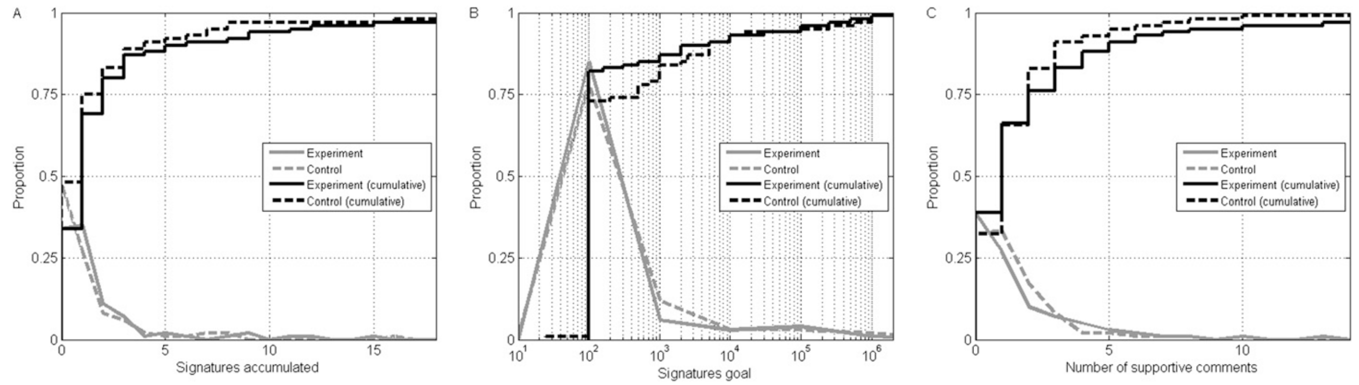

Fig. S6. Distribution of signatures and supportive comments in the signatures study. (*A*) Distribution of signatures solicited, by condition. (*B*) Distribution of signature goal amount, by condition. (*C*) Distribution of number of supportive comments on petitions, by condition.

Table S1. Crowd-funding study: Descriptive statistics of crowd-funding study, by condition

	Round 1		Round 2		
				Experiment	
Donations and dollars raised by day 24	Control 0 donations	Experiment 1 donation	Control 0 donations	1 donation	4 donations
Donations by day 24					
Mean	1.11	2.49	1.32	5.65	10.77
Median	0	1	0	2	6
SD	2.77	4.55	3.73	7.53	11.66
Dollars raised by day 24					
Mean	50.35	77.50	102.65	293.65	562.35
Median	0.00	10.00	0.00	50.00	180.00
SD	177.31	166.88	400.26	509.00	697.70
N	100	100	31	31	31

All statistics reported exclude the treatment.

Table S2. Crowd-funding study: Negative-binomial regression of posttreatment donations and dollars on treatment

	Effects on no. of donations				Effects on no. of dollars			
	Model 1		Model 2		Model 3		Model 4	
Round	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Round 1								
Treatment	0.785***	0.194	0.529**	0.203	0.859***	0.198	0.634**	0.208
Log no. of Facebook likes			0.313***	0.071			0.302***	0.074
N	200		200		200		200	
Round 2								
One donor	1.37***	0.376	0.924*	0.398	1.26**	0.372	0.776*	0.387
Four donors	1.85***	0.367	1.35***	0.386	1.74***	0.362	1.21**	0.375
Log no. of Facebook likes			0.257***	0.054			0.287***	0.056
N	93		93		93		93	

$*P < 0.05$; $**P < 0.01$; $***P < 0.001$.

Table S3. Crowd-funding study: Donations and dollars raised in round 1 by percentage donated

Additional donations or dollars by day 24	0% donated	1% donated (mean, $6.77)	10% donated (mean, $66.76)
Additional donations, %	39	68	72
No. donations by day 24			
Mean	1.11	2.36	2.62
Median	0	1	1
SD	2.77	4.87	4.25
Dollars raised by day 24			
Mean	50.35	79.97	75.02
Median	0.00	10.00	10.50
SD	177.31	193.37	137.34
N	100	50	50

Table S4. Ratings study: Descriptive statistics of ratings study, by condition

	No. of "very helpful" or "not helpful" ratings applied*		
Type of review/rating	0	1	4
High-quality reviews: "very helpful" ratings by day 14			
Mean	11.4	14.9	13.6
Median	11.5	16	15
SD	10.1	9.68	8.26
N	150	155	69
Low-quality reviews: "not helpful" ratings by day14			
Mean	0.581	2.40	3.00
Median	0	0.5	2
SD	1.56	3.24	2.72
N	43	48	16

All statistics reported exclude the treatment.
*"Very helpful" ratings were applied to high-quality reviews; "not helpful" ratings were applied to low-quality reviews.

Table S5. Ratings study: Ratings other than treatment rating, by condition

	No. of "very helpful" or "not helpful" ratings applied*		
Type of review/rating	0	1	4
High-quality reviews: ratings worse than "very helpful" by day 14			
Mean	3.72	2.27	1.68
Median	0	0	0
SD	4.95	4.16	3.41
N	150	155	69
Low-quality reviews: ratings better than "not helpful" by day 14			
Mean	10.9	8.23	6.00
Median	9	8.5	5
SD	6.29	6.05	5.06
N	43	48	16

*"Very helpful" ratings were applied to high-quality reviews; "not helpful" ratings were applied to low-quality reviews.

Table S6. Awards study: Descriptive statistics of awards study, by condition

	After 30 d		After 90 d	
	Control	Experiment	Control	Experiment
Mean	0.169	0.284	0.559	0.683
Median	0	0	0	0
SD	0.519	0.689	1.13	1.21
N	313	208	313	208

All statistics reported exclude the treatment.

Table S7. Awards study: Regression of posttreatment awards on treatment and pretreatment awards

	30 d after treatment				90 d after treatment			
	Model 1		Model 2		Model 3		Model 4	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Logistic								
Effect of treatment	0.634**	0.239	0.656**	0.243	0.406*	0.187	0.408*	0.194
Effect of pretreatment awards			0.797***	0.209			1.18***	0.227
Negative binomial								
Effect of treatment	0.544*	0.215	0.550**	0.212	0.272	0.146	0.293*	0.143
Effect of pretreatment awards			0.751***	0.140			0.743***	0.090
N	521		521		521		521	

*$P < 0.05$; **$P < 0.01$; ***$P < 0.001$.

Table S8. Awards study: Regression of posttreatment awards on treatment and productivity

	30 d after treatment				90 d after treatment			
	Model 1		Model 2		Model 3		Model 4	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Logistic								
Effect of treatment	0.634**	0.239	0.583*	0.241	0.406*	0.187	0.343	0.190
Effect of productivity			0.641*	0.262			0.205*	0.086
Negative binomial								
Effect of treatment	0.544*	0.215	0.489*	0.216	0.272	0.146	0.205	0.147
Effect of productivity			0.624***	0.214			0.178**	0.061
N	521		521		521		521	

*$P < 0.05$; **$P < 0.01$; ***$P < 0.001$.

Table S9. Signatures study: Descriptive statistics of signatures study, by condition

	After 14 d	
	Control	Experiment
Mean	1.74	2.32
Median	1	1
SD	3.96	4.59
N	100	100

Note: All statistics reported exclude the treatment.

Table S10. Signatures study: Negative binomial regression of posttreatment signatures

	Model 1		Model 2		Model 3		Model 4	
Predictor	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Treatment	0.288	0.231	0.525*	0.241	0.600**	0.220	0.478*	0.212
Pretreatment signatures			0.164***	0.037	0.114***	0.037	0.091**	0.091
Signature goal					1.043***	0.249	0.659**	0.252
Supportive comments							0.382**	0.117
N	200		200		200		199	

$^*P < 0.05$; $^{**}P < 0.01$; $^{***}P < 0.001$.