American Psychological Association

Journal of Experimental Psychology: General

0096-3445/19/$12.00

2019, Vol. 148, No. 3, 550–569
http://dx.doi.org/10.1037/xge0000563

The Collective Aggregation Effect: Aggregating Potential Collective Action Increases Prosocial Behavior

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The authors investigated the effectiveness of aggregating over potential noncontingent collective action ("If X people all do Y action, then Z outcomes will be achieved") to increase prosocial behavior. They carried out 6 experiments encouraging 4 different prosocial activities and found that aggregating potential benefits over 1,000 people produced more prosocial intentions and actions than aggregating over 1 person did. The authors further showed that aggregating potential benefits over 1,000 people produced more prosocial intentions than aggregating benefits over 1,000 days did. This collective aggregation effect was due to the presentation of larger aggregated benefits (Experiments 1–6), attenuation of psychological discounting (Experiment 4), and increased perceptions of outcome efficacy (Experiments 5–6). The effect was not due to social norms (Experiment 3) or a simple anchoring process (Experiments 4–5). Often individual contributions to societal ills seem like mere "drops in a bucket"; collective aggregation helps by making individual actions seem bucket-sized, immediate, important, and effective.

Keywords: numerosity, outcome efficacy, prosocial behavior, psychological discounting

> If everyone reading this gave $5, our fundraiser would be done within an hour.
>
> —Jimmy Wales, *Wikipedia* founder

According to a 2008 Walmart TV commercial, if every Walmart shopper—more than 200 million Americans—replaced just one incandescent light bulb with a compact fluorescent light bulb, "it would be like taking 11 million cars off the road." Over the lifetime of the bulbs, this would be equivalent to a total of more than $9 billion saved and more than 50 million tons of greenhouse gases avoided. Marketing statements of the form "If X people all do Y action, then Z outcomes will be achieved" are commonplace when one starts to look for them. For example, as shown in the epigraph, each year *Wikipedia* runs a donation program fronted with such a statement. Nor has the power of potential collective action been lost on environmental conservationists. Earth Hour is an effort that encourages households and businesses to turn off their nonessential lights for a period of 1 hr. In 2012, under the campaign slogan "I will if you will," it was reported that more than a billion people globally turned off their lights (Cubby, 2012). Given these examples, it is surprising that there exists no specific research examining whether it is useful to describe potential behavioral outcomes, particularly those in the prosocial domain, using this basic structure and, if so, the theoretical reasons why. The current research aimed to remedy that gap.

There are several interesting features of the statement "If X people all do Y action, then Z outcomes will be achieved." First, it is hypothetical: The cooperative behavior of others is in no way certain or even promised. Second, it is noncontingent: If one decides to act, then this decision and its outcome occur regardless of what others do. These two features distinguish the structure from other practices that rely on reciprocation of cooperative behavior such as the use of small gifts to solicit donations (cf. Cialdini, 2001). Third, although impressively large outcomes can be described after aggregation (e.g., $9 billion saved in total) the individual may reap only a small fraction of those outcomes (e.g., $47 saved by each person).

Our overarching hypothesis was that presenting outcomes aggregated over potential collective action would increase prosociality. This expectation derived from two literatures. First, based on the numerosity literature, we expected large numerators (e.g., $9 billion saved in total) to be given more weight than the associated denominators (e.g., 200 million Walmart shoppers). Second, based on the efficacy literature, we expected large numerators to boost feelings of outcome efficacy; that is, belief that actions will have a meaningful impact.

We find that presenting outcomes that are aggregated over potential collective action produces more prosocial motivation and actions compared to presenting the equivalent nonaggregated out-

Editor's Note. Hal R. Arkes served as the action editor for this article. —NC

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Parts of this research were presented at the 2019 Society for Consumer Psychology Boutique Conference, the 2017 and 2016 Association for Consumer Research North American Conferences, and the 2015 World Social Marketing Conference. Adrian R. Camilleri was supported by an American Australia Fellowship and a Duke Bass Connections Award.

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comes or when describing the same potential outcomes by aggregating over time. We show that this collective aggregation effect is driven by the magnitude of the numerator and via increased outcome efficacy perceptions. We conclude that collective aggregation may be a unique and effective mechanism for encouraging prosocial behavior in a range of contexts.

Numerosity

The contribution of an individual person to a large societal problem is often small; in most cases, equivalent to a small drop in a large bucket. One implication of the statement "If X people all do Y action, then Z outcomes will be achieved" is that the drop-in-the-bucket metaphor is scaled up: The contribution becomes bucket-sized. The contrast between a drop-sized and bucket-sized contribution can be impressive if people ignore the fact that the metaphorical container has also significantly increased in size. This sort of neglect has been demonstrated in several lines of research. For example, people tend to focus on numerators relative to denominators: Many people prefer to own nine tickets in a 100-ticket lottery than own one ticket in a 10-ticket lottery despite the statistical dominance of the latter scenario (Kirkpatrick & Epstein, 1992). This tendency, which has been labeled *the numerosity heuristic* and also *the scale expansion effect,* reveals that people judge the amount of something based on the number of units the stimuli is represented by while underweighting other important information such as the size of the units (Gourville, 1998; Pelham, Sumarta, & Myaskovsky, 1994; Wertenbroch, Soman, & Chattopadhyay, 2007). Some explanations for why people rely on the numerosity heuristic include anchoring (Pandelaere, Briers, & Lembregts, 2011), construal (Monga & Bagchi, 2012), and conversational norms (Zhang & Schwarz, 2012).

In recent years, the importance of numerosity has become of great interest to those studying consumer behavior (Adaval, 2013). The growing body of research in this area shows that rescaling otherwise identical ratio information can systematically change preferences in multiattribute choice (Bagchi & Li, 2011; Burson, Larrick, & Lynch, 2009). The consistent finding is that people tend to perceive differences as larger when they are expressed on an expanded scale than when they are expressed on a contracted scale. For example, people perceive the difference between 108 and 84 months warranty as larger than the difference between the equivalent 9 and 7 years' warranty (Pandelaere et al., 2011). Larger differences in turn prompt greater reliance on that dimension in choice, thereby increasing preference for the option favored on that dimension. For example, people's tendency to select a relatively expensive but fuel-efficient vehicle over a relatively cheap but fuel-inefficient vehicle increased when gas consumption and cost information was expanded from "per 100" miles to "per 100,000" miles (Camilleri & Larrick, 2014).

The examples just described increase the numerator by aggregating over physical units, such as time (e.g., 1 vs. 100 days; cf. Gourville's, 1998, pennies-a-day strategy) or distance (100 vs. 100,000 miles). The statement "If X people all do Y action, then Z outcomes will be achieved" employs a unique scale-expansion policy by aggregating over people. Aggregating potential prosocial actions, such as giving a $1 charity donation over 1,000 days or over 1,000 people produces the equivalent amount: The new, larger numerator describing the total amount is equal ($1,000). We expected numerosity findings to extend to situations in which the scale was expanded by aggregating over many people because the underlying mechanism driving the numerosity effect is increased attention to larger numerators.

> *Hypothesis 1 (H1):* People will be more likely to engage in prosocial actions when the potential benefits from such actions are aggregated over many people rather than one (the collective aggregation effect).

Previous research on social norms has shown that evoking the past behavior of a large number of people increases the motivation to engage in that behavior. For example, telling people that "most other people pay their taxes on time" increases compliance rates on tax filing (Cabinet Office, Behavioural Insights Team, 2012). We argue that the numerosity explanation is separate from a social norms explanation. Specifically, a numerosity explanation of the collective aggregation effect depends on the magnitude of the benefit expressed by the numerator and not simply the presence of a large denominator (such as a 1,000 people). We argue that using a statement such as "If 1,000 people donated their time to reduce hunger, there would be a large benefit" may evoke a norm, but the denominator alone will be less effective at persuading people than when a large numerator of benefits is also presented.

> *Hypothesis 2 (H2):* The collective aggregation effect will be attenuated when denominator information is presented in the absence of numerator information.

We also expected that the nature of aggregation matters: The same large numerator will have a different psychological effect if it comes from aggregating over people than over time. In the next sections, we discuss two processes that make aggregation over people more effective than over time. The first is that aggregating over many people in the present yields an immediate benefit, whereas aggregating over long periods of time delays benefits into the distant future, and delayed benefits are heavily discounted. The second is that aggregating over people (but not time) increases a sense of outcome efficacy.

Time Discounting

One difference between aggregating over many days (for one person) and aggregating over many persons (for 1 day) is the time horizon involved. Much research has shown that, when it comes to evaluating potential benefits and costs, the future is far less important than the present (Hardisty & Weber, 2009). Correspondingly, people tend to discount future benefits and costs, usually according to a hyperbolic or quasi-hyperbolic function.

The effect of aggregating potential benefits over time, compared to aggregating over people, may be reduced because of psychological time discounting. By comparison, aggregating over people may bypass this psychological discounting because the potential savings can be described as occurring in the immediate future. Such an effect would contribute to collective aggregation's being more motivating than time aggregation is. We sought to provide evidence for this explanation by demonstrating that psychological discounting moderates the influence of aggregating over time but not the influence of aggregating of people, such that the difference between aggregation policies is greatest for those with higher time discounting.

Efficacy

One of the ways aggregating potential collective action might increase prosocial motivation is by boosting the feeling that an actor has an ability to produce a desired result. The literature has termed this perceived ability *efficacy*. Efficacy is a central construct in many theories of motivation, including social–cognitive theory (Bandura, 1977, 1986) and the theory of planned behavior (Ajzen, 1991). These highly influential theories have been used to predict behavior across a wide range of settings, including education, marketing, work, and health (Armitage & Conner, 2001; Bandura, 1997; Luszczynska & Schwarzer, 2005).

Theoretically, four types of efficacy, which are formed by crossing the target of perception (of ability or outcome) against reference unit (individual or collective; Bandura, 1986, 1997; Koletsou & Mancy, 2011), have been discussed. We describe each efficacy type with reference to the example of an individual deciding how much money to donate to a charity that fights world hunger. This is a societal level goal that can be achieved only collectively; thus, all four forms of efficacy are relevant.

Individual ability efficacy refers to a person's perception of their own ability to perform a particular behavior (e.g., belief regarding their power to donate money). *Individual outcome efficacy* refers to their perception of how well or likely that particular behavior will produce the desired result (e.g., belief that their donating money will contribute meaningfully to fight world hunger). *Collective ability efficacy* refers to a person's perception of their group's ability to perform a particular behavior (e.g., belief regarding most other people's ability to donate money). *Collective outcome efficacy* refers to their perception of how well or likely their group's actions will produce the desired result (e.g., belief that if most other members of society donated money, then this will contribute meaningfully to fighting world hunger). In general, the higher a person's efficacy, the more likely that person is to engage in a particular action (Bandura, 1997).

We focus on outcome efficacy because perceptions of how well an action produces an outcome is intimately connected with the magnitude of that outcome, which collective aggregation manipulates. Logically, outcome efficacy beliefs should be determined by the size of an action's contribution to the desired result. For example, imagine that the desired result is to eliminate world hunger and that achieving this requires one billion grains of rice to be donated. An action that is expected to result in one million grains of rice being donated should produce a higher outcome efficacy than an action expected to produce one thousand grains of rice.

Based on the numerosity literature, we proposed that the perceived size of the contribution—and therefore the associated outcome efficacy beliefs—can be increased by aggregating potential collective actions. Continuing the same example, a *collection* of actions that are expected to result in one million grains of rice being donated should be more motivating than an *individual* action expected to produce one thousand grains of rice. In short, we hypothesized that people will attend more to the numerator—the size of the contribution (1,000,000 vs. 1,000)—and respond less strongly to the denominator: the number of individuals in the collective (1,000 vs. 1). Thus, through aggregation, an individual drop becomes bucket-sized. We expected that this large collective numerator would translate into a sense of collective outcome efficacy that would then directly influence individual motivation.

Several literatures have shown that increases in collective outcome efficacy produce greater motivation. In the context of cooperating, interacting teams, a higher degree of collective outcome efficacy is associated with better group performance (Mulvey & Klein, 1998; Prussia & Kinicki, 1996; Riggs & Knight, 1994). Higher collective efficacy is also positively associated with a variety of individual societal behaviors such as voting, donating, volunteering, and engaging in activism (Doherty & Webler, 2016; Lee, 2006; Roser-Renouf, Maibach, Leiserowitz, & Zhao, 2014). For example, in the context of proenvironmental behavior, a higher degree of collective outcome efficacy is associated with more proenvironmental intentions (Chen, 2015; Homburg & Stolberg, 2006; Jugert et al., 2016).

One potential danger of collective aggregation, however, is that it can set up the temptation to free-ride, which has been discussed at length in the social dilemmas literature (e.g., Dawes, 1980). Specifically, participants who picture 1,000 people donating may take advantage of the contributions of others at no personal cost. In this case, expecting others to contribute may lead people to hold back because they expect others to take care of the problem. This temptation is often compounded by the "sucker effect"—people want to avoid making a contribution that others take advantage of by not contributing (Kerr, 1983). We argue that aggregating the benefits of potential collective action may reduce these fears and temptations. Indeed, people do not always act selfishly (Crocker, Canevello, & Brown, 2017) and are often "conditional cooperators": They will reciprocate what they see or expect others to (Fehr & Fischbacher, 2004). We argue that aggregating the benefits of prosocial actions from many people creates an image that others are or will be contributing and will motivate greater contributions. Metaphorically, it is motivating to add a drop to a large bucket that appears to be on its way to being filled. Thus, we expected that people would be more motivated when they picture a large collective contribution—even though the contributions from others are purely hypothetical and not guaranteed.

Hypothesis 3 (H3): People will be more likely to engage in prosocial actions when the potential benefits from such actions are aggregated over many people compared to when they are aggregated over many days (holding constant the magnitude of the benefit).

Hypothesis 4 (H4): People will be more likely to perceive higher outcome efficacy when the potential benefits from action are aggregated over many people.

Hypothesis 5 (H5): The collective aggregation effect will be mediated by perceptions of outcome efficacy.

Experiment 1

We conducted Experiment 1 to test H1. We examined a range of aggregation sizes (1; 10; 100; 1,000; and 10,000 people) to test the basic hypothesis that larger aggregations would become increasingly more persuasive. In Experiment 1 (and also Experiments 3 and 4), the target action was to reduce TV watching to reduce energy use.

Method

The research had Duke University Institutional Board Review approval. For each study, data collection was completed entirely before commencing data analysis. To ensure sufficient power for a medium effect size, we aimed for a sample size of 50 to 100 per cell in our designs.

Participants. The participants were 506 American respondents (M_{age} = 34.94 years, SD = 11.20; 278 female) recruited from Amazon Mechanical Turk (AMT). In general, AMT participants are more nationally representative of the general population than are typical in-person convenience samples such as college students (Berinsky, Huber, & Lenz, 2012; Paolacci & Chandler, 2014); nonetheless, AMT participants generally underrepresent older and richer members of the population.

Design. The experiment tested a single independent variable with five levels in a between-subjects design. Potential benefits of prosocial action were aggregated over 1 person, 10 people, 100 people, 1,000 people, or 10,000 people. The numerator—CO_2 savings—ranged from 1.19 lb (539.8 g; 1 person) to 11,900 pounds (5,398,000 g; 10,000 people) across these group sizes. Note that the rate of impact (i.e., the numerator [the amount of savings] divided by the denominator [i.e., the number of people]) was equivalent across groups. The main dependent variable was the participants' evaluation of the persuasiveness of the message on a 10-point scale ranging from 1 (*Not at all persuasive*) to 10 (*Extremely persuasive*).

Procedure and materials. The experiment took a median of 5.4 min to complete. After agreeing to complete the experiment, participants were directed to a page stating that watching TV is a popular form of entertainment but that powering the TV requires electricity, which has both financial and environmental costs. The purpose of the study was stated to be an evaluation of some messaging designed to encourage people to reduce their TV usage. On the next page, participants were presented with a single message (see Appendix A), and participants indicated its persuasiveness. There was also a free-response text box where participants could expand upon their response. On the next page, participants answered an attention-check question and a manipulation-check question. On the next page, participants were asked about the number of hours of TV they themselves watched each day on average as well as their belief about others. On the next page, participants were asked to complete the New Ecological Paradigm—Revised scale (NEPr; Dunlap, Van Liere, Mertig, & Jones, 2000), which measures proenvironmental worldview. Participants answered 15 questions on a 5-point scale from 1 (*Strongly agree*) to 5 (*Strongly disagree*). Scores on the NEPr range from 15 to 75, with higher scores indicating more proenvironment attitudes. We observed a Cronbach's alpha of .87, which is an acceptable item reliability assuming a unidimensional construct. Participants were thanked and debriefed.

Results

Attention check. Four participants failed the attention-check question. In this and all subsequent experiments, we report analyses including all data and report all changes in statistical significance when compared to the same analyses conducted on the data excluding those who failed the attention-check question.

Persuasiveness. As expected, the average degree of persuasiveness generally increased monotonically with aggregation size (see Figure 1). A one-way analysis of variance (ANOVA) with persons entered as the independent variable and persuasiveness entered as the dependent variable revealed a significant main effect of persons, $F(1, 501) = 6.40$, $p < .0001$, $\omega^2 = .041$. Follow-up comparisons using Tukey's honestly significant difference (HSD) ($\alpha = .05$), which compares all possible pairs of means, support H1: Persuasiveness was higher for those in the 1,000-people group than for those in the 1-person group, $F(1, 501) = 20.91$, $p < .0001$, $d = .63$; those in the 10-people group, $F(1, 501) = 12.32$, $p = .0005$, $d = .48$; and also those in the 100-people group, $F(1, 501) = 10.03$, $p = .002$, $d = .43$. Additionally, persuasiveness was higher for those in the 10,000-people group than for those in the 1-person group, $F(1, 501) = 8.62$, $p = .004$, $d = .41$. The remaining pairwise comparisons revealed no significant differences.

We also conducted a linear regression analysis with the logged group value entered as a continuous independent variable and persuasiveness entered as the dependent variable. In this regression model, all of the other measured variables (i.e., demographics, NEPr, TV-viewing behavior) were also entered as covariates. The analysis revealed a main effect for number of persons ($b = .30$), $t(493) = 3.84$, $p = .0001$, $\omega^2 = .025$; a main effect for NEPr ($b = .05$), $t(493) = 3.87$, $p = .0001$, $\omega^2 = .025$; and a main effect for gender ($b = .23$), $t(493) = 1.99$, $p = .05$, $\omega^2 = .005$.

Discussion

The data collected in Experiment 1 demonstrate that aggregating the benefits of potential collective action over many people is more persuasive than is aggregating over fewer people, particularly a single person. On average, we observed a linear trend such that persuasion increased as the number of people being aggregated over increased. Additionally, persuasiveness was rated higher for those who were female and those with a more proenvironmental worldview. We note that the most effective aggregation size for

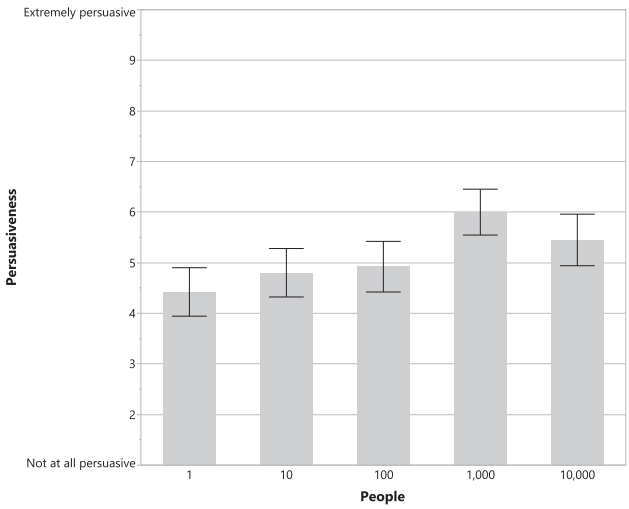

Figure 1. Average persuasiveness as a function of the number of people aggregated over in Experiment 1. Error bars correspond to 95% confidence intervals.

these stimuli appeared to be 1,000 people ("preventing 1,190 pounds of CO_2 being released into the environment"). It may be the case that there are offsetting effects as aggregation size increases—the larger numerator is more impressive, but diminishing sensitivity reduces the impact of large numbers. Extremely large numbers may be difficult to process, especially for those low in numeracy. Alternatively, plausibility of the denominator may decrease as the number of people aggregated over increases.

Experiment 2

We conducted Experiment 2 to again test H1 but this time in a purely altruistic context and measuring actual behavior. In Experiment 2 (and also 5), the target action was to encourage people to navigate to an ad-supported website that allowed users to donate to charity by answering multiple-choice quiz games.

Method

Participants. The participants were 202 American respondents (M_{age} = 30.98, *SD* = 11.08; 71 female) recruited from AMT.

Design. This experiment tested a simpler design than Experiment 1 did by examining two levels of aggregation: 1 person or 1,000 people donating to charity. There were three dependent variables. The first variable was participants' response to a statement asking how persuasive the presented information was on a 10-point scale. The second variable was the binary decision of whether or not participants chose to navigate to an external website (freerice.com) where they could answer questions to donate grains of rice. The third variable was the number of self-reported charity actions completed.

Procedure and materials. The experiment was conducted online and took a median of 4.0 min to complete. After agreeing to complete the experiment, participants were directed to a page stating that many people around the world were hungry, that one of the basic food items that could help these people was rice, and that websites that donate rice on behalf of people who correctly answer questions exist. The page concluded with a statement about the potential benefits of making a charity donation at one of these websites. The manipulated sentence read: "If 1 person (1,000 people) correctly answered 50 questions, then they could in total donate 5,000 (5,000,000) grains, or approximately .5 (500) cups, of rice. This amount could contribute approximately 1 (1,000) meals to the hungry." The participants were then asked to indicate how persuasive the presented information was.

On the next page, participants were told that they would be given the opportunity to visit one of these websites, which would donate 10 grains of rice on their behalf for correct English vocabulary question answered. Participants were again presented with one of the aforementioned manipulated potential benefits statement before being asked, "What would you like to do next?" The two options were to go to the website where they could answer questions to donate grains of rice or to move on to the next section of the study. Participants who chose to go to the external website were presented with a brief explanation of the freerice.com website, encouraged to go to the website to answer some questions before returning to complete the experiment, and then directed to the freerice.com website. Upon returning to the experiment, these participants were asked how many questions they correctly answered and were given a free-response text box to explain why they stopped answering questions when they did. Participants who chose to continue to the next section of the study skipped this part of the experiment entirely. On the next page, participants answered two attention-check questions and a number of demographic items before being thanked and debriefed.

Results

Persuasiveness. The average level of persuasiveness was significantly higher for those in the 1,000-people group (*M* = 7.7, *SD* = 2.1) compared to those in the 1-person group (*M* = 6.9, *SD* = 2.4), $t(200) = 2.52$, $p = .01$, $\omega^2 = .026$.

Choice. The proportion of people who chose to visit the external website to donate was significantly higher for those in the 1,000-people group (59.4%) compared to those in the 1-person group (39.6%), $\chi^2(1, N = 202) = 7.92$, $p = .005$, $\varnothing_c = .20$.

Actions. Given the highly skewed nature of the actions data, we report medians and test for differences using Mood's median test, which is appropriate when data are skewed (Siegel & Castellan, 1988). The median number of questions answered at the charity donation website was significantly higher for those in the 1,000-people group (5.0) compared to those in the 1-person group (0.0), $\chi^2(1, N = 202) = 7.18$, $p = .007$. Unexpectedly, when conditioning on only those who decided to visit the website, there was a nonsignificant difference in the medians in the opposite direction between those in the 1,000-people group (19.5) compared to those in the 1-person group (33.5), $\chi^2(1, N = 100) = 3.55$, $p = .06$.

Discussion

The data collected in Experiment 2 present further evidence that aggregating benefits of potential collective action—in this case, 1,000 people all giving to charity by answering questions at an ad-supported donation website—is more persuasive than is simply presenting the mathematically equivalent benefits for one individual. This experiment shows that the effect generalizes to a purely altruistic context. It is important to note that the initial evaluation of persuasiveness was not simply cheap talk but was carried forward to actual behavior: The odds of choosing to visit the charity donation website was 2.2 times higher when presented with the potential benefits aggregated over 1,000 people compared to just 1 person. This higher conversion rate resulted in overall more charity actions completed by those in the 1,000-people group than those in the 1-person group. We note that the median individual in the 1,000-people group tended to contribute less. This may be due to a selection effect: The marginal people induced by the collective aggregation statement to participate may have had lower motivation. Alternatively, those who chose to participate may have felt less responsibility to contribute based on the expected (larger) number of others who would also be contributing. In any case, the net effect on contributions was positive.

Experiment 3

We conducted Experiment 3 to provide an additional test of the collective aggregation effect (H1) and to test whether large de-

nominators alone induce greater motivation to contribute (H2). The literature on social norms argues that individuals use the behavior of others to infer what is normal or appropriate, and this guides their own decisions to match the norms (Cialdini, Reno, & Kallgren, 1990; Cialdini & Trost, 1998; Schultz, Nolan, Cialdini, Goldstein, & Griskevicius, 2007). For example, the statement "most people use cold water to wash their laundry" is a social norm that motivates by informing what most people do. The proposed collective aggregation effect is unique in that the information that it presents is hypothetical and draws attention to the potential impact (e.g., if many people use cold water to wash their laundry, then together they would save XX energy).

In this experiment, we manipulated both the number of people imagined to be engaged in an activity (e.g., 1 vs. 1,000) and the impact of that engagement. A social norms account might predict that simply picturing 1,000 people engaging in an activity is motivating because it makes the behavior seem like a norm. Under this argument, differences in the denominator are sufficient to produce differences in prosocial behavior. The collective aggregation argument is that the aggregated benefit reflected in the numerator changes motivation. To test whether the denominator alone is sufficient to cause the patterns observed in Experiments 1 and 2, we presented messages that either did or did not provide numerator information. If differences in the denominator alone do not produce differences in behavior, it suggests that the collective aggregation effect goes beyond social norms.

We also designed Experiment 3 to show the psychological primacy of the numerator over other relevant information, such as the ratio of benefits to the number of people involved (which can be thought of as an efficiency measure). Thus, when numerator information was presented, we also varied the rate of impact such that the ratio of the numerator to the denominator was much higher for those presented with the 1-person information than those presented with the 1,000-people information. By pitting a larger rate against a larger numerator, we could test for a form of (between-subjects) preference reversal: A large absolute number is more motivating than a superior rate is.

Method

Participants. The participants were 402 American respondents (M_{age} = 32.49, SD_{age} = 9.84; 225 female) recruited from AMT.

Procedure, materials, and design. The experiment, which took a median of 5.2 min to complete, used a procedure similar to the one employed in Experiment 1. This experiment used a 2 (number of persons: 1 vs. 1,000) × 2 (numerator: absent vs. present) between-subjects design. Following Experiment 1, the first independent variable was whether the potential benefits of prosocial action were aggregated over 1 person ($.28) or 1,000 people ($70.00). The second independent variable was whether the savings were stated as a quantified numerator (see Appendix B). Note that the rate of impact was 4 times higher for those in the 1-person group (e.g., save $.28/1 person) than for those in the 1,000-people group (save $70.00/1,000 people; that is, $.07/1 person). We used the same 10-point persuasiveness dependent variable as in Experiment 1.

Results

Persuasiveness. An ANOVA with persons and numerator entered as independent variables and persuasiveness entered as the dependent variable revealed a main effect of number of persons, $F(1, 398) = 8.69$, $p = .003$, $\omega^2 = .019$; no effect of the presence of a numerator, $F(1, 398) = 2.79$, $p = .10$, $\omega^2 = .004$; and an interaction between these two variables, $F(1, 398) = 4.95$, $p = .03$, $\omega^2 = .009$. Follow-up comparisons using Tukey's HSD supported H1 (see Figure 2): When the numerator was present, persuasiveness was significantly higher for the 1,000-people group than the 1-person group, $F(1, 398) = 13.46$, $p = .0002$, $d = .51$. In contrast, and supporting H2, when the numerator was absent, persuasiveness was not significantly different between the 1,000-people group and the 1-person group, $F(1, 398) = .26$, $p = .61$, $d = .07$.

Discussion

The data collected in Experiment 3 once again show that aggregating the benefits of potential collective action—in this case, 1,000 people all reducing the number of TV hours watched—is more persuasive than is simply presenting the equivalent benefits for one individual. However, as predicted, this effect was apparent only when numerator information was present. The effect also emerged even though the rate of impact was specified as higher for those in the 1-person group. These observations suggest that the collective aggregation effect is driven by changes in the numerator (i.e., the aggregated benefits) rather than the denominator (i.e., the number people) and that it occurs even when the ratio is unfavorable. These results show that increasing the imagined group size alone is not sufficient to change judgments, suggesting that the magnitude of the numerator—and not just social norms—underlie the effect of aggregation. This has an interesting implication for the larger literature on motivating prosocial behavior. One severe limitation of motiving action through descriptive norms is that the behavior that is desired may be uncommon. In such cases, a descriptive norm requires that a policymaker either lies (to inflate

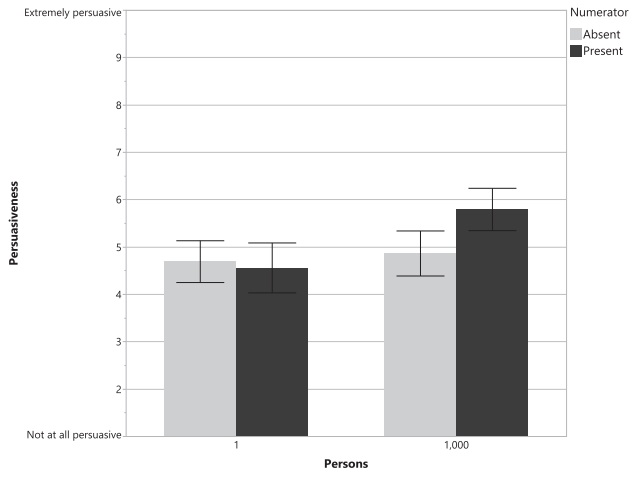

Figure 2. Average persuasiveness as a function of the number of people aggregated over and numerator status in Experiment 3. Error bars correspond to 95% confidence intervals.

the norm) or risks giving a low norm that is demotivating. An advantage of collective aggregation is that it does not rely on actual descriptions of behavior, because the actions and benefits are all hypothetical.

Experiment 4

We conducted Experiment 4 to test H1 and H3. The previous experiment, which implicated the numerator as central for producing the collective aggregation effect, could lead to the expectation that the type of denominator does not matter; any aggregation policy that produces a large numerator could be equally effective. However, we argue that the type of aggregation does matter. Specifically, we believe that aggregating over a collection of people has two effects that are central to producing the collective aggregation effect. First, it reduces the effect of discounting delayed benefits. Second, it evokes a sense of outcome efficacy. Experiment 4 tests the role of discounting; Experiments 5 and 6 test the role of outcome efficacy.

To begin testing these hypotheses, we carried out a factorial experiment in which half of the participants had potential outcomes aggregated over people (1 vs. 1,000) and half had potential outcomes aggregated over days (1 vs. 1,000). It is important to note that two of the resulting experimental cells—1 person for 1,000 days, and 1,000 people for 1 day—had identical total outcomes. This design allowed a direct comparison of whether aggregating over people was more persuasive and motivating than was aggregating over time and also allowed us to explore the interactive effects of combining multiple aggregation policies. We again predicted a main effect for the number of people being aggregated over. We additionally expected that responses would be higher for those presented with savings aggregated over 1 day and 1,000 people than 1,000 days and 1 person.

Experiment 4 also included measures of individual rates of time discounting. This allowed us to test whether time discounting moderated the relationship between aggregation over days and persuasiveness of the message. We predicted a two-way interaction between aggregation over days and individual discounting rates such that those who discount time highly would perceive aggregation over 1 day and 1,000 days as similarly persuasive, whereas those who discounted time less would find aggregation over 1,000 days more persuasive than over 1 day. In addition, we expected that within the two cells that yielded the same total savings (1 day and 1,000 people vs. 1,000 days and 1 person) there would also be an interaction between aggregation source (days vs. people) and individual time discounting, such that those low in discounting would find both paths to savings equally persuasive and those high in discounting would be more persuaded by people than by days.

A final benefit of this study was that it allowed us to address another explanation for the effect of aggregation: that large numerators produce an anchor that spills over to the response mode (Oppenheimer, LeBoeuf, & Brewer, 2008). According to this anchoring account, large aggregated outcomes influence the selective accessibility of a larger (vs. smaller) response (Mussweiler & Strack, 1999). Observation of a difference between aggregations over time versus people with identical numerators serving as an anchor would suggest that the effect reflects more than anchoring—that other processes, such as discounting and outcome efficacy, are also operating. In Experiment 4 the target action was reducing shower length.

Method

Participants. The participants were 207 American respondents recruited from AMT.

Design. The experiment used a 2 (number of persons: 1 vs. 1,000) × 2 (number of days: 1 vs. 1,000) between-subjects design. As in Experiments 2 and 3, the first independent variable was whether the potential savings of prosocial action were aggregated over 1 person or 1,000 people. The second independent variable was whether the savings were aggregated over 1 day or 1,000 days. Thus, participants were randomly allocated to one of four presentation formats where potential benefits were aggregated: (a) per 1 day per 1 person, (b) per 1 day per 1,000 people, (c) per 1,000 days per 1 person, or (d) per 1,000 days per 1,000 people. As in Experiments 1 and 3, the main dependent variable was persuasion.

Procedure and materials. The experiment took a median of 3.8 min to complete. After agreeing to complete the experiment, participants were directed to a page stating that taking long, hot showers were enjoyable but also consumed energy. The page concluded with the statement: "If 1 person [1,000 people] for 1 day [1,000 days] took a shower that was 1% shorter than average then, in total, they would: Use .04 [43.48] [[43,482.56]] KwH less energy; save $0.01 [$5.16] [[$5,156.98]]; prevent .07 [65.22] [[65,223.98]] lb of CO2 being released into the environment (equivalent to .003 [3.39] [[3,391.64]] gallons of gas)," where double-bracket amounts represent the effect of aggregating over 1,000 people and 1,000 days.

The participants were asked to indicate how persuasive the presented information was. A free-response text box allowed participants to expand upon their answer. On subsequent pages, the participants were asked questions about their actual shower behavior. Next, the participants completed an attention-check question. Finally, participants' answered some questions designed to reveal their discount rate—that is, their tendency to discount future costs and savings. Participants' discount rates were assessed via a monetary-choice questionnaire and scored using the procedure described by Kirby and colleagues (Kirby & Maraković, 1996; Kirby, Petry, & Bickel, 1999). Participants were required to choose between 27 hypothetical payment schedules offering a smaller, immediate reward (SIR) versus a larger, delayed reward (LDR). We calculated the discount rate, k, that would produce indifference between the options with the formula $k = [(\text{LDR}/\text{SIR}) - 1]/\text{Delay}$. By examining the point at which participants switched from preferring the SIR to the LDR across a number of choices where indifference points imply different discount rates, we estimated their implied discount rate. Note that a higher discount rate, k, was associated with greater discounting.

Results

Discount rate. Given the skew in the data, we analyzed the natural log-transformed implied psychological discount rate (henceforth, discount rate). To confirm that the discount rate was not influenced by the manipulations, we conducted an ANOVA with persons, days, and their interaction entered as independent variables and discount rate entered as the dependent variable. The analysis revealed no significant effects (all $ps > .05$).

Persuasiveness. A linear regression analysis was conducted with persons, days, discount rate, and their interactions entered as independent variables and persuasiveness entered as the dependent variable. Note that in all regressions we used effects coding (e.g., 1 person coded as −1; 1,000 people coded as +1), and discount rate was centered. The analysis revealed a significant main effect of persons ($b = .95$, $p < .0001$, $\omega^2 = .126$), a nonsignificant main effect of days ($b = .32$, $p = .06$, $\omega^2 = .011$), and no effect for discount rate ($b = .10$, $p = .30$, $\omega^2 < .001$). Thus, H1 was supported (see Figure 3): The average level of persuasiveness was higher for those in the 1,000-people group ($M = 6.89$, $SD = 2.14$) than for those in the 1-person group ($M = 5.05$, $SD = 2.66$). A follow-up contrast supported H3: Persuasiveness was higher for those in the 1-day 1,000-people group ($M = 6.65$, $SD = 1.92$) than for those in the 1,000-day 1-person group ($M = 5.43$, $SD = 2.44$), $F(1, 199) = 6.59$, $p = .01$, $d = .48$.

The regression analysis also revealed a nonsignificant interaction between days and discount rate ($b = .18$, $p = .07$, $\omega^2 = .010$) and no significant interaction between persons and days ($b = -.001$, $p = .99$, $\omega^2 < .001$) or between persons and discount rate ($b = .1$, $p = .89$, $\omega^2 < .001$) or the three-way interaction ($b = .02$, $p = .88$, $\omega^2 < .001$). When the same regression was conducted again with the 23 participants who failed the attention-check question removed, the days main effect was significant ($b = .40$, $p = .03$, $\omega^2 = .019$), and so too was the predicted interaction between days and discount rate ($b = .24$, $p = .02$, $\omega^2 = .019$). As can be seen in Figure 4, those who discounted time highly found aggregation over 1 day and 1,000 days to be similarly persuasive, whereas those who discounted time less found aggregation over 1,000 days more persuasive than over 1 day. A follow-up analysis examining the two cells that yielded the same total savings—1 day and 1,000 people versus 1,000 days and 1 person—provided additional insight. As displayed in Figure 5, participants low in discounting found aggregation over 1,000 days and 1,000 people to be equally persuasive; in contrast, participants high in discounting were more persuaded by aggregation over 1,000 people than over 1,000 days ($p = .047$).

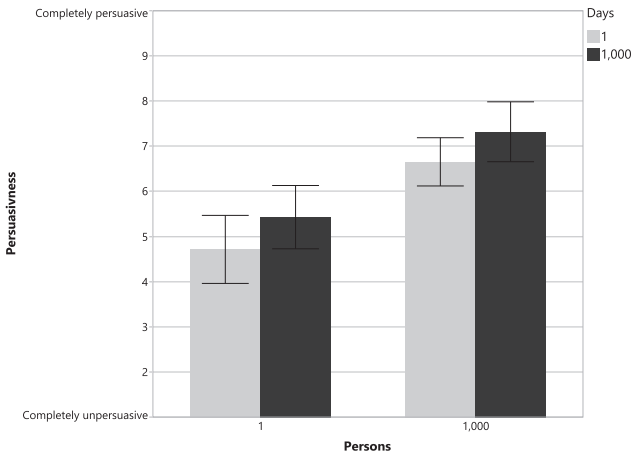

Figure 3. Average persuasiveness as a function of the number of people and number of days aggregated over in Experiment 4. Error bars correspond to 95% confidence intervals.

Discussion

The data collected in this experiment once again show that aggregating the benefits of potential collective action—in this case, 1,000 people all reducing the length of their showers—is more persuasive than simply presenting the equivalent benefits for one individual. The data also suggest that the type of aggregation policy used matters: Participants were more persuaded when the potential savings were aggregated for many people than for the equivalent savings aggregated for many days. This pattern suggests that more than anchoring is at work.

This experiment finds that one of the factors driving the difference between aggregation types is individual differences in individual's psychological discount rates. We observed that those who possessed a high discount rate—implying that they valued future benefits relatively less than others did—were relatively less persuaded by benefits described as being achieved only after much time had passed. In other words, as depicted in Figure 4, those who discounted heavily did not distinguish between 1-day and 1,000-day aggregation policies (averaged over people), whereas those who discounted little were more persuade by 1,000-day versus 1-day aggregation (when averaged over people). This observation lends support to the suggestion that one advantage of aggregating benefits over potential collective action is that the benefits can be expressed as occurring soon, which is particularly engaging for those with relatively high discount rates.

Experiment 5

We designed Experiment 5 to test H1 and H3, H4, and H5. We have argued that aggregating collective potential action increases outcome efficacy, which in turn boosts prosocial action. Experiment 5 sought to test the role of outcome efficacy by measuring it a mediator. In Experiment 5 the target action was to persuade people to unplug their mobile phone chargers when not using them.

Method

Participants. The participants were 345 American respondents ($M_{age} = 29.98$, $SD = 9.32$; 120 female) recruited from AMT.

Design. The experiment used the same 2 (number of persons: 1 vs. 1,000) × 2 (number of days: 1 vs. 1,000) between-subjects design as in Experiment 4. The dependent variable was the participants' intention to engage in the prosocial action, which was assessed via a 7-point scale from 1 (*Strongly prefer leaving phone charger plugged in all the time*) to 7 (*Strongly prefer unplugging phone charger when not using it*).

Procedure and materials. The experiment was conducted online and took a median of 7.3 min to complete. After agreeing to complete the experiment, participants were asked in separate questions to estimate the percentage of the time that they and others unplugged mobile phone chargers when not using them. Next, participants were directed to a page stating that a typical mobile phone charger consumed electrical energy when it was plugged into the wall socket even when no mobile phone was connected. The page included a table that summarized the potential electricity costs and carbon emissions from leaving the phone charger

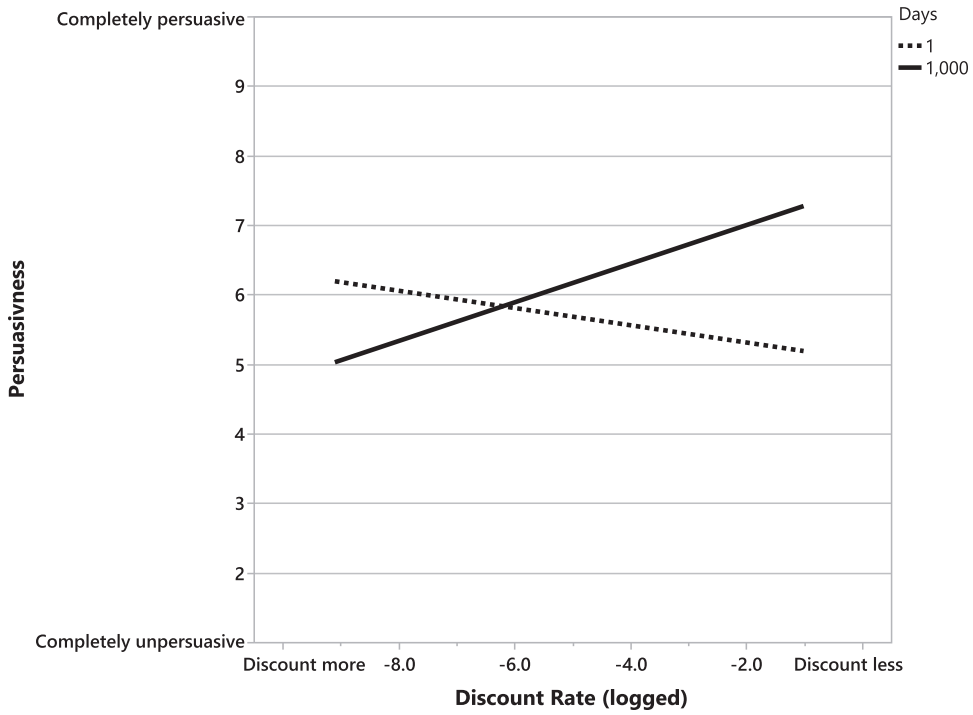

Figure 4. Average persuasiveness as a function of number of days and discount rate in Experiment 4.

plugged in all the time compared to unplugging the phone charger when not using it (see Appendix C). At the bottom of the page was the question measuring intention together with a free-response text box that allowed participants to expand upon their response.

We included four questions adapted from Koletsou and Mancy (2011) designed to measure the four types of efficacy. Individual ability efficacy was measured with "How able are you to unplug your charger when not using it in real life?" on a 6-point scale from 1 (*Completely unable*) to 6 (*Completely able*). Individual outcome efficacy was measured with "If you unplug your charger when not using it, how will this contribute meaningfully to reducing carbon emissions in real life?" on a 6-point scale from 1 (*No contribution*) to 6 (*Enormous contribution*). Collective ability efficacy was measured with "How able are others to unplug their charger when not using it in real life?" on a 6-point scale from 1 (*Completely unable*) to 6 (*Completely able*). Collective outcome efficacy was measured with "If others unplug their chargers when not using them, how will this contribute meaningfully to reducing carbon emissions in real life?" on a 6-point scale from 1 (*No contribution*) to 6 (*Enormous contribution*).

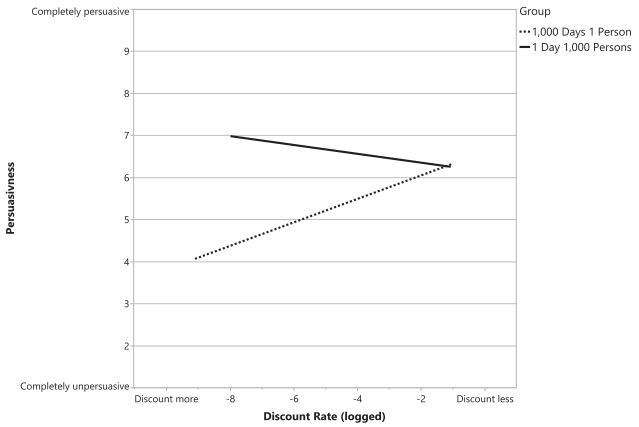

Figure 5. Average persuasiveness as a function of two groups and discount rate in Experiment 4.

Results

Intention. An ANOVA with persons and days entered as independent variables and intention entered as the dependent variable revealed a main effect of persons, $F(1, 341) = 12.46$, $p < .0001$, $\omega^2 = .032$; no effect of days, $F(1, 341) = 030$, $p = .58$, $\omega^2 < .001$; and no interaction between these two variables, $F(1, 341) = .61$, $p = .44$, $\omega^2 < .001$. Thus, H1 was supported (see Table 1): The average level of persuasiveness was higher for those in the 1,000-people group ($M = 5.80$, $SD = 1.53$) than for those in the 1-person group ($M = 5.15$, $SD = 1.88$). A follow-up contrast supported H3: Intention was higher for those in the 1-day 1,000-people group than for those in the 1,000-day 1-person group, $F(1, 341) = 4.45$, $p = .04$, $d = .32$.

Efficacy. The average responses to the four efficacy questions are shown in Table 1. We noticed a strong positive correlation between individual and collective outcome efficacy ($r = .68$, $p < .0001$). We therefore conducted an exploratory factor analysis using maximum likelihood factoring and a quartimin oblique rotation. Using a scree plot inspection, we extracted three factors that had item loadings of at least .71. The factors were individual ability efficacy, collective ability efficacy, and outcome efficacy.

Table 1
The Effect of Potential Collective Action on Efficacy in Experiment 5

Variable	1 person		1,000 people	
	1 day	1,000 days	1 day	1,000 days
N	86	89	84	86
Individual ability efficacy				
M	5.31	5.37	5.33	5.13
SD	1.24	1.12	1.27	1.49
Individual outcome efficacy				
M	2.35	2.25	2.42	2.83
SD	.84	.73	.87	1.11
Collective ability efficacy				
M	5.17	5.19	5.27	4.87
SD	1.21	.94	1.07	1.39
Collective outcome efficacy				
M	2.95	3.04	3.23	3.49
SD	.98	1.08	1.07	1.18
Intention				
M	5.02	5.27	5.82	5.78
SD	1.89	1.88	1.42	1.63

Thus, we proceeded with data analysis by combining the variables for individual and collective outcome efficacy.

An ANOVA (analyzing outcome efficacy) revealed a significant effect of persons, $F(1, 341) = 12.26$, $p = .0005$; a nonsignificant effect of days, $F(1, 341) = 2.89$, $p = .09$; and a nonsignificant interaction between these two variables, $F(1, 341) = 3.07$, $p = .08$. In sum, and supporting H4, aggregating over many persons compared to one person produced higher outcome efficacy. As expected, aggregating over many persons compared to one person had no effect on either individual or collective levels of ability efficacy (all ps $> .05$).

We used Hayes's (2017) PROCESS Model 4 to test mediation with persons as the independent variable, outcome efficacy as the mediator variable, and intention as the dependent variable. The analysis supported H5: Outcome efficacy was a significant mediator of the relationship between the number of persons and intention (Indirect Effect [*IE*] = .25, *SE* = .08, 95% confidence interval [CI: .11, .41]).[1] Thus, the collective aggregation manipulation increased preference for unplugging unused phone chargers through a process of increased perceived outcome efficacy.

Discussion

The data collected in Experiment 5 once again demonstrated that reading about the aggregated benefits of potential collective action—in this case, 1,000 people all unplugging unused mobile phone chargers for 1 day—increased the intention to act compared to the equivalent benefits for one individual. We also replicated the finding that people are sensitive not only to the size of the potential outcomes but also to the aggregation policy used to calculate those outcomes. Specifically, people expressed a stronger intention for prosocial action when the aggregation occurred over 1,000 people rather than 1,000 days.

In this experiment, we found that outcome efficacy was at least partially responsible for the collective aggregation effect. Specifically, people were more likely to feel that their actions and that the actions of others were going to be effective at achieving a collective goal—in this case, addressing the threat of climate change—when the potential savings were described as deriving from the effort of many hypothetical people compared to the effort of just one. It is interesting that although the literature tends to assume that they are conceptually distinct, the pattern of responses to the efficacy questions suggested that participants treated individual and collective outcome efficacy as one overall construct. We return to this point in the General Discussion.

Experiment 6

We conducted Experiment 6 to test H1, H3, and H5. Following the moderation-of-process design approach advocated by Spencer, Zanna, and Fong (2005) to examine psychological processes, in this experiment we sought to directly manipulate outcome efficacy. Antecedents of efficacy include mastery experiences, vicarious experiences, and verbal persuasion (Bandura, 1997). We attempted to manipulate outcome efficacy by presenting participants with a vicarious experience in the form of a news story reporting the outcome of a prosocial charity drive.

The logic of this study was to break the relationship between collective aggregation and outcome efficacy by artificially increasing outcome efficacy for one group via the new story. In other words, we expected to see the normal collective aggregation effect mediated by outcome efficacy when outcome efficacy was manipulated to be low (after reading about an unsuccessful charity drive). This pattern of results would be similar to that in Experiment 5. In contrast, we expected no collective aggregation effect, and thus no mediation, when outcome efficacy was manipulated to be high (after reading about a successful charity drive). This is because the primary benefit of collective aggregation—increasing outcome efficacy—would have no effect when outcome efficacy was already very high.

Method

Participants. The participants were 608 American respondents ($M_{age} = 32.82$, $SD = 10.76$; 362 female) recruited from AMT.

Design. The experiment used a 2 (number of persons: 1 vs. 1,000) × 2 (outcome efficacy: low vs. high) between-subjects design. The manipulation of the number of persons was identical to the one used in Experiment 2. Outcome efficacy was manipulated by presenting participants with a (fictional) news story describing a collective charity effort that was either successful or unsuccessful. The dependent variables used were the same as those used in Experiment 2: persuasiveness, the choice to contribute, and the number of actions.

Procedure and materials. The experiment was conducted online and took a median of 6.6 min to complete. The procedure was similar to that in Experiment 2 except for the following: First, prior to the choice part of the study, participants were exposed to

[1] We also tested a moderated mediation model according to Hayes's (2017) PROCESS Model 8 with persons, days, and their interaction as the independent variables; outcome efficacy as the mediator variable; and intention as the dependent variable. The analysis revealed significant mediation at the 1,000 days level (*IE* = .38, *SE* = .11, 95% confidence interval [CI: .16, .61]) and nonsignificant mediation at the 1 day level (*IE* = .13, *SE* = .10, 95% CI [−.06, .33]). The index of moderated mediation, which tests for a difference in mediation strength between levels of days, was nonsignificant (*IE* = .25, *SE* = .15, 95% CI [−.03, .55]).

an outcome efficacy manipulation. Specifically, participants were presented with one of two (fictional) newspaper articles describing the outcomes of a charity drive to raise money to fight hunger in the developing world (see Appendix D). The low-efficacy article, titled "Collective Charity Effort Fails to Feed an Entire Village," was designed to induce a sense of diminished outcome efficacy. In contrast, the high-efficacy article, titled "Collective Charity Effort Helps to Feed an Entire Village," was designed to induce a sense of increased outcome efficacy. Both articles featured identical content with the exception of keywords and phrases that were switched from positive to negative. The articles were identical on all other attributes (e.g., publication date, length, author, formatting). Second, after making a choice to visit the free-rice website, the participants were asked to complete the four efficacy questions. The questions were similar to those used in Experiment 5 with the target action modified (from "unplug your charger when not using it" to "answer vocabulary questions to donate rice") and the societal goal modified (from "reducing carbon emissions" to "help feed the hungry").

Results

Efficacy. The key data are summarized in Table 2. We again noticed a strong positive correlation between individual and collective outcome efficacy ($r = .78$, $p < .0001$). We therefore conducted an exploratory factor analysis using maximum likelihood factoring and a quartimin oblique rotation. Using a scree plot inspection, we extracted three factors that had item loadings of at least .73. The factors were individual ability efficacy, collective ability efficacy, and outcome efficacy. Thus, we proceeded with data analysis by combining the variables for individual and collective outcome efficacy.

Table 2
The Effect of Potential Collective Action on Efficacy, Persuasiveness, Choice, And Actions in Experiment 6

Variable	1 person		1,000 people	
	Low efficacy	High efficacy	Low efficacy	High efficacy
N	152	153	151	152
Individual ability efficacy				
M	8.93	8.90	9.28	8.84
SD	1.93	2.06	1.60	2.14
Individual outcome efficacy				
M	5.32	6.20	6.11	6.05
SD	2.55	2.42	2.48	2.55
Collective ability efficacy				
M	7.45	8.03	8.01	7.87
SD	2.25	2.13	2.23	2.13
Collective outcome efficacy				
M	5.94	6.61	6.81	6.77
SD	2.72	2.38	2.48	2.61
Persuasiveness				
M	6.11	7.10	7.68	7.76
SD	2.60	2.32	2.22	2.18
Choice (*M*)	.48	.52	.61	.62
Actions (*Mdn*)	0	0	10.0	10.0
Actions[a]				
M	.73	.77	.88	.85
SD	.81	.81	.78	.76

[a] All values were transformed with log(X + 1).

We conducted a series of ANOVAs with persons and efficacy entered as independent variables and each of the three efficacy measures entered as dependent variables. As expected, there was a significant interaction between efficacy and persons on outcome efficacy, $F(1, 604) = 4.53$, $p = .03$, $\omega^2 = .01$. Follow-up contrasts confirmed that outcome efficacy was significantly lower for those in the low-efficacy 1-person group compared to the three others groups (all $ps < .05$). This pattern of results is consistent with our expectations that outcome efficacy would be boosted in the three other groups by way of the high outcome efficacy news article or the 1,000-people aggregation. As expected, the efficacy manipulation had no effect on either individual or collective ability efficacy (all $ps > .05$).

Persuasiveness. An ANOVA with persons and efficacy entered as independent variables and persuasiveness entered as the dependent variable revealed a main effect of persons, $F(1, 604) = 34.62$, $p < .0001$, $\omega^2 = .051$; a main effect of efficacy, $F(1, 604) = 8.23$, $p = .004$, $\omega^2 = .011$; and an interaction between these two variables, $F(1, 604) = 5.79$, $p = .02$, $\omega^2 = .007$. Thus, H1 was supported (see Table 2): The average level of persuasiveness was higher for those in the 1,000-people group ($M = 7.72$, $SD = 2.20$) than for those in the 1-person group ($M = 6.61$, $SD = 2.51$). Simple contrasts revealed that, consistent with the outcome efficacy manipulation, persuasiveness was lower for those in the 1-person low-efficacy group compared to those in the other three groups (all $ps < .05$).

We conducted a moderated mediation analysis using Hayes's (2017) PROCESS Model 8 with persons, efficacy, and their interaction as predictor variables; outcome efficacy as the mediator variable; and persuasiveness as the predicted variable. Supporting H5, when outcome efficacy was low, there was a significant mediation effect between number of persons and persuasiveness via outcome efficacy ($IE = .41$, $SE = .15$, 95% CI [.14, .72]). In contrast, but as predicted, when outcome efficacy was high, there was no mediation effect between number of persons and persuasiveness via outcome efficacy ($IE = .001$, $SE = .13$, 95% CI [−.26, .25]). A significant index of moderated mediation ($IE = -.41$, $SE = .20$, 95% CI [−.82, −.04]) confirmed that the indirect effects were unequal between efficacy groups.

Choice. A logistical regression analysis with persons and efficacy entered as independent variables and choice entered as the dependent variable revealed a main effect of persons, $\chi^2(1, N = 608)$, $p = .004$; no effect of efficacy, $\chi^2(1, N = 608) = .31$, $p = .58$; and no interaction between these two variables, $\chi^2(1, N = 608) = .10$, $p = .75$. Thus, H1 was supported (see Table 2): The proportion choosing to visit the charity website was higher for those in the 1,000-people group ($M = .61$) than for those in the 1-person group ($M = .50$).

We ran a second moderated mediation analysis, this time with choice as the dependent variable. Supporting H5, when outcome efficacy was low, there was a significant mediation effect between number of persons and choice via outcome efficacy ($IE = .17$, $SE = .07$, 95% CI [.05, .31]). In contrast, but as predicted, when outcome efficacy was high, there was no mediation effect between number of persons and choice via outcome efficacy ($IE = .001$, $SE = .53$, 95% CI [−.10, .10]). A significant index of moderated mediation ($IE = -.16$, $SE = .09$, 95% CI [−.34, −.02]) confirmed that the indirect effects were unequal between efficacy groups.

Actions. Given the highly skewed nature of the actions data, we report medians and test for differences using Mood's median test. The median number of questions answered was nonsignificantly higher for those in the 1,000-people group compared to those in the 1-person group, $\chi^2(1, N = 608) = 2.69$, $p = .06$ (see Table 2). The median number of questions answered was not significantly different between those in the high-efficacy group and those in the low-efficacy group, $\chi^2(1, N = 608) = .01$, $p = .92$. It is interesting that when conditioning on only those who decided to visit the website, there was a nonsignificant difference in the opposite direction between those in the 1,000-people group (30) compared to those in the 1-person group (45), $\chi^2(1, N = 338) = 3.07$, $p = .08$.

We ran a third moderated mediation analysis, this time with number of actions as the dependent variable (after adding 1 to each value and then taking the log). Supporting H5, when outcome efficacy was low, there was a significant mediation effect between the number of persons and actions via outcome efficacy ($IE = .07$, $SE = .03$, 95% CI [.02, .12]). In contrast, but as predicted, when outcome efficacy was high, there was no mediation effect between number of persons and choice via outcome efficacy ($IE = .0002$, $SE = .02$, 95% CI [−.04, .04]). A significant index of moderated mediation ($IE = -.07$, $SE = .03$, 95% CI [−.14, −.005]) confirmed that the indirect effects were unequal between efficacy groups.

Discussion

The data collected in Experiment 6 replicated the collective aggregation effect. In this experiment, we additionally manipulated a hypothesized psychological mechanism for the effect: outcome efficacy. The purpose of this manipulation was to demonstrate conditions under which we turned on and off the relation between collective aggregation and responses via outcome efficacy.

As predicted, when participants were allocated to a group in which perceptions of outcome efficacy were manipulated to be low, showing the quantified benefits of collective aggregation increased perceptions of outcome efficacy. Consistent with Experiment 5, an increase in outcome efficacy mediated the effect of aggregation on downstream consequences—in this case, persuasiveness, choice, and actions. However, when participants were experimentally led to perceive a high level of outcome efficacy, showing the quantified benefits of collective aggregation had no additional impact on perceptions of outcome efficacy. Accordingly, we did not observe significant mediation when outcome efficacy was manipulated to be high. This moderation of mediation complements the results of Experiment 5 (Spencer et al., 2005). Under ordinary circumstances, collective aggregation boosts outcome efficacy above a low baseline level and motivates prosocial behavior. When the same mind-set is induced through a separate intervention, collective aggregation has no additional effect.

General Discussion

In this article, we have demonstrated a collective aggregation effect: People's inclination to engage in prosocial behavior is higher when the potential benefits of such actions are aggregated over many people. We showed this result across six experiments that included judgments of persuasiveness, stated intentions, and observations of real behavior. We observed the collective aggregation effect across a range of prosocial actions, some of which were purely charitable and some of which were associated with both personal and societal benefits.

Supporting H1, across all experiments, we observed that people were more persuaded and likely to engage in a prosocial action when the potential benefits of that action were aggregated over many people compared to just one. Experiment 1 suggests that the larger the aggregation, the larger the effect. It is important to note that this collective aggregation effect extends past scale-expansion effects involving aggregation over time and distance (Bagchi & Li, 2011; Burson et al., 2009; Camilleri & Larrick, 2014; Gourville, 1998; Pandelaere et al., 2011; Zhang & Schwarz, 2012) to a new unit of aggregation: people. Notably, although collective aggregation increased the tendency to engage in prosocial action, it did not influence the magnitude of the prosocial action. In fact, if anything, those moved to engage in prosocial action after being presented with the potential aggregated benefits contributed less on average. This pattern may be due to a selection effect: Aggregation induced those on the margin to participate in prosocial behavior. And the categorical decision to act or not may be more sensitive to framing than to the number of actions (Hsee & Rottenstreich, 2004). Nevertheless, fewer contributions from a larger number of contributors produced an overall larger contribution from those presented with aggregated benefits.

Supporting H2, Experiment 3 demonstrated that the numerator is crucial in generating the collective aggregation effect. Supporting H3, Experiments 4 and 5 demonstrated that the source of aggregation is also critical: Aggregating potential benefits over 1,000 people was more persuasive than aggregating over 1,000 days was. The results of Experiments 4, 5, and 6 helped to explain why. They revealed that avoiding psychological discounting and boosting outcome efficacy contribute to the collective aggregation effect (Bandura, 1997; Luszczynska & Schwarzer, 2005). Supporting H4, in Experiment 5 we found that people tend to perceive higher outcome efficacy when the potential benefits from action are aggregated over many people. Supporting H5, in Experiments 5 and 6, a mediation analysis revealed that larger reported potential benefits increase belief in the likelihood that one's actions and the actions of others can actually make a difference, which subsequently increases motivation to act.

Theoretical Implications

In the current article, we have shown that a statement of the form "If X people all do Y action, then Z outcomes will be achieved" induces a new form of numerosity (Pandelaere et al., 2011; Pelham et al., 1994) or scale expansion effect (Burson et al., 2009; Camilleri & Larrick, 2014). Collective aggregation offers new insights to the growing literature on numerosity. Larger numerators created by aggregating over many people increase the belief that individuals have the power to achieve a prosocial goal, and that belief helps to drive action. In Experiment 6, we were able to directly manipulate outcome efficacy through the provision of a vicarious experience in the form of a news story. This moderation evidence, combined with the mediation evidence collected in Experiment 5, provides con-

verging evidence for the role of outcome efficacy in driving the collective aggregation effect.

Research on motivation has often looked at efficacy at an individual level (Ajzen, 1991). However, in the context of many major social challenges, such as the reduction of greenhouse gas emissions, it is also important that one believe that one's group's actions can impact upon one's group's goals (Koletsou & Mancy, 2011). Currently, the literature demonstrating this important association is relatively sparse. This article contributes to the literature on collective efficacy by showing that, in situations where prosocial behaviors are optional and goals are not necessarily shared, a simple intervention can motivate individuals to address the social challenge.

One noteworthy result we observed was that individual and collective forms of outcome efficacy were strongly positively correlated. This correlation has been observed in previous research in the context of organization effectiveness (Riggs & Knight, 1994) and also proenvironmental intentions (Jugert et al., 2016). Although individual outcome efficacy and collective outcome efficacy are conceptually distinct, it is interesting that in some cases they move together. The correlation may stem from perceptions of high group efficacy's boosting individuals' perception of their own power to incrementally transform the situation (Jugert et al., 2016; van Zomeren, Saguy, & Schellhaas, 2013). For example, people may see their individual contribution as being more valuable in the context of a larger collective contribution: A drop in an *empty* bucket might be less valuable than is a drop that helps *fill* the bucket.

Our results also make a theoretical contribution to the growing literature on numerosity and scale expansion. Previous research has focused on the psychological impact of changes in the numerator for rescaled values. The current work reinforces the importance of numerators but also concludes that equal numerators are not psychologically equal. The aggregation policy matters. In Experiments 4 and 6 we found that aggregating over people, compared to days, is the more effective aggregation policy even though both approaches produce identical, albeit hypothetical, total savings. As described earlier, one reason for the difference is that collective aggregation triggers outcome efficacy. An additional individual difference factor that we found to contribute is psychological discounting; that is, the tendency for people to discount future benefits (Hardisty & Weber, 2009). A clear benefit of aggregating over people (vs. days) is that the total savings, despite being hypothetical, are all described as occurring today.

The negative influence of aggregation is obviously applicable to only situations in which the aggregation involves delayed benefits. There are ways to avoid marketing statements with delayed benefits, such as describing the potential aggregated benefits of many related actions by an individual. For example, rather than describing the potential benefits of unplugging an unused phone charger over many days, the potential benefits of unplugging all unused appliances in the household could be aggregated for a single day to make the potential outcome larger without any changes in the timing of those benefits.

The collective aggregation effect provides an interesting contrast to the literature on the identifiable victim effect (Small & Loewenstein, 2003). People are willing to expend more resources to save the life of an identified victim than to save the lives of a group of identifiable victims (Galak, Small, & Stephen, 2011) unless they are described as a single coherent unit (Smith, Faro, & Burson, 2013). This pattern has been explained in terms of psychological numbing—people are emotionally aroused by the plight of one victim but overwhelmed by the plight of many (Small, Loewenstein, & Slovic, 2007). In contrast, the collective aggregation effect shows that a larger number, such as a 1,000 people, is more motivating than is thinking about a single person. The difference is not hard to reconcile. Picturing a single individual is emotionally evocative; this is diluted by numbers. However, thinking about larger benefits (the numerator when aggregating over 1,000 people) increases desirability and worthiness of taking action because the effort seems worth it. Future research might explore how the collective aggregation effect could be enhanced when the context is made more psychologically close and emotional by, for example, describing the collective as a single coherent unit.

Practical Implications

The direct policy and marketing implication of our study is that governments and firms trying to promote prosocial actions could try to increase feelings of outcome efficacy. In this research, we have demonstrated that one way to do this is to express potential efficiency savings aggregated across people. This may be more effective than is simply aggregating across time, and certainly more effective than is not aggregating at all. We believe that collective aggregation represents a useful, alternative tool to a popular proven technique—the use of descriptive norms. A major shortcoming of descriptive norms is that they are difficult to use for uncommon behaviors, because policymakers must inflate their claims through fabrication or distortion to make the norm sound common. Collective aggregation can work well in these cases because it does not claim a norm—just a conditional outcome.

We have focused on encouraging prosocial behaviors. However, we see no reason why the findings observed here could not be imported into for-profit domains, as exemplified by the opening Walmart example. The practitioner may worry that the effect sizes in our experiments were often statistically small. Given the subtlety of the manipulation, small effects are unsurprising. Moreover, even small effects can be impressive when the manipulations that produced them are minimal (Prentice & Miller, 1992) and utilization of the collective aggregation effect is essentially a cost-free exercise.

More broadly, the collective aggregation effect represents a new type of choice architecture, which refers to an approach that uses behavioral insights to understand how different ways of presenting information can affect choice behavior (Sunstein, 2011; Thaler & Sunstein, 2008). Like many popular choice architecture tools—for example, defaults and the partitioning of options (see Camilleri & Larrick, 2015; Johnson et al., 2012, for reviews)—presenting potential benefits aggregated over many people is a simple, cost-effective solution.

Future Directions

The collective aggregation effect studied in this series of experiments aggregated over a very generic collective of "peo-

ple." However, as the opening examples highlight, there is great flexibility in the type of people that can be aggregated over. It is not much of a stretch to imagine that aggregating over people living in one's neighborhood or working in one's organization could introduce interesting complexities. Indeed, when considering the collective action of groups, it is often useful to delineate between "collectives" and "teams" (Koletsou & Mancy, 2011), which are distinguished by the level of interdependence among individuals and the extent to which goals are shared and members interact. The second feature of groups that maps well onto the team-collective dimension is *entitativity*, which refers to the extent to which a collection of individuals comprises a single coherent entity (Campbell, 1958). Factors that contribute to group entitativity include properties such as similarity, organization, interdependence, interaction, and common goals (Lickel et al., 2000). It would be interesting to learn how entitativity interacts with the collective aggregation effect. For example, greater entitativity may magnify the effect of collective aggregation on perceptions of outcome efficacy. People may want to live up to the perceived norms of the group to signal that they are good members, especially when their behavior is easily observed. These processes are likely to be self-fulfilling—when people expect others to cooperate, they are likely to act on this expectancy by reciprocating the expected behavior of others. The main danger of a highly specific group is that it may bring to bear practical real-world knowledge. For example, "1,000 professors in your university" may bring to mind actual tendencies (and shortcomings) associated with the group that would reduce expectancies about others and motivation for the self.

A second area of future study is identifying the optimal aggregation unit. We observed that 1,000 people seemed to be an optimal denominator, but a number of factors could influence this result. First, very large denominators may stretch credibility—at some point they raise the question, is it really plausible that 200 million people would undertake this action? Second, our core argument is that scale expansion actually influences judgment by changing the magnitude of the numerator. The magnitude of the numerator depends not just on the denominator but on the size of the benefits and the units chosen to express the benefit (e.g., grams, kilograms, metric tons). It is likely that very large numerators will have diminishing impact, both because of the psychophysics of interpreting large numbers and because people might avoid processing difficult, large numbers.

Conclusion

The central conclusion of our work is that people are more inclined to engage in prosocial behavior that might otherwise appear to be inconsequential—actions such as switching off a light bulb to reduce one's impact on climate change—by presenting efficiency savings in terms of potential benefits when aggregated over a large number of people. Such collective aggregation can transform demotivating drop-in-the-bucket perceptions by making individual actions seem bucket-sized, immediate, and important and thereby boosting belief in the effectiveness of many buckets.

Context of the Research

This work builds on our previous research reported in the 2014 article "Metric and Scale Design as Choice Architecture Tools," which was published in the *Journal of Public Policy and Marketing* (Camilleri & Larrick, 2014). In that article, we observed an increased preference for proenvironmental car options by aggregating fuel consumption and fuel costs over time. In conducting that research, we suspected that it might be even more effective to aggregate over social units. The current article emerged from that basic hunch.

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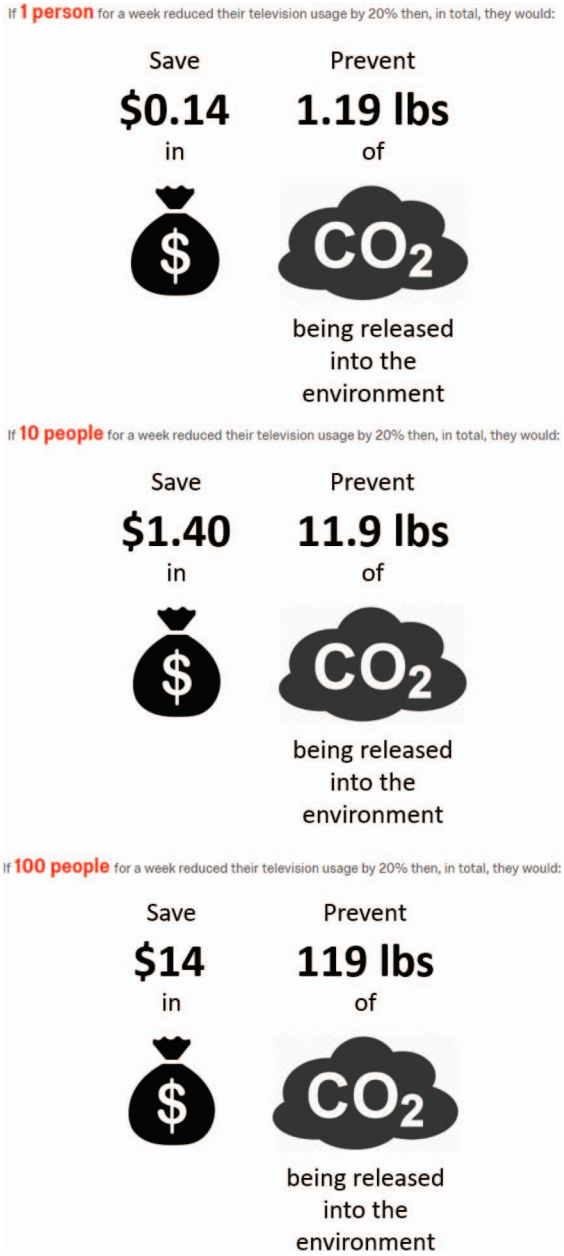
(*Appendices follow*)

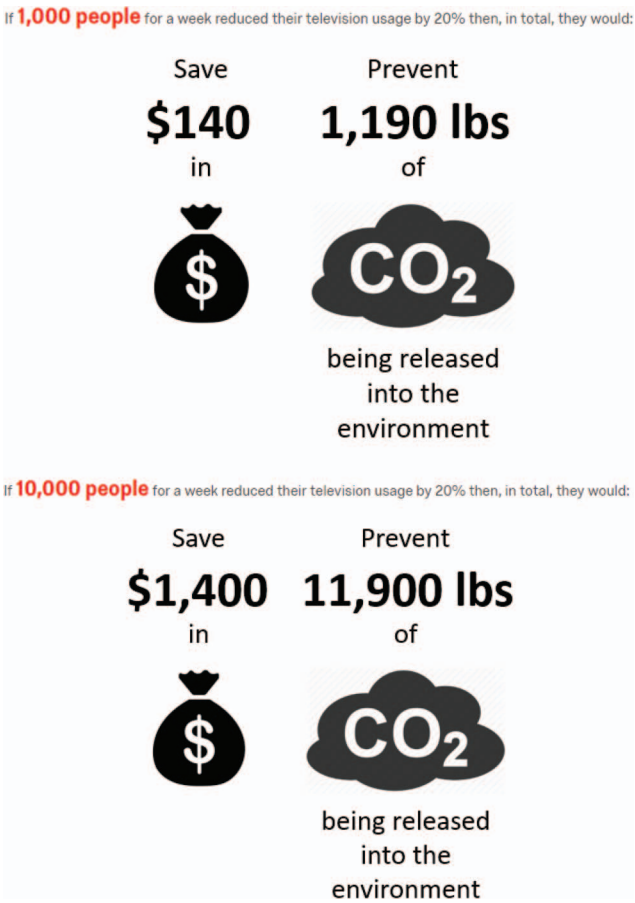
Appendix A

Stimuli Used in Experiment 1



See the online article for the color version of this figure.

(*Appendices continue*)

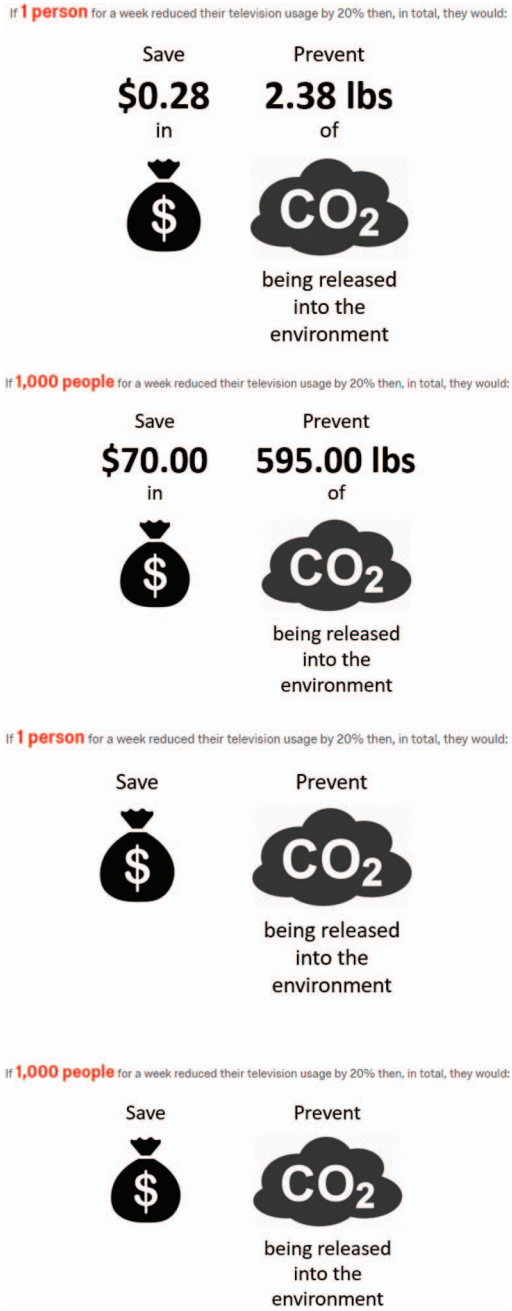


See the online article for the color version of this figure.

(*Appendices continue*)

Appendix B

Stimuli Used in Experiment 3



See the online article for the color version of this figure.

(*Appendices continue*)

Appendix C

Example of the Stimuli Used in Experiment 5

Variable	Potential electricity cost[a] per 1 day per 1,000 people	Potential carbon emissions[b] per 1 day per 1,000 people
Unplug phone charger when not using it	$1.18	13.25 lb (6,010.10 g) CO_2 (equivalent to .87 gal [3.29 L] of gas)
Leave phone charger plugged in all the time	$2.83	31.68 lb (14,369.81 g) CO_2 (equivalent to 2.09 gal [7.91 L] of gas)

[a] Assuming: 3.68 watts and 3 hours per day for a charging phone, 2.24 watts and 5 hours per day used for a charged phone still plugged in, and 0.26 watts and 16 hours per day for a plugged in charger; 10.71 cents per kWh of electricity (US average). [b] Assuming: 1.2 lbs of CO2 emitted per kWh of electricity produced (US average); 0.079 gallons of gas equivalent for per eachkWh of electricity produced.

Appendix D

The Text Used to Manipulate Collective Outcome Efficacy Stimuli

Those in the high collective efficacy group were presented with the first half of text in each of the square brackets, whereas those in the low collective efficacy group were presented with the second half of text in each of the square brackets. The articles were also accompanied by an image. For those in the high collective efficacy group, the image showed happy, cheering runners, whereas for those in the low efficacy group, the image showed the legs of many runners.

Collective Charity Effort [Helps/Fails] to Feed an Entire Village

By Ryan Miller, published on 11th January 2016

Those skeptical of collective charity efforts to make an impact on people in need have been [refuted/supported]. The sentiment emerged after the [success/lack of success] of a charity run held in Victorville, CA last month.

The Victorville-based charity "Food for Africa" received small donations from approximately 1,000 citizens over the month of December. The amount raised was enough to feed [the whole/just two families from the whole] village of Amuria in Uganda, one of that nation's poorest villages, for up to 3 [months/weeks]. This result was well [above/below] the target goal.

Mark Chapman, the CEO of the charity, commented [optimistically/pessimistically], "A group of people this large all contributing in the fight against hunger is [more than enough/not nearly enough] to make a big difference. Next year we hope to see more people."

The [inspiring/disappointing] example of the charity run caught the attention of the Executive Director of the UN World Food Program over the weekend who [praised/criticized] the effort, commenting, "These citizens have shown that, [fortunately/unfortunately], people [can/cannot] always have a significant effect when they join together and collectively donate to achieve a common goal."

Received March 17, 2017
Revision received December 4, 2018
Accepted December 6, 2018 ■