Don't get it or don't spread it? Comparing self-interested versus prosocially framed COVID-19 prevention messaging

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Abstract
The COVID-19 pandemic threatens millions of lives, and an effective response will require individuals to take costly and difficult measures to slow the rate of transmission. Yet it is unclear how to best motivate preventative actions, which can be conceptualized as either self-interested or cooperative efforts. Should public health messaging focus on the benefits of prevention to individuals, society, or both? We shed light on this question across two pre-registered studies conducted online via Amazon Mechanical Turk (total n = 2176 Americans) during the early days of the COVID-19 pandemic reaching the United States. We investigated the effects of three treatments, consisting of a written appeal and a flier, on intentions to engage in coronavirus prevention behaviors. We presented identical information across treatments, but varied our framing to emphasize the *personal*, *public*, or both *personal and public* benefits of prevention behaviors. While all three treatments increased prevention intentions relative to a no-information control, we found important differences across treatments. In particular, we found strong evidence for the power of prosocial framing: the Public treatment was more effective than the Personal treatment, and the Personal+Public treatment was no more effective than the pure Public treatment. Our results thus suggest that emphasizing the public benefits of prevention efforts may be an effective pandemic response strategy.

Introduction

I have a message for young people: You are not invincible; this virus could put you in hospital for weeks or even kill you. Even if you don't get sick, the choices you make about where you go could be the difference between life and death for someone else.

–Tedros Adhanom Ghebreyesus, March 2020
Director, World Health Organization

The COVID-19 pandemic poses an enormous threat to humans across the globe. Relative to the seasonal flu, coronavirus is more contagious, has a longer incubation period (allowing for more asymptomatic transmission), and results in much higher rates of hospitalization and death[1,2]. For these reasons, exponential growth of the virus[3,4] threatens to overwhelm health care systems and kill millions of people[5,6]. To combat the pandemic, it is thus essential that individuals engage in prevention behaviors that can slow the rate of transmission[7–9]. Many of these behaviors, however, are difficult to adhere to (e.g., vigilant handwashing) or require people to make substantial personal sacrifices (e.g., staying home from work), posing a substantial challenge that will require us to draw on insights from public health and the behavioral sciences[10–15]. So how can we best motivate people to act to prevent coronavirus?

The leading quote from WHO director Tedros Adhanom Ghebreyesus highlights one reason that this question is interesting: Coronavirus prevention efforts serve not only to protect the people who engage in them, but also to promote the welfare of society as a whole. And for many people, it may not be obvious which of these benefits is more meaningful. For example, as Ghebreyesus stresses, even young and healthy individuals can be severely impacted by the virus. Yet the rate of death for this demographic is nonetheless quite low in absolute terms. Thus, coronavirus prevention behaviors can reasonably be conceptualized as either self-interested actions (that are undertaken because of their direct benefits to the actor) or as cooperative efforts (that are worthwhile only when considering their benefits to society). Here we ask which of these framings is more effective for motivating infection prevention behaviors.

According to classical economic theories of decision-making, people care only about their own welfare. This perspective would suggest that self-interested appeals should be the most effective motivators. As Adam Smith once famously said, "It is not from the benevolence of the butcher, the brewer, or the baker that we expect our dinner, but from their regard to their own interest"[16]. Yet while the classical economic perspective is still highly influential in guiding policy, behavioral scientists are increasingly aware of the importance of more psychologically informed theories[10–12,17–21]. And research in psychology and behavioral economics provides clear evidence that while self-interest of course looms large in human decision-making, people are also moral actors[22–25] who care about the welfare of others[26], are motivated to cooperate[26–31], and strive to avoid appearing selfish in the eyes of others[32–37]. On this basis, we might expect it to be effective to emphasize the threat that coronavirus poses to others, and to encourage people to take preventative action to avoid spreading the disease.

Furthermore, to the extent that both self-interested and prosocial motives can drive preventative efforts, appealing to both together—as Ghebreyesus does in the leading quote—need not be more effective than appealing to one or the other separately. Of course, it is possible that providing two motivations is better than one. But self-interested framing might also serve to "crowd out" prosocial motives for prevention (e.g., by shifting people to a self-focused mindset,

distracting people from the collective threat, or reducing the perceived reputation value of prevention efforts)[38–41]. And reciprocally, prosocial framing might serve to "crowd out" self-interested motives for prevention (e.g., by implying that the threat to individuals is low, and prosocial motives are necessary to make prevention efforts worthwhile).

Previous research investigating self-interested versus prosocial motives to prevent disease spread has focused primarily on vaccination decisions. This body of work provides strong evidence that people have both self-interested *and* altruistic motives for vaccination, and that increasing both types of motivations can increase intentions to vaccinate[42–45]. Furthermore, a few studies have directly compared the effectiveness of personal versus public framing in vaccination appeals[46–49], with inconsistent results. Some of these studies have failed to show that public-focused frames are effective[46,47], while others provide some mixed evidence for the value of such frames[48,49], and a set of field studies investigating handwashing among healthcare professionals found that it was more effective to emphasize patient safety than personal safety[50].

Thus, it is unclear whether COVID-19 prevention messaging is most effectively framed around benefits to the individual (e.g., "don't get it"), to others (e.g., "don't spread it"), or both (e.g., "don't get and spread it"). Here, across two pre-registered studies (total $n = 2176$), we aim to shed light on this question. We do so by investigating the effects of three messaging treatments on intentions to engage in COVID-19 prevention behaviors: one that emphasizes the *personal* benefits of prevention, one that emphasizes the *public* benefits of prevention, and one that emphasizes *both* personal and public benefits.

We find strong evidence that the Public message is more effective than the Personal message. We also find that the Personal+Public message is no more effective than the pure Public message, and thus that adding self-interested framing to prosocial framing produces no additional benefit. These findings suggest that framing prevention efforts as a public good may be an effective strategy for motivating people to help combat the COVID-19 pandemic.

Method

Overview

We conducted two studies online via Amazon Mechanical Turk. These studies were conducted during the early days of the COVID-19 pandemic reaching the United States, on March 14, 2020 (Study 1) and March 16 (Study 2).

Both Study 1 (https://aspredicted.org/blind.php?x=mb9t3e) and Study 2 (https://aspredicted.org/blind.php?x=w8jk9m) were pre-registered, and our analyses adhere closely to our pre-registered analysis plans. We note the substantive exceptions in our main text where relevant, and list all exceptions in SI Section 2. We also note that our full materials, raw data, and a script reproducing all analyses are available online at https://osf.io/sr4n9/.

Our two studies employed very similar designs, but differed in a few ways. We begin by describing the Study 1 method, and then describe the ways that Study 2 differed from Study 1.

In Study 1, we recruited a target of $n = 1000$ subjects, and assigned them to one of four experimental conditions, which consisted of a control condition (involving no treatment) and three treatment conditions (Personal, Public, and Personal+Public). After obtaining consent from subjects, we began by exposing subjects in our treatment conditions to the relevant treatment.

Treatments

In all treatments, we first assigned subjects to read some written text about COVID-19, and then presented subjects with a flier about COVID-19.

Written text

We began by providing subjects with some basic information about the virus and the threat it poses. This portion of the written text was identical across treatments, and read:

> Coronavirus disease 2019 (COVID-19) is a respiratory illness that can spread from person to person. The virus that causes COVID-19 is a novel coronavirus that was first identified during an investigation into an outbreak in Wuhan, China. Because COVID-19 is a novel virus, there is no immunity in the community yet. There is also no vaccine for COVID-19.
>
> COVID-19 is currently spreading rapidly through the US. As of today, there are at least 1,701 confirmed cases, and this number is likely a major underestimate given that testing in the US has been extremely limited. The number of cases is growing exponentially. According to one projection by the Center for Disease Control (CDC), between 160 million and 214 million people in the U.S. could be infected over the course of the epidemic. As many as 200,000 to 1.7 million people could die. And, the calculations based on the CDC's scenarios suggested, 2.4 million to 21 million people in the U.S. could require hospitalization, potentially crushing the nation's medical system, which has only about 925,000 staffed hospital beds. Fewer than a tenth of those are for people who are critically ill.
>
> COVID-19 is much worse than the ordinary flu. The flu has a death rate of around 0.1% of infections. Globally, about 3.4 percent of reported COVID-19 cases have died. Furthermore, experts think COVID-19 is more contagious than the ordinary flu. And people can spread COVID-19 before experiencing any symptoms.

Next, we encouraged subjects to take the virus seriously and take preventative action. This portion of the written text varied across treatments. In the *Personal* treatment, it read:

> For all of these reasons, **coronavirus is a serious threat to you**. It is recommended that you take this threat very seriously to prevent contracting COVID-19 and getting very ill or dying. Fortunately, **there are steps you can take to keep yourself safe.**

In the *Public* treatment, it read:

> This means **coronavirus is a serious threat to your community**. It is recommended that you take this threat very seriously to prevent spreading COVID-19 and causing people in your community to get very ill or die. Fortunately, **there are steps you can take to keep your community safe.**

And in the *Personal+Public* treatment, it read:

> This means **coronavirus is a serious threat to you and your community**. It is recommended that you take this threat very seriously to prevent contracting COVID-19 and getting very ill or dying, or spreading COVID-19 and causing people in your

> community to get very ill or die. Fortunately, **there are steps you can take to keep yourself and your community safe.**

Thus, in the Personal treatment, we emphasized the threat to the subject, in the Public treatment, we emphasized the threat to the subject's community, and in the Personal+Public treatment, we emphasized the threat to the subject and their community.[1]

Finally, the written text concluded by encouraging subjects to engage in prevention behaviors. This portion of the text was again constant across conditions, and read:

> It is recommended that you practice good personal hygiene (wash your hands, avoid shaking hands or hugging others, avoid touching your face, and cover your mouth when you cough or sneeze), stay home if you are even a little bit sick, practice social distancing (by staying home as much as possible and avoiding close contact with others), and prepare by purchasing food reserves, medication, and cleaning supplies.

Fliers

After subjects finished reading this text, they were asked to carefully read a flier about COVID-19 (see Figure 1). This flier varied across treatments, again by emphasizing threat to the subject, their community, or both.

Figure 1. *Fliers shown in each treatment.*

Measures

After the treatment screens, subjects in treatment conditions advanced to the outcome measures. Subjects in the control condition immediately advanced to the outcome measures following the consent form.

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[1] We note that the difference between the clause "for all these reasons" (Personal treatment) and "this means" (Public treatment and Public + Personal treatment) reflects an unintentional error; however, we think that it is very unlikely to account for our results.

We began by measuring (i) two dependent variables and (ii) two potential mediators. We randomized between-subjects whether we measured our dependent variables before or after our potential mediators.

Dependent variables

As dependent variables, we collected two sets of variables measuring subjects' intentions to behave in ways that can help prevent the spread of coronavirus. We measured these variables in a fixed order.

First, subjects reported their *prevention intentions.* To do so, they reported their intentions, on 0-100 sliding scales, to engage in a series of 11 prevention behaviors ("wash my hands at least 10 times a day", "wash my hands more often", "stop shaking other people's hands", "stop hugging people", "try my hardest to avoid touching my face", "stay home if I am feeling even a little bit sick", "try to stay home whenever possible, even if I am not sick", "cover my mouth when I cough and sneeze", "purchase food reserves and medication", and "stock up on cleaning supplies"). To create a composite measure of prevention intentions, we averaged intentions to engage in these 11 behaviors.

Next, subjects reported their *social distancing intentions.* To do so, they reported their intentions, on 0-100 sliding scales, to engage in social distancing by avoiding a set of 10 activities ("going to bars", "going to restaurants", "going to coffee shops", "going to the grocery store", "going to the gym", "going to work (somewhere outside of your home)", "using public transportation", "going to the airport and flying", "socializing in small gatherings", and "attending large events or gatherings"). Then, on a subsequent page, we asked subjects which of these activities they would engage in at least sometimes if coronavirus were not a concern. To create a composite measure of social distancing intentions, for each subject, we averaged intentions to avoid all activities that the subject indicated they would otherwise engage in.

Potential mediators

We also measured two potential mediating variables, both of which pertained to the perceived threat of the virus. Specifically, we measured both the perceived *personal* threat (to the subject) and *public* threat (to society) of coronavirus. We reasoned that if our treatments function by influencing the perceived (personal or public) threat of the virus, these variables might mediate our effects. Alternatively, our treatments could operate through other causal pathways (e.g., increasing the perceived efficacy, importance, or reputation value of engaging in prevention behaviors) not captured by these potential mediators.

We measured perceived personal and public threat on separate pages in random order, via two questions per construct (which we averaged to form composite variables). See SI for exact wording of these questions, and a description of a programming error that caused a minor difference in the way that we measured personal versus public threat.

Individual difference variables

Finally, we collected a series of individual difference variables. First, we asked subjects to report, in a fixed order, their age, gender, level of education, zip code, subjective health, number of pre-existing health conditions (from a list of conditions we specified), income bracket, political ideology (as measured by three questions asking about political party identification, position on social issues, and position on fiscal issues), and previous exposure to information about COVID-19. Next, we presented subjects with a three-item cognitive reflection task[51].

Finally, we asked subjects to answer a simple analogy question and write a few sentences about their plans for the day; these measures were designed to screen for subjects who did not speak English (see SI Section 1.5 for analyses). For exact wording for all questions, see SI Section 3.

Modifications for Study 2

Study 2 was very similar to Study 1, with a few differences. First, in Study 2, we recruited a target of $n = 1200$ subjects and assigned them to one of our three treatment conditions. Study 2 thus omitted the control condition that was included in Study 1, and had a larger target sample size per condition (of $n = 400$ rather than $n = 250$).

Second, because Study 2 was conducted two days after Study 1, we modified the information about the number of confirmed cases of coronavirus in the United States. Specifically, we replaced the clause "As of today, there are at least 1,701 confirmed cases" with the clause "As of Sunday night, there are now over 3,000 confirmed cases".

Third, in Study 1 we moved the measurement of all of our individual difference variables, with the exception of performance on the cognitive reflection task, to the beginning of the study (i.e., before presenting our treatments); the cognitive reflection task was still presented last (i.e., after we measured our dependent variables and potential mediating variables).

Fourth, we made a few modifications to the wording we used to measure our potential mediating variables; see SI for details.

Finally, as our dependent variable, we only measured prevention intentions (and did not measure social distancing intentions). We made this decision because in Study 1, our measure of prevention intentions (which also included an item about social distancing) produced stronger evidence for treatment effects and interesting differences between treatments. In Study 2, we thus chose to focus on replicating the observed effects on our measure of prevention intentions. We note also that for this reason, we focus primarily on prevention intentions (and less on social distancing intentions) in our analyses.

Analysis approach

To form our final samples, when we collected duplicate responses from the same IP address or Mturk worker ID, we included only the chronologically first response. We also excluded responses from subjects who did not complete all of our key measures (defined as our dependent measures and potential mediators). This left us with $n = 988$ subjects in Study 1 and $n = 1188$ subjects in Study 2.

All of our analyses use linear regression. In all regression models aggregating data from both Studies 1 and 2, we include a study dummy. For analyses of our dependent variables, we report results (i) among all subjects, and (ii) among subjects for whom we measured our dependent variables *before* measuring our potential mediators.[2] Although the latter analysis was not pre-registered, we include it to confirm the robustness of our results after having discovered an unexpected interaction between condition and the order in which we measured our dependent variables versus potential mediators.[3]

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[2] Likewise, for analyses of our potential mediators, we report results (i) among all subjects, and (ii) among subjects for whom we measured our potential mediators *before* measuring our dependent variables.

[3] In a regression predicting prevention intentions across both studies as a function of dummies for each condition, order, and the interactions between order and each condition dummy, the interaction terms are jointly significant, $F(3,2167) = 4.97$, $p = .002$, revealing that order significantly influenced the effects of our conditions on prevention intentions.

Finally, we note that in Study 1, we found some evidence that individuals reporting greater subjective health showed relatively larger effects of the Public treatment. Thus, in our Study 2 pre-registration, we planned for our primary analyses to focus specifically on healthier individuals. However, evidence for an interaction between health and our Public treatment effects was weaker in Study 2 than in Study 1 (see SI Section 1.4 for details). Thus, we do not feel confident focusing on health in our primary analyses, and instead report analyses of all subjects. We note, however, that as shown in the SI, analyses of healthy individuals support (and actually provide even stronger evidence for) our key finding that the Public treatment was more effective than the Personal treatment, and no less effective than the Personal+Public treatment.

Results

We begin by investigating composite prevention intentions (across our 11-item scale, α = .89 in each study). First, we compare each of our treatment conditions to the control in Study 1 (Figure 2). We conduct regressions predicting prevention intentions, taking the control condition as the baseline and including dummies for the other three conditions. The results, shown in in Table 1, suggest that all three treatments were effective at increasing prevention intentions relative to the control.

	All subjects (*n* = 988)	**Dependant variables first (*n* = 506)**
Personal	Control = 76.41, Personal = 79.19, **b = 2.78, t = 1.89, p = .059**	Control = 74.49, Personal = 78.08, **b = 3.59, t = 1.72, p = .086**
Public	Control = 76.41, Public = 81.88, **b = 5.47, t = 3.70, p < .001**	Control = 74.49, Public = 82.39, **b = 7.90, t = 3.74, p < .001**
Personal + Public	Control = 76.41, Personal+Public = 79.76, **b = 3.35, t = 2.26, p = .024**	Control = 74.49, Personal+Public = 82.22, **b = 7.73, t = 3.64, p < .001**

Table 1. Treatment effects on prevention intentions in Study 1. *We compare each of our three treatments to the control condition in Study 1. For each treatment, we report mean prevention intentions in the treatment and control conditions, and the treatment effect. We report results both among all subjects, and subjects for whom we measured our dependent variables first.*

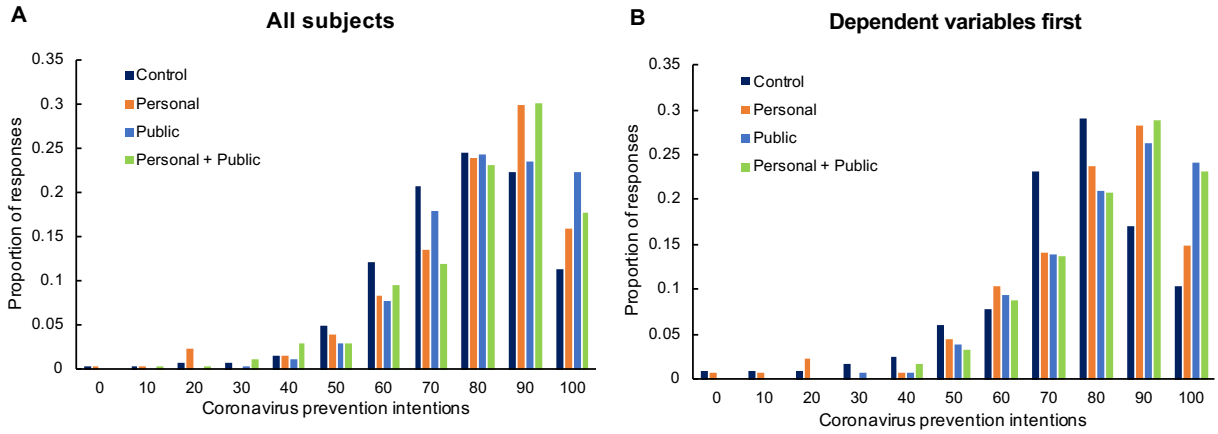

Figure 2. Prevention intentions by experimental condition in Study 1. *Subjects report greater intentions to engage in coronavirus prevention behaviors in the treatment conditions than the*

control condition. Shown are frequencies of composite prevention intentions, rounded to zero or a multiple of ten, by experimental condition in Study 1, among all subjects (Panel A, n = 988) and subjects for whom we measured our dependent variables first (Panel B, n = 506).

Next, we turn to comparing the relative effectiveness of our different treatments. We find that the Public treatment had the directionally largest effect, and thus organize these analyses around comparing the Public treatment to the other two treatments. Throughout this paper, we conduct this comparison via regressions that take the Public treatment condition as the baseline and measure relative effectiveness of Public using dummies for the other two treatment conditions. The results, shown in Table 2 and Figure 3, indicate that the Public treatment was more effective than the Personal treatment, and that there was no significant difference between the effectiveness of the Public treatment and Personal+Public treatment. Together, these results suggest that it is advantageous to frame coronavirus prevention efforts as a public good.

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	All subjects	
	Public vs. Personal	**Public vs. Personal+Public**
Study 1 ***n*** **= 742**	Public = 81.88, Personal = 79.19, **b = 2.69, t = 1.84, p = .066**	Public = 81.88, Personal+Public = 79.76, **b = 2.12, t = 1.44, p = .151**
Study 2 ***n*** **= 1188**	Public = 82.85, Personal = 80.39, **b = 2.46, t = 2.25, p = .025**	Public = 82.85, Personal+Public = 81.88, **b = 0.98, t = 0.89, p = .375**
Studies 1 and 2 ***n*** **= 1930**	Public = 82.48, Personal = 79.93, **b = 2.55, t = 2.90, p = .004**	Public = 82.48, Personal+Public = 81.07, **b = 1.41, t = 1.60, p = .109**
	Dependant Variables first	
	Public vs. Personal	**Public vs. Personal+Public**
Study 1 ***n*** **= 389**	Public = 82.39, Personal = 78.08, **b = 4.32, t = 2.19, p = .029**	Public = 82.39, Personal+Public = 82.22, **b = 0.17, t = 0.08, p = .933**
Study 2 ***n*** **= 592**	Public = 83.78, Personal = 80.34, **b = 3.45, t = 2.39, p = .017**	Public = 83.78, Personal+Public = 83.70, **b = 0.09, t = 0.06, p = .950**
Studies 1 and 2 ***n*** **= 981**	Public = 83.22, Personal = 79.42, **b = 3.80, t = 3.25, p = .001**	Public = 83.22, Personal+Public = 83.14, **b = 0.13, t = 0.11, p = .913**

Table 2. Differences in effectiveness across treatments. *We compare the Public treatment to each of our other two treatment conditions, across the treatment conditions of (i) Study 1, (ii) Study 2, and (iii) Studies 1 and 2 combined. For each comparison, we report mean prevention intentions by condition and the relative effect of the Public treatment. We report results both among all subjects, and subjects for whom we measured our dependent variables first.*

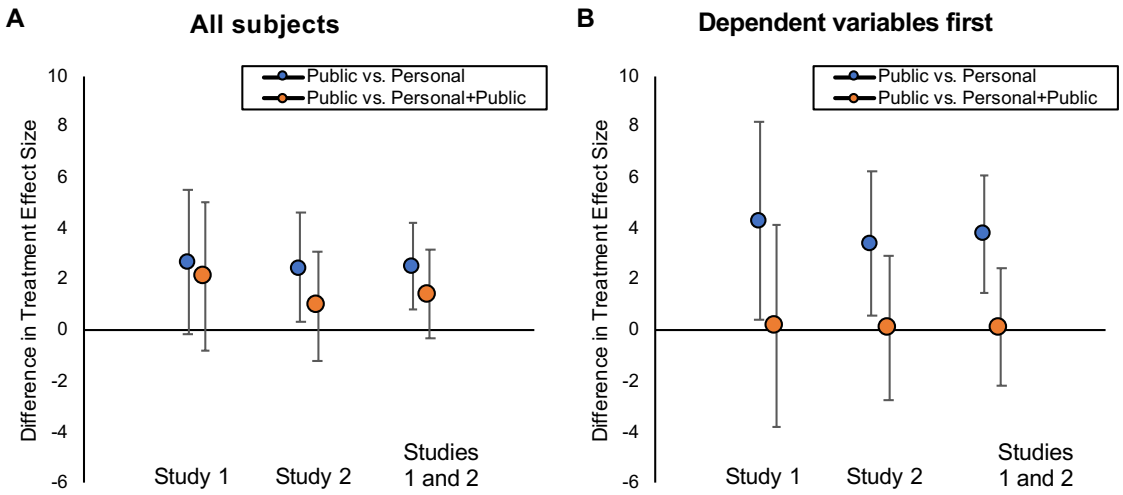

Figure 3. Differences in effectiveness across treatments. *The Public treatment was more effective than the Personal treatment, and no less effective than the Personal+Public treatment, at increasing reported prevention intentions. Shown are the relative effects of the Public treatment, as compared to the Personal treatment (blue dots) and Personal+Public treatment (orange dots). We plot results among all subjects (Panel A: Study 1 n = 742, Study 2 n = 1188, Studies 1 and 2 combined n = 1930) and subjects for whom we measured our dependent variables first (Panel B: Study 1 n = 389, Study 2 n = 592, Studies 1 and 2 combined n = 981).*

Importantly, we find no clear evidence of heterogeneity in the effects of our treatments across the 11 prevention behaviors we investigated (see SI Section 1.1), or across individuals based on the individual difference variables we collected (see SI Section 1.2). We also find no compelling evidence for treatment effects on our potential mediators (i.e., perceived personal and public threat of coronavirus; see SI Section 1.3), suggesting that the Public treatment was especially effective not because it was especially good at increasing the perceived threat of the virus, but instead because it had other advantageous effect(s) (e.g., activating prosocial preferences or increasing the perceived moral or reputational value of prevention efforts).

Next, we report a set of exploratory analyses investigating the associations between our individual difference variables and composite prevention intentions (Table 3). This exploratory analysis provides evidence that prevention intentions are associated with older age, female gender, better health, higher income, liberal political ideology, greater previous exposure to COVID information, and greater population density. We also find some evidence for a negative association between performance on the cognitive reflection test and prevention intentions (when controlling for the other individual difference variables in our multiple regression models).

	All subjects		Dependant variables first	
	Separate models	Multiple regression	Separate models	Multiple regression
Age	**0.102*** **	**0.0888*** **	**0.0957** **	**0.0855** **
	(0.0214)	(0.0216)	(0.0300)	(0.0300)
Male	**-0.183*** **	**-0.153*** **	**-0.202*** **	**-0.167*** **
	(0.0210)	(0.0212)	(0.0295)	(0.0294)
College degree	**0.0333**	**-0.0161**	**0.0305**	**-0.0421**
	(0.0214)	(0.0228)	(0.0300)	(0.0321)
Subjective health	**0.119*** **	**0.0998*** **	**0.137*** **	**0.126*** **
	(0.0212)	(0.0220)	(0.0298)	(0.0310)
Pre-existing health conditions	**-0.0515***	**-0.0224**	**-0.0632***	**-0.0369**
	(0.0214)	(0.0232)	(0.0300)	(0.0324)
Income	**0.0887*** **	**0.0427**	**0.115*** **	**0.0666***
	(0.0213)	(0.0229)	(0.0299)	(0.0326)
Conservative (vs. liberal) political ideology: Party identification	**-0.0787*** **	**-1.45e-06**	**-0.113*** **	**-0.0288**
	(0.0213)	(0.0359)	(0.0299)	(0.0501)
Conservative (vs. liberal) political ideology: Social issues	**-0.0673** **	**-0.0208**	**-0.0999*** **	**-0.0518**
	(0.0213)	(0.0392)	(0.0299)	(0.0541)
Conservative (vs. liberal) political ideology: Fiscal issues	**-0.0788*** **	**-0.0775***	**-0.105*** **	**-0.0487**
	(0.0213)	(0.0387)	(0.0299)	(0.0525)
Previous exposure to COVID info	**0.282*** **	**0.254*** **	**0.304*** **	**0.260*** **
	(0.0206)	(0.0211)	(0.0287)	(0.0293)
CRT score	**-0.0346**	**-0.0772*** **	**-0.0348**	**-0.0752***
	(0.0214)	(0.0226)	(0.0301)	(0.0312)
Log-transformed population density (from zip)	**0.0680** **	**0.0754*** **	**0.0724***	**0.0837** **
	(0.0220)	(0.0212)	(0.0310)	(0.0297)

Standard errors in parentheses

*** p<0.001, ** p<0.01, * p<0.05

Table 3. Individual difference variables as predictors of prevention intentions. *Here we explore the associations between our individual difference variables and prevention intentions. We report results from regressions predicting prevention intentions as a function of our individual difference variables, among all subjects (Columns 1 and 2) and among subjects for whom we measured our dependent variables first (Columns 3 and 4). For each group, we report results from (i) a series of separate models for each individual difference variable (Columns 1 and 3) and (ii) one multiple regression model using all individual difference variables (Columns 2 and 4), across all conditions of Studies 1 and 2. All coefficients are standardized coefficients, and standard errors are reported below each coefficient in parentheses. Before conducting these analyses, we (i) computed a "college degree" dummy from our measure of education, (ii) computed CRT scores (as the number of questions correct out of a possible three), and (iii)*

natural log-transformed our measure of population density. For our separate models analyzing all subjects, n = 2176 for all variables except population density, for which n = 2083, and for our separate models analyzing subjects for whom we measured our dependent variables first, n = 1098 for all variables except population density, for which n = 1047. For our multiple regressions analyzing all subjects, n = 2083, and for our multiple regressions analyzing subjects for whom we measured our dependent variables first, n = 1047.

Finally, we turn to investigating composite social distancing intentions (across our 10-item scale collected in Study 1, α = .91). As shown in Table 4, we find some limited evidence for positive treatment effects, relative to control. Yet we find no significant differences between our treatments (all ps > .1 for pairwise comparisons of regression coefficients). Importantly, however, we note that the advantage of the Public treatment relative to the Personal treatment *did* extend to the overall social distancing item included in our composite measure of prevention intentions ("try to stay home whenever possible, even if I am not sick"). In an aggregate analysis of Studies 1 and 2, we find a significant positive effect of a Public vs. Personal dummy on this item, both among all subjects, $b = 3.70$, $t = 2.83$, $p = .005$, and subjects for whom we measured our dependent variables first, $b = 6.11$, $t = 3.35$, $p = .001$. Thus, we believe there is reason to expect that the Public treatment may be relatively more effective than the Person treatment at encouraging social distancing.

	All subjects (*n* = 985)	**Dependant variables first (*n* = 505)**
Personal	Control = 66.50, Personal = 69.56, **b = 3.06, t = 1.58, p = .114**	Control = 62.91, Personal = 68.10, **b = 5.19, t = 1.92, p = .056**
Public	Control = 66.50, Public = 69.47, **b = 2.97, t = 1.53, p = .127**	Control = 62.91, Public = 68.59, **b = 5.68, t = 2.07, p = .039**
Personal + Public	Control = 66.50, Personal+Public = 69.60, **b = 3.10, t = 1.58, p = .114**	Control = 62.91, Personal+Public = 71.83, **b = 8.92, t = 3.23, p = .001**

Table 4. Treatment effects on social distancing intentions in Study 1. *We compare each of our three treatments to the control condition in Study 1. For each treatment, we report mean prevention intentions in the treatment and control conditions, and the treatment effect. We report results both among all subjects, and subjects for whom we measured our dependent variables first.*

Discussion

The COVID-19 pandemic poses an enormous global threat, and to effectively combat this threat it is essential that individuals engage in costly and difficult prevention-focused behaviors. Here we have provided evidence that messaging that frames prevention efforts as public goods—emphasizing the benefits of such behaviors for *other* people's health—is more effective for increasing prevention intentions than messaging focused on the benefits for the individual. We also find that emphasizing both the public *and* personal benefits of prevention efforts is no more effective than emphasizing only the public benefits. Our results thus suggest that when it comes to encouraging people to adopt COVID-19 prevention behaviors, "don't spread it" is a more effective message than "don't get it".

These results have both theoretical and practical implications. They suggest that people are particularly receptive to conceptualizations of coronavirus prevention efforts as a public good, implying that prosocial motives—or the desire to *appear* prosocial in the eyes of others—

may be a major driver of intentions to prevent infection. This finding is striking, considering the substantial risks of COVID-19 infection to individuals: as discussed in the introduction, death and hospitalization rates for coronavirus far exceed those for the seasonal flu.

Our findings thus serve to bolster the body of existing evidence, which has primarily focused on vaccination, that prosocial motives can drive intentions to prevent the spread of infectious diseases[42–45]. They also contribute to the set of studies that have directly compared the self-interested and prosocial frames[46–50] by adding strong evidence in favor of prosocial frames to a mixed literature. And more broadly, our results support the growing body of work suggesting that it is important for policymakers to consider that, in addition to being self-interested, people are moral actors who care for others and care to avoid appearing selfish[22–25,27–30,32–37,52].

Additionally, our finding that the combined Personal+Public treatment was no more effective than the pure Public treatment is notable. Assuming that people have both self-interested and prosocial motives, it would be natural to expect emphasizing both the personal *and* public benefits of prevention efforts to be the most effective strategy. Indeed, this was the strategy chosen by WHO director Tedros Adhanom Ghebreyesus in the opening quote highlighted in our introduction.

Yet in our studies, combined appeals were *not* more effective than appeals focused exclusively around the benefits of prevention efforts to others. One explanation for this finding is that, once the public benefits of prevention efforts were highlighted, additionally highlighting the personal benefits did nothing to compel people to act. Alternatively, it is possible that adding self-interested framing *did* compel people to act, but this effect was offset by a countervailing effect whereby self-interested framing served to "crowd out" prosocial motives[38–41] (e.g., by shifting people to a self-focused mindset, distracting people from the collective threat, or reducing the perceived reputation value of prevention efforts). Future research should investigate this possibility, and more generally the effectiveness of combined appeals, in more detail.

Finally, we discuss a few important limitations of our work. First, as is common throughout the literature investigating disease prevention behaviors[53], our studies measured self-reported intentions to engage in prevention behaviors (rather than actual prevention behaviors, which are extremely difficult to observe and measure). As such, it is possible that prosocially-framed messaging was simply effective at causing subjects to *report* greater prevention intentions, but not at truly changing intentions—perhaps because prosocially framed messaging creates greater social pressure to report prevention intentions. And it is also possible that any true effect on *intentions* would fail to translate into an effect on actual *behavior*.

Even if social pressure to report prevention intentions did contribute to our results, however, we believe that the findings may still be relevant. Insofar as prosocially-framed messaging creates heightened social pressure to report prevention intentions, it seems likely that prosocially-framed messaging might also create social pressure to actually engage in prevention behaviors in the real world. And a great deal of research shows the power of social pressure for promoting cooperative behavior outside the laboratory[15]. Nonetheless, it is critical that future work test the impact of self-interested versus prosocially framed messaging on actual prevention behavior using field experiments.

Another important limitation is that our studies used Amazon Mechanical Turk to recruit convenience samples of Americans. The fact that we found no compelling evidence for moderation of our treatment effects by our individual difference variables may provide some reason to expect that our results might generalize to other Americans; and the fact that our results

highlight the power of prosocially framed messaging, despite the United States being a fairly individualist (rather than collectivist) culture[54], may provide some reason to expect that our results might generalize to other countries. Nonetheless, it is important for future work to assess how our results replicate using more representative samples and subjects from other countries and cultures.

Finally, while our results provide evidence for the potential power of prosocially framed messaging, we only tested one set of treatment messages. When designing our treatments, we sought to provide information about COVID-19 that emphasized the substantial threats posed by coronavirus to both individuals and society. However, it is possible that different self-interested appeals might be more effective—for example, if they presented a compelling argument that young and/or healthy individuals are at more serious personal risk than most people believe. It is thus important that future research investigates the generalizability of our results across a range of different messaging content.

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Supplementary information
for
Don't get it or don't spread it? Comparing self-interested versus prosocially framed COVID-19 prevention messaging

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1. Supplementary analyses

1.1 Investigating heterogeneity of treatment effects across prevention behaviors

As discussed in the main text, our primary analyses of prevention intentions investigated composite prevention intentions, computed by averaging intentions to engage in our set of eleven individual prevention behaviors. Here, we investigate whether there is meaningful heterogeneity of treatment effects across individual prevention behaviors. In Figure S1A, we plot overall treatment effects (i.e., effects of a "treatment vs. control" dummy) on each individual prevention behavior in Study 1. In Figure S1B, we plot effects of the Public treatment, relative to the other two treatments, on each individual prevention behavior across Studies 1 and 2. We show results both among all subjects, and subjects for whom we measured our dependent variables first.

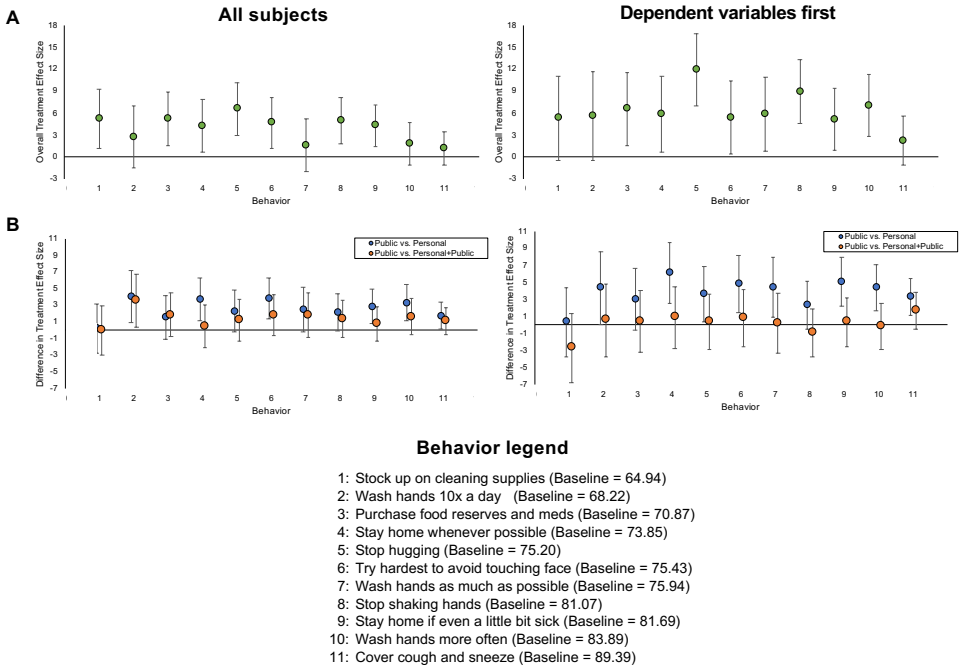


Figure S1. Treatment effects on individual prevention behaviors. *(A) Overall treatment effects on individual behaviors. Shown are the aggregated effects of our three treatments, as compared to the control condition (green dots), in Study 1 (among all subjects, n = 988, and subjects for whom we measured our dependent variables first, n = 506). (B) Effects of the Public treatment on individual behaviors. Shown are the relative effects of the Public treatment, as compared to the Personal treatment (blue dots) and Personal+Public treatment (orange dots), across the treatment conditions of Studies 1 and 2 combined (among all subjects, n = 1930, and subjects for whom we measured our dependent variables first, n = 981). Behaviors are organized in ascending order by "baseline" intentions to engage in the behavior (defined by mean intentions in the control condition of Study 1, among all subjects), and this value is also reported in the behavior legend. Error bars are 95% CIs.*

Figure S1 reveals that our overall treatment effect, and the advantage of our Public treatment relative to other treatments, are relatively robust across individual prevention behaviors. Confirming this visual impression, we find no significant heterogeneity across individual prevention behaviors. To test for heterogeneity for each condition contrast in Figure

S1, we reshaped our data to have one observation per subject-behavior. We then performed a joint significance test on the interaction terms between a dummy for the relevant condition contrast, and dummies for each of the behaviors (with robust standard errors clustered on subject).

In analyses of all subjects, we found no significant heterogeneity across behaviors for (i) the contrast between treatments and control in Study 1, $F(10,987) = 1.38$, $p = .183$, or (ii) the contrasts between Public and Personal, $F(10,1929) = 1.51$, $p = .131$, or Public and Personal+Public, $F(10,1929) = 0.91$, $p = .519$, across Studies 1 and 2. Likewise, in analyses of all subjects for whom we measured our dependent variables first, we found no significant heterogeneity across behaviors for (i) the contrast between treatments and control in Study 1, $F(10,505) = 1.65$, $p = .090$, or (ii) the contrasts between Public and Personal, $F(10,980) = 1.54$, $p = .121$, or Public and Personal+Public, $F(10,980) = 1.31$, $p = .218$, across Studies 1 and 2.

Thus, we find no evidence of heterogeneity in treatment effects across individual behaviors.

1.2 Investigating heterogeneity of treatment effects across individuals

Next, we investigate potential heterogeneity of treatment effects across individuals. Specifically, in Table S1, we report a set of exploratory analyses investigating whether each of our individual difference variables moderate our treatment effects.

	All subjects			**Dependant variables first**		
	All treatments vs. Control in Study 1	Public vs. Personal in Studies 1-2	Public vs. Pesonal + Public in Studies 1-2	All treatments vs. Control in Study 1	Public vs. Personal in Studies 1-2	Public vs. Pesonal + Public in Studies 1-2
Age	**0.170**	**0.120**	**0.0688**	**0.228**	**0.169**	**-0.0218**
	(0.118)	(0.0903)	(0.0906)	(0.177)	(0.127)	(0.129)
Male	**-0.0365**	**-0.0352**	**-0.00150**	**-0.130**	**0.00771**	**-0.0259**
	(0.0717)	(0.0440)	(0.0444)	(0.102)	(0.0641)	(0.0624)
College degree	**0.0848**	**0.0410**	**0.0248**	**0.155**	**0.0352**	**0.0560**
	(0.0762)	(0.0498)	(0.0477)	(0.109)	(0.0710)	(0.0668)
Subjective health	**-0.0922**	**0.251**	**0.326***	**-0.0225**	**0.102**	**0.200**
	(0.169)	(0.141)	(0.135)	(0.221)	(0.198)	(0.187)
Pre-existing health conditions	**-0.0339**	**-0.0496**	**-0.0455**	**-0.0555**	**-0.0399**	**0.00773**
	(0.0644)	(0.0375)	(0.0366)	(0.0939)	(0.0591)	(0.0546)
Income	**0.166**	**0.142**	**0.185***	**0.251**	**0.123**	**0.179**
	(0.105)	(0.0746)	(0.0733)	(0.147)	(0.106)	(0.103)
Conservative (vs. liberal) political ideology: Party identification	**-0.0292**	**-0.0634**	**-0.128***	**-0.00122**	**-0.146**	**-0.143**
	(0.0867)	(0.0606)	(0.0594)	(0.120)	(0.0895)	(0.0842)
Conservative (vs. liberal) political ideology: Social issues	**-0.0532**	**-0.0451**	**-0.0198**	**-0.0194**	**-0.133**	**-0.0616**
	(0.0864)	(0.0579)	(0.0569)	(0.120)	(0.0835)	(0.0802)
Conservative (vs. liberal) political ideology: Fiscal issues	**-0.0512**	**0.0334**	**-0.00500**	**0.0888**	**-0.0377**	**-0.0475**
	(0.0910)	(0.0623)	(0.0616)	(0.128)	(0.0898)	(0.0872)
Previous exposure to COVID info	**-0.120**	**0.0649**	**-0.116**	**-0.0632**	**-0.210**	**-0.237**
	(0.139)	(0.0933)	(0.0953)	(0.188)	(0.130)	(0.136)
CRT score	**0.000501**	**0.0154**	**-0.0149**	**0.0834**	**-0.0305**	**-0.0672**
	(0.0747)	(0.0456)	(0.0459)	(0.108)	(0.0638)	(0.0642)
Log-transformed population density (from zip)	**0.136**	**-0.0390**	**-0.0325**	**0.135**	**-0.174**	**-0.0741**
	(0.133)	(0.107)	(0.109)	(0.177)	(0.154)	(0.156)

Standard errors in parentheses
*** p<0.001, ** p<0.01, * p<0.05

Table S1. Individual difference variables as moderators of treatment effects. *Here we explore the extent to which our individual difference variables moderate our treatment effects. We report results from regressions predicting prevention intentions as a function of our individual difference variables, relevant condition contrasts, and their interactions, among all subjects (Columns 1-3) and subjects for whom we measured our dependent variables first (Columns 4-6). For each individual difference variable (in a series of separate regression models), shown is the interaction with (i) the overall treatment effect relative to control in Study 1 (Columns 1 and 4), and (ii) effects of the Public treatment relative to each of our other treatments, across the treatment conditions of Studies 1 and 2 (Columns 2-3 and 5-6). All coefficients are standardized coefficients, and standard errors are reported below each coefficient in parentheses. Before conducting these analyses, we (i) computed a "college degree" dummy from our measure of education, (ii) computed CRT scores (as the number of questions correct out of a possible three), and (iii) natural log-transformed our measure of population density. For our analyses of the overall treatment effect in Study 1, among all subjects n = 988 for all variables except population density, for which n = 954, and among subjects for whom we measured our dependent variables first, n = 506 for all variables except population density, for which n = 487. For our analyses of the Public treatment effects across Studies 1 and 2, among all subjects n = 1930 for all variables except population density, for which n = 1845, and among subjects for whom we measured our dependent variables first, n = 981 for all variables except population density, for which n = 935.*

Table S1 reveals that we find no compelling evidence for moderation of our treatment effects. We find no significant moderation in our analyses of subjects for whom we measured our dependent variables first. In our analyses of all subjects, we also find no significant moderation of the overall treatment effect or the comparison of the Public vs. Personal treatments. We do, however, find three significant moderators of the comparison of the Public vs. Personal+Public treatments. Specifically, as compared to Personal+Public, we observe relatively larger effects of the Public treatment among individuals who report higher subjective health, higher income, and stronger identification with the Democratic party.

However, we note that (i) all three of these interactions are significant at $p < .05$ but not at $p < .01$, (ii) we conducted a large number of exploratory analyses (creating a multiple comparisons problem), and (iii) for two of the significant interactions, conceptually related variables showed null effects (specifically, subjective health is conceptually related to pre-existing health conditions, and identification with the Democratic party is related to our other two political ideology variables). Thus, we ultimately do not see Table S1 as providing compelling evidence for moderation (without further replication).

1.3 Analyses of potential mediators

Next, we turn to discussing our potential mediators: perceived personal and public threat of coronavirus.

We begin by noting a minor programming error that occurred when measuring our potential mediators. Recall that we measured perceived personal and public threat on separate pages in random order, via two questions per construct. Our measures of personal and public threat were generally tightly matched, with the only differences between them pertaining to the distinction between personal and public threats. However, due to a programming error, they also

differed in the order in which the two questions for each construct were presented. See the "Experimental materials" section at the end of this SI for more detail.

As noted in the main text, across both studies, we find no compelling evidence that either of our potential mediators differed significantly across conditions. We do, however, find that both of our potential mediators were associated with prevention intentions, and that the association with prevention intentions was somewhat stronger for perceived *public* threat than for perceived *personal* treat. Here, we report statistical analyses supporting these claims. Despite the fact that we measured our potential mediators via questions that used slightly different wording in Study 1 versus Study 2 (see "Experimental materials" for more detail), both studies show similar patterns.

First, we support the claim that we find no compelling evidence that either of our potential mediators differed significantly across conditions. In Table S2, we report descriptive statistics for each of our potential mediators, in each condition of each study, among all subjects and among subjects for whom we measured our potential mediating variables first.

	All subjects			
	Study 1 (n = 988)		**Study 2 (n = 1188)**	
	Personal threat	**Public threat**	**Personal threat**	**Public threat**
Control	M = 54.54, SD = 29.99	M = 67.87, SD = 24.63		
Personal Treatment	M = 58.82, SD = 29.19	M = 70.57, SD = 24.46	M = 57.9, SD = 28.09	M = 70.4, SD = 22.42
Public Treatment	M = 54.75, SD = 28.51	M = 71.1, SD = 23.42	M = 57.15, SD = 29.44	M = 69.56, SD = 23.37
Personal + Public Treatment	M = 55.65, SD = 29.69	M = 71.31, SD = 22.22	M = 57.57, SD = 29.51	M = 70.44, SD = 22.41
	Mediating variables first			
	Study 1 (n = 482)		**Study 2 (n = 596)**	
	Personal threat	**Public threat**	**Personal threat**	**Public threat**
Control	M = 54.15, SD = 29.99	M = 67.43, SD = 25.53		
Personal Treatment	M = 59.94, SD = 29.35	M = 73.75, SD = 22.57	M = 56.58, SD = 27.62	M = 70.16, SD = 22.95
Public Treatment	M = 55.45, SD = 25.84	M = 71.58, SD = 21.09	M = 56.72, SD = 29.59	M = 68.91, SD = 23.51
Personal + Public Treatment	M = 51.75, SD = 31.55	M = 68.23, SD = 24.72	M = 55.38, SD = 29.4	M = 68.66, SD = 22.57

Table S2. Threat variables by study and condition. *We report the mean and standard deviation of each threat variable by condition in each study. We report results both among all subjects and subjects for whom we measured our potential mediating variables first.*

Among all subjects, we find no significant effects of any of our treatments relative to control (using a regression that takes the control as the baseline and includes dummies for each treatment), and no significant differences between any of our treatments (using Wald tests to do pairwise comparisons of the magnitudes of each treatment effect), for either study or either threat variable (all ps > .1).

Among subjects for whom we measured our potential mediating variables first, we find qualitatively identical results, with the exceptions that, in Study 1 (n = 482), the Personal treatment had (i) a significant positive effect on public threat, relative to control, b = 6.33, t =

2.10, $p = .036$, and (ii) a significant positive effect on personal threat, $F(1,478) = 4.60$, $p = .033$, as well as a marginally significant positive effect on public threat, $F(1,478) = 3.22$, $p = .074$, relative to the Personal+Public treatment.

Given the large number of comparisons we tested, we see this as weak evidence for any differences between conditions on either of our potential mediators. Furthermore, the only significant effects we *did* observe were for positive effects of the *Personal* treatment, which was the least effective of our treatments. Together, then, we see this set of results as suggesting that our potential mediators do not explain our treatment effects. For this reason, we do not report mediation analyses.

These results thus suggest that our treatments work not by increasing the perceived threat of virus, but instead through mechanisms (e.g., increasing the perceived efficacy, importance, or reputation value of engaging in prevention behaviors). They also suggest that the Public treatment is more effective than the Personal treatment not because it is especially effective at increasing the perceived threat of the virus, but instead for other reasons (e.g., because the Public treatment activates prosocial preferences, or increases the perceived moral or reputational value of prevention efforts).

Next, we support the claim that our threat variables (and especially the *public* treat variable) predict prevention intentions (see Table S3).

	All subjects			
	Study 1 (*n* = 988)		**Study 2 (*n* = 1188)**	
	Separate models	Multiple regression	Separate models	Multiple regression
Personal threat	**0.412*** **	**0.0730**	**0.401*** **	**0.0652***
	(0.0290)	(0.0393)	(0.0266)	(0.0332)
Public threat	**0.522*** **	**0.469*** **	**0.540*** **	**0.496*** **
	(0.0272)	(0.0393)	(0.0244)	(0.0332)
	Mediating variables first			
	Study 1 (*n* = 482)		**Study 2 (*n* = 596)**	
	Separate models	Multiple regression	Separate models	Multiple regression
Personal threat	**0.418*** **	**0.136***	**0.443*** **	**0.125****
	(0.0415)	(0.0573)	(0.0368)	(0.0480)
Public threat	**0.489*** **	**0.391*** **	**0.542*** **	**0.454*** **
	(0.0398)	(0.0573)	(0.0345)	(0.0480)

Standard errors in parentheses

*** p<0.001, ** p<0.01, * p<0.05

Table S3. Correlations between threat variables and prevention intentions. *Here we report results from regressions predicting prevention intentions as a function of our threat variables. Shown are results from (i) a set of separate regression models for each threat variable (Columns 1 and 3) and (ii) one multiple regression model using both threat variables (Columns 2 and 4), for Study 1 (Columns 1-2) and Study 2 (Columns 3-4). We report results both among all subjects and subjects for whom we measured our potential mediating variables first. All coefficients are standardized coefficients, and standard errors are reported below each coefficient in parentheses.*

Table S3 reveals that both threat variables predict prevention intentions. It also reveals that, both in the context of our single and multiple regressions, perceived *public* threat is a somewhat stronger predictor than perceived *personal* threat. The difference in predictive power between public and personal threat is especially large in our multiple regressions; however, our multiple regression results should be interpreted with caution given the high covariance between our two threat variables (among all subjects, $r = .72$ in Study 1 and $r = .68$ in Study 2; among subjects for whom we measured our potential mediating variables first, $r = .72$ in Study 1 and $r = .70$ in Study 2, all ps $< .001$).

1.4 Moderation by subjective health

Next, we discuss moderation by subjective health in more detail. As noted in the main text, in Study 1, we found some evidence that individuals reporting greater subjective health showed relatively larger effects of the Public treatment. This result makes theoretical sense: healthy individuals are at lower risk for coronavirus, and thus should be less likely to see prevention behaviors as self-interested and more likely to treat them like a public good. Thus, in our Study 2 pre-registration, we planned for our primary analyses to focus specifically on healthier individuals (defined as individuals reporting subjective health above the Study 1 median). However, evidence for an interaction between health and our Public treatment effects was weaker in Study 2 than in Study 1. Thus, despite the fact that moderation by subjective health makes theoretical sense, we did not feel confident focusing on health in our primary analyses, and instead chose to report main effects among all subjects.

Here, however, we report detailed analyses of subjective health (see Table S4). Our objective in doing so is to provide transparency with respect to our pre-registered plan to focus on health in Study 2. Thus, because our pre-registrations only planned analyses of all subjects, for brevity in this section we do not additionally report analyses of subjects for whom we measured our dependent variables first.

Table S4 investigates the effects of the Public treatment, relative to our other treatments, as a function of subjective health. We report separate analyses of the treatment conditions of (i) Study 1 (Column 1), (ii) Study 2 (Column 2), and (iii) the Studies 1 and 2 combined (Column 3). In these analyses, we compare the Public treatment to the Personal treatment (top rows), and to the Personal+Public treatment (bottom rows). For each comparison, we report (i) the relative effect of the Public treatment, separately among healthier and less healthy subjects, and (ii) the interaction between (continuous) health and the Public (vs. other) treatment.

	Study 1 (Healthier n = 375, Less healthy n = 367)	**Study 2** (Healthier n = 560, Less healthy n = 628)	**Studies 1 and 2** (Healthier n = 935, Less healthy n = 995)
Public vs. Personal			
Healthier	**b = 4.58, t = 2.34,** p = .020	**b = 2.55, t = 1.75,** p = .080	**b = 3.35, t = 2.86,** p = .004
Less healthy	**b = 0.72, t = 0.33,** p = .739	**b = 2.25, t = 1.41,** p = .159	**b = 1.69, t = 1.31,** p = .189
Interaction (with continuous health)	**b = 1.78, t = 1.26,** p = .208	**b = 1.33, t = 1.25,** p = .212	**b = 1.51, t = 1.77,** p = .076
Public vs. Personal + Public			
Healthier	**b = 6.07, t = 3.02,** p = .003	**b = 1.52, t = 1.03,** p = .304	**b = 3.32, t = 2.77,** p = .006
Less healthy	**b = -1.95, t = -0.91,** p = .361	**b = 0.23, t = 0.15,** p = .883	**b = -.58, t = -.45,** p = .651
Interaction (with continuous health)	**b = 3.39, t = 2.44,** p = .015	**b = 1.12, t = 1.09,** p = .278	**b = 2.00, t = 2.41,** p = .016

Table S4. Effects of the Public treatment as a function of subjective health. *Here we report effects of the Public treatment, both relative to the Personal treatment and the Personal+Public treatment. For each treatment contrast, we report the effect of the Public (vs. other) treatment among healthier and less healthy individuals, as well as the interaction between our (continuous) subjective health measure and the Public (vs. other) treatment. We report these analyses across the treatment conditions of (i) Study 1 (Column 1) (n = 742), (ii) Study 2 (Column 2) (n = 1188), and (ii) Studies 1 and 2 combined (Column 3) (n = 1930). Results come from analyses of all subjects; we do not report analyses among subjects for whom we measured our dependent variables first.*

Table S4 reveals that when we restrict our analyses to healthier individuals, as planned in our Study 2 pre-registration, we continue to support (and actually provide even stronger evidence for) our key finding that the Public treatment was more effective than the Personal treatment, and no less effective than the Personal+Public treatment. However, Table S4 also reveals that the evidence for an interaction between health and our Public treatment effects was weaker in Study 2 than in Study 1. Thus, our results do not ultimate provide clear support for the proposal that the Public treatment is especially effective among healthier individuals.

1.5 Analyses excluding subjects who failed an "English check"

In our pre-registrations, we planned to conduct secondary analyses that exclude subjects who appear not to speak English, on the basis of incorrect answers to a simple analogy question or incoherent responses to a free-response question (about plans for the subject's day). We coded answers to the simple analogy question (in a way that was blind to condition) for correct or near-correct answers (i.e., correct answers with typos/misspellings); across both studies (and all subjects), 6.99% of responses were incorrect. A visual scan of our data revealed that most subjects who answered the analogy question incorrectly provided incoherent and/or irrelevant responses to the free-response question, while the vast majority of subjects who answered the analogy question correctly provided coherent and relevant answers. On this basis, we repeated

our analyses excluding subjects who incorrectly answered the analogy question. We found that our results were unchanged qualitatively, but most patterns became a bit stronger. For brevity, we do not report these analyses; however, our "English check" data are available to interested readers.

2. Discussion of pre-registered analysis plans

As noted in the main text, both Study 1 (https://aspredicted.org/blind.php?x=mb9t3e) and Study 2 (https://aspredicted.org/blind.php?x=w8jk9m) were pre-registered. We adhered closely to our pre-registered analysis plans, with a few exceptions. The substantive exceptions are noted in the main text where relevant, but here we provide one comprehensive list of all deviations.

First, in both of our pre-registrations, we planned only to report results among all subjects, and not to explore the order in which we measured our dependent variables versus potential mediators. However, after completing both studies, we discovered an unexpected interaction between condition and order. Thus, to confirm the robustness of our results, for analyses of our dependent variables, we report results (i) among all subjects, and (ii) among subjects for whom we measured our dependent variables *before* measuring our potential mediators. And for analyses of our potential mediators, we report results (i) among all subjects, and (ii) among subjects for whom we measured our potential mediators *before* measuring our dependent variables.

Second, in our Study 1 pre-registration, we planned to focus equally on both of our dependent variables (i.e., prevention intentions and social distancing intentions). However, as mentioned in main text, in Study 1 the prevention intentions variable produced stronger evidence for treatment effects and interesting differences between treatments, and thus in Study 2 we chose to focus on replicating these results. For this reason, we focus our paper on prevention intentions. Specifically, in the main text, we report primary analyses of social distancing intentions (i.e., treatment effects relative to control, and comparisons between treatment effects). But nowhere in our paper do we report analyses of the relationships between social distancing intentions and our individual difference variables or candidate mediators, or heterogeneity in treatment effects on intentions to avoid individual social behaviors.

Third, in our Study 1 pre-registration, we planned to compare all pairs of treatments to each other. However, given our pattern of results, we chose to focus on the comparison of the Public treatment to each of the other two treatments, and thus we do not compare the Personal treatment to the Public+Personal treatment. We pre-registered this plan before running Study 2.

Fourth, in our Study 1 pre-registration, we planned, as a secondary analysis, to explore treatment effects on intentions to engage in individual prevention behaviors, and to avoid individual social activities. Additionally, we noted that we were in particular concerned about ceiling effects, and thus would repeat our primary analyses looking only to the prevention behaviors and social activities for which baseline responses were the relatively lowest (i.e., furthest from ceiling). We did, in fact, explore individual prevention behaviors (see Figure S1), and in the main text we also report an analysis of the overall social distancing item included in our composite measure of prevention intentions. But because we did find treatment effects on prevention intentions (i.e., there was not a ceiling effect) and we found no significant heterogeneity across individual behaviors, we did not repeat our primary analyses looking only to behaviors furthest from ceiling. We note, however, that Figure S1 sorts individual behaviors by average baseline responses (i.e., distance from ceiling) for interested readers. (As noted

above, we also did not explore intentions to avoid individual social activities, given our primary focus on our prevention intentions dependent variable).

Fifth, in our Study 2 pre-registration, we planned for our primary analyses to focus specifically on healthier individuals (defined as individuals reporting subjective health above the Study 1 median). As noted in the main text and the "moderation by subjective health" section of this SI, this decision reflected that, in Study 1, we found evidence suggesting that healthier individuals show relatively larger Public treatment effects.

However, evidence for an interaction between health and our Public treatment effects was weaker in Study 2 than in Study 1. Thus, despite the fact that this interaction pattern makes theoretical sense, we did not feel confident focusing on it in our primary analyses, and instead chose to focus primarily on main effects among all subjects. We note, however, that as shown in the "moderation by subjective health" section of this SI, analyses of healthy individuals support (and actually provide even stronger evidence for) our key finding that the Public treatment was more effective than the Personal treatment, and no less effective than the Personal+Public treatment.

Relatedly, in our Study 2 pre-registration, we also planned, as a secondary analyses, to (i) repeat our primary analyses among subjects reporting zero pre-existing health conditions, and (ii) test for interactions between pre-existing health conditions and the Public treatment. But because we chose not to focus extensively on moderation by health, we do not report these analyses.

Finally, our pre-registrations did not plan to explore the associations between our individual difference variables and prevention intentions (reported in Table 3). However, we chose to include these as unplanned exploratory analyses for interested readers.

3. Experimental materials

Here, we provide the full text of our all measures collected in our studies. Additionally, PDFs of all experimental materials used in both studies are available online at https://osf.io/sr4n9/.

3.1 Dependent variables

Measure of prevention intentions:

Note that these items were presented in the fixed order shown here.

Measure of social distancing intentions:

Note that these items were presented in the fixed order shown here.

On the previous page, we asked you about your intentions to avoid engaging in a variety of behaviors.

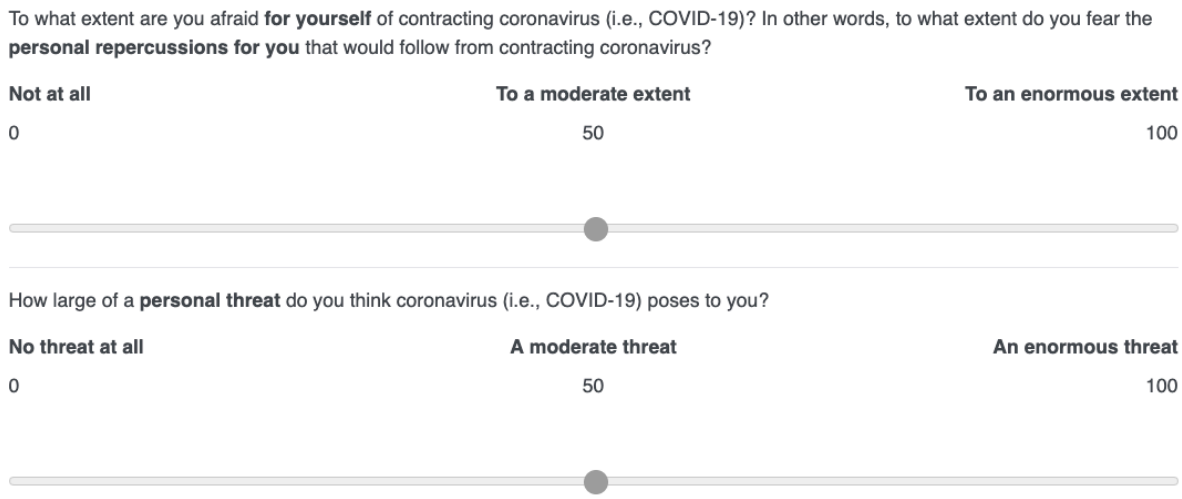
If coronavirus were NOT a concern, which of these these behaviors would you at least sometimes engage in? **Please check all that apply.**

- ☐ Going to bars
- ☐ Going to restaurants
- ☐ Going to coffee shops
- ☐ Going to the grocery store
- ☐ Going to the gym
- ☐ Going to work (somewhere outside of your home)
- ☐ Using public transportation
- ☐ Going to the airport and flying
- ☐ Socializing in small gatherings
- ☐ Attending large events or gathering

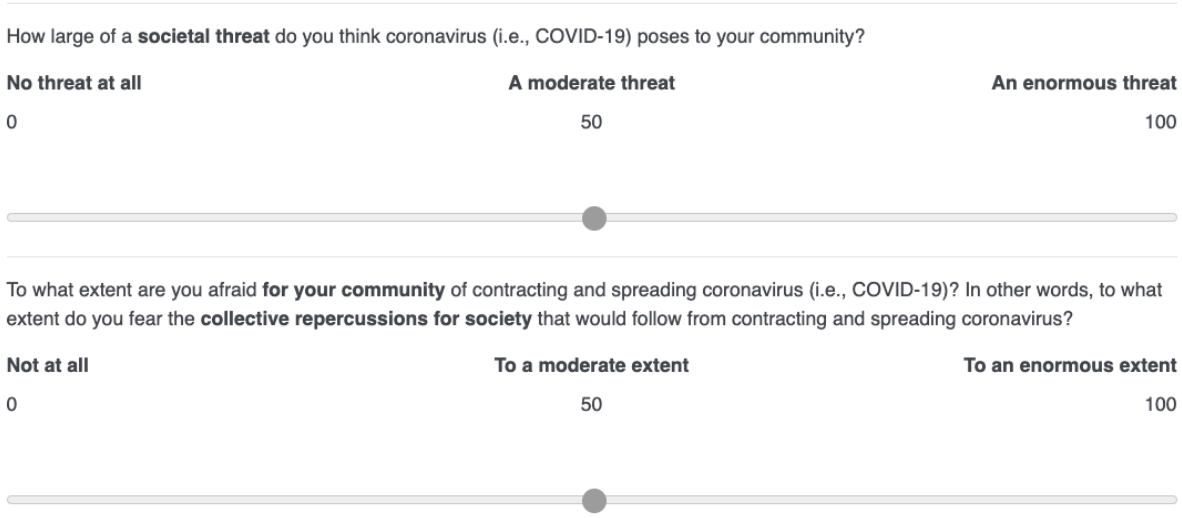
Note that these items were presented in the fixed order shown here.

3.2 Potential mediators

Study 1 measure of perceived personal threat:

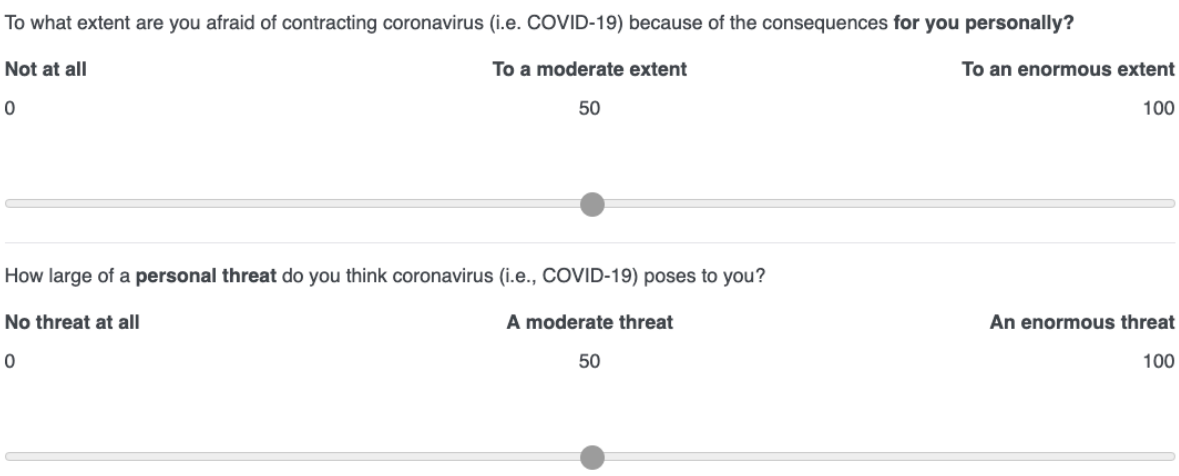


Study 1 measure of perceived public threat:

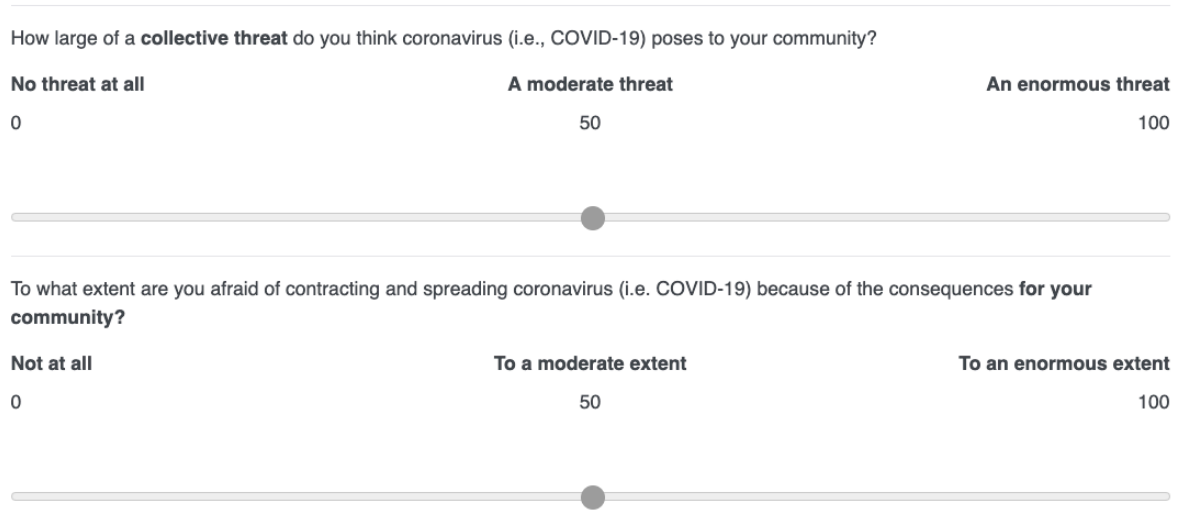


Note that these two pages were measured in random order, but the order of questions on each page was fixed in the order shown. Due to a programming error, this meant that the question starting with "to what extent are you afraid" was shown first when measuring *personal* threat, and second when measuring *public* threat.

Study 2 measure of perceived personal threat:



Study 2 measure of perceived public threat:



Note that these two pages were measured in random order, but the order of questions on each page was fixed in the order shown. Due to a programming error, this meant that the question starting with "to what extent are you afraid" was shown first when measuring *personal* threat, and second when measuring *public* threat.

3.3 Individual difference variables

Items besides the Cognitive Reflection Task:

Please answer the following questions about yourself.

Age:

Gender

- ○ Male
- ○ Female

Highest level of education completed:

- ○ Less than a high school degree
- ○ High School Diploma
- ○ Vocational Training
- ○ Attended College
- ○ Bachelor's Degree
- ○ Graduate Degree
- ○ Unknown

What is your zip code?

Overall, taking into account any health conditions that you have, how would you evaluate your health?

Terrible	Very bad	Bad	Moderate	Good	Very good	Excellent
○	○	○	○	○	○	○

Please consider the following list of health conditions:

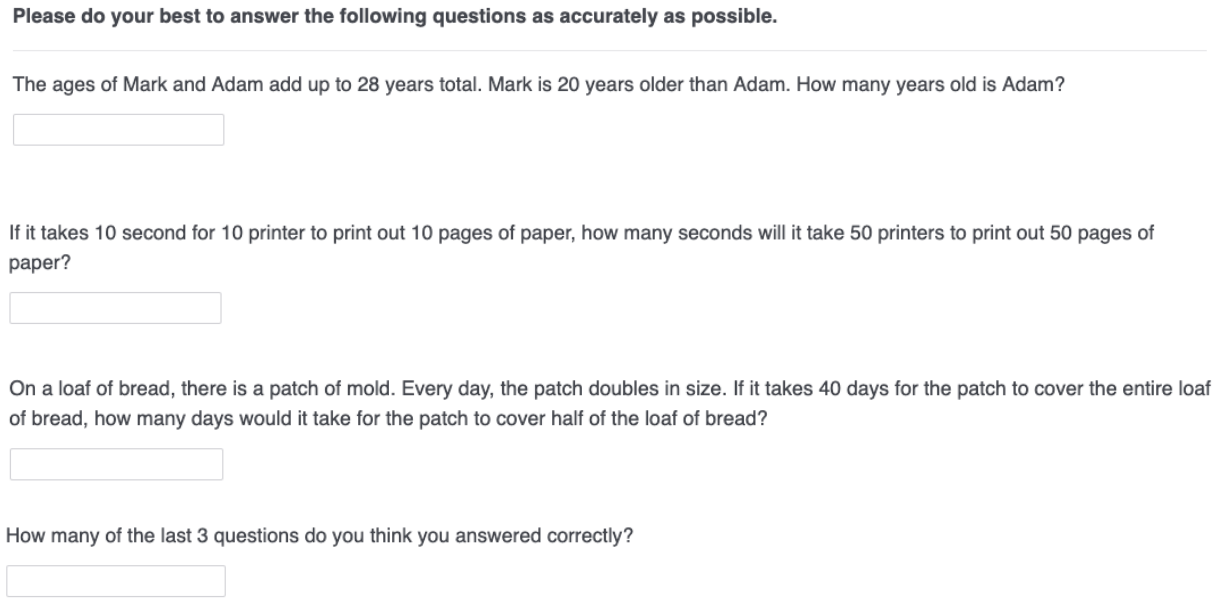
Cardiovascular diseases, diabetes, hepatitis B, chronic obstructive pulmonary disease, chronic kidney diseases, and cancer.

How many of these conditions do you have?

- ○ 0
- ○ 1
- ○ 2
- ○ 3
- ○ 4
- ○ 5 or more

Note that these items were presented in the fixed order shown here. Note also that to compute a "college degree" dummy from our categorical education variable, we coded the data as follows: 0 = "less than a high school degree", "high school diploma", "vocational training", "attended college", or "unknown"; 1 = "bachelor's degree" or "graduate degree".

Cognitive Reflection Task:



Note that each of these four questions was presented on a separate page, in the fixed order shown here. We did not analyze responses to the final question (about the number of questions answered correctly). Correct answers to the first three questions: 4, 10, 39.

English check: