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People Are Slow to Adapt to the Warm Glow of Giving

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Abstract

People adapt to repeated getting. The happiness we feel from eating the same food, from earning the same income, and from many other experiences quickly decreases as repeated exposure to an identical source of happiness increases. In two preregistered experiments ($N = 615$), we examined whether people also adapt to repeated giving—the happiness we feel from helping other people rather than ourselves. In Experiment 1, participants spent a windfall for 5 days ($5.00 per day on the same item) on themselves or another person (the same one each day). In Experiment 2, participants won money in 10 rounds of a game ($0.05 per round) for themselves or a charity of their choice (the same one each round). Although getting elicited standard adaptation (happiness significantly declined), giving did not grow old (happiness did not significantly decline; Experiment 1) and grew old more slowly than equivalent getting (happiness declined at about half the rate; Experiment 2). Past research suggests that people are inevitably quick to adapt in the absence of change. These findings suggest otherwise: The happiness we get from giving appears to sustain itself.

Keywords

happiness, change, prosocial behavior, hedonic adaptation, open data, open materials, preregistered

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Many things make people happy, from eating foods to cashing checks. Alas, these sources are united by a sadder fate: The more we experience some dosage of pleasure, the less pleasurable the dosage. Hedonic adaptation is ubiquitous (Campbell, O'Brien, Van Boven, Schwarz, & Ubel, 2014; Frederick & Loewenstein, 1999; Galak & Redden, 2018; Lyubomirsky, 2010; Myers, 1992).

Hedonic adaptation is also functional. Without it, people could not discriminate what things are most advantageous to pursue, disrupting one's ability to notice and address other important goals (Lyubomirsky, 2010). Traditional strategies for combating hedonic adaptation therefore recommend making situational changes that manufacture ways to capture our attention, such as taking breaks between consumption episodes (Quoidbach & Dunn, 2013), using new methods of consumption (O'Brien & Smith, 2019), and consuming new entities altogether (Sheldon, Boehm, & Lyubomirsky, 2013). A long history of research on moderators of adaptation has taken this approach, from testing how the frequency of exposure affects the intensity of a light, noise, or arousing image (Thompson & Spencer, 1966) to how new environments affect the potency of a drug (Poulos & Cappell, 1991).

Underlying this approach is an assumption that the thing itself is rather hopeless on its own: Repetition will quickly dull our reactions unless we change the way in which an entity is experienced (the *how*). Less research, on happiness and hedonic outcomes in particular, has systematically compared adaptation rates between similar kinds of things (the *what*), holding exposure constant. In one study, participants viewed images that were complex (optical illusions) or simple (plain drawings) and rated their interest across exposure. Interest remained higher for complex images (Bornstein, Kale, & Cornell, 1990; see also Berlyne, 1970). In another study, participants enjoyed an experiential

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reward (e.g., watched a video) or received a material reward (e.g., a pencil). Over 2 weeks, they rated how happy they felt about their reward. Happiness remained higher for experiential rewards (Nicolao, Irwin, & Goodman, 2009). In a third study, participants uploaded an image with high sentimental value (e.g., one's wedding venue) and rated how happy it made them feel after viewing it six times. For comparison, that same image was shown to naive control participants who attached no sentimental value to it. Happiness remained higher when the image had sentimental value (Yang & Galak, 2015). These examples hint that people might be able to fend off hedonic adaptation by strategically consuming certain things, in addition to employing the traditional strategy of changing how things are consumed. However, more research is needed to better isolate repeated exposure to similar stimuli with equally high value (but different kinds of high value) at Time 1.

What broader kinds of things might matter? Past research on hedonic adaptation has almost exclusively assessed the happiness people get from getting: eating food, earning money, and so on. However, people also derive happiness from giving: helping other people rather than themselves (the "warm glow"; Andreoni, 1990). In one study, participants reported greater happiness after spending a one-shot windfall on other people as opposed to themselves (Dunn, Aknin, & Norton, 2008). Other studies have found that the neural activity mediating self-reported happiness from self-oriented behavior such as earning money is also elicited by prosocial behavior such as donating to charity, suggesting a common hedonic basis (Harbaugh, Mayr, & Burghart, 2007; Moll et al., 2006). In the present research, we hypothesized that the happiness that people get from giving may be one such thing that is relatively resistant to adaptation on its own, without changing content or context from exposure to exposure.

Why? First, economists extensively document people's tendency to engage in prosocial behavior even when it is not normatively expected (e.g., people still donate when recipients cannot reciprocate or after a third party has fixed the problem; Camerer, 2003; Charness & Rabin, 2002; Gintis, Smith, & Bowles, 2001). Economists interpret these findings to mean that the warm glow is uniquely outcome independent; people derive happiness from giving itself. Imas (2014) directly tested this idea: Participants worked harder to earn large versus small monetary sums when working for themselves (they were sensitive to outcome), but they worked equally hard regardless of payout when working for charity (they were sensitive to act; see also Imas & Loewenstein, 2018). In turn, hedonic adaptation may be slower when people focus on acts rather than outcomes because repeated outcomes are easy to compare and integrate (thereby triggering diminishing sensitivity; Frederick & Loewenstein, 1999; Tennant & Hsee, 2017; Wilson, Centerbar, Kermer, & Gilbert, 2005), likely more so than repeated acts. The less people focus on the literal value of $5.00 each time they win $5.00—as people may naturally do when winning for others—the more each win may be experienced as its own happy event, resulting in longer lasting happiness to winning repeatedly (despite no objective change).

Second, it is further informative to consider why people are so sensitive to prosocial acts to begin with and how this might bear on the functional nature of hedonic adaptation. As social beings, people have a need to belong (Baumeister & Leary, 1995) and therefore reap many benefits from signaling to themselves (and to other people) that they are helpful, contributing members of the community (Bodner & Prelec, 2003; Gintis et al., 2001; Grant & Dutton, 2012; Harbaugh, 1998; Inagaki & Orehek, 2017; Leary, Tambor, Terdal, & Downs, 1995; Milinski, Semmann, & Krambeck, 2002). Unlike many other needs, however, social needs are especially precarious: They can be unsatisfied in an instant. Interpersonal standings take time to build and require constant maintenance to uphold (Baumeister & Leary, 1995; Leary et al., 1995); it takes many prosocial acts to establish a prosocial image, whereas prosocial self-views and reputations can be undermined by just a hint of evidence to the contrary (Harbaugh, 1998; Klein & O'Brien, 2016, 2017; Milinski et al., 2002; Nowak & Sigmund, 1998; O'Brien & Klein, 2017; Reeder & Coovert, 1986). Because hedonic adaptation is functional—keeping us alert to relevant targets of attention—this precarious quality may bear on the rate at which the warm glow declines. After eating the same good food 5 days in a row, a sharp decline in happiness may serve to reorient us to more relevant eating-related goals (i.e., keeping a varied diet); after helping the same good cause 5 days in a row, hedonic adaptation may be slower to kick in because each exposure serves to maintain our prosocial standing (i.e., keeping us oriented to that same target). In short, the happiness people get from giving may be relatively immune to adaptation on its own because repeatedly giving the same thing to the same target likely draws happiness from the act itself and what each act builds over time. Giving may be slow to grow old.

Experiment 1: Repeated Getting Versus Repeated Giving

Method

Participants were randomly assigned to treat themselves or another person (the same other each day) to the same

small gift each day for 5 days in a row. We hypothesized that happiness would drop less steeply in the giving condition than in the getting condition, even though each participant received and spent the same daily windfall in the same way each day.

We report all measures, manipulations, and exclusions. All data, materials, and preregistration files can be found at https://osf.io/njea2/. Sample size was predetermined by a rule of thumb (and minding financial considerations) to attain around 50 participants per cell. We rounded up and sampled to the end of the week that we hit this number, anticipating some attrition.

Participants. We recruited 113 participants from our campus laboratory (age: $M = 26.15$ years, $SD = 11.06$; 41.59% female; 35.40% Caucasian American/White, 20.35% Asian American/Asian, 25.66% African American/Black, 7.96% mixed ethnicity, 4.42% other ethnicity) to complete the experiment for a set payment of $10.00 (plus the experiment's spending money). Our pool drew from across the university community (61.95% of our sample were students). To reduce selection concerns, we advertised the experiment as the "Everyday Experiences Study" with no mention of money, giving, happiness, and so on.

Procedure. We modeled our experiment on Study 3 by Dunn et al. (2008), adding the critical repetition component plus more thorough measures and controls. Our design for this and Experiment 2 also contributes to studies on general well-being over time. For example, Nelson, Layous, Cole, and Lyubomirsky (2016) assigned participants to commit acts of kindness toward other people or themselves and tracked changes in mental health over 4 weeks. However, the study was openly advertised as being about "happiness-enhancing" activities, there were no resources provided to equalize and enable the activities, and by design there were no repetition restrictions (i.e., participants could do different things, in different ways, at different times). Our designs afforded high experimental control, uniquely highlighting the repetition component to test rates of adaptation to a specified experience.

Participants came into the laboratory, where a research assistant explained all procedures and handed off all supplies in private individual sessions. First, each participant provided an e-mail address and cell phone number and received a unique ID code that allowed us to link the participant's responses. Next, each participant received $25.00, divided into five envelopes, each containing a $5.00 bill and labeled with a day number and the ID code. Participants were informed that their task was to spend the money each day and complete a survey each evening. The target of the spending, however, varied by condition.

Each participant was randomly assigned to one of two conditions. Participants in the getting condition ($n = 54$) were instructed to treat themselves to a small $5.00 gift each day (starting that day), for 5 days in a row, at their own leisure as they went about daily life. Their rules were to spend the money in piecemeal fashion (no aggregating), directly on themselves and themselves alone, and that whatever they chose for the first day had to be repeated exactly for all days (e.g., buying the same drink from the same café around the same time, depositing the money into their bank account at the same branch around the same time, and so on). Participants in the giving condition ($n = 59$) were instructed to treat other people to a small $5.00 gift each day (starting that day), for 5 days in a row, at their own leisure as they went about daily life. Their rules were to spend the money in piecemeal fashion (no aggregating), directly on other people, and that whatever they chose for the first day had to be repeated exactly for all days (e.g., dropping the money in the same tip jar at the same café around the same time, donating the money to the same online charity in the same way around the same time, and so on). Otherwise, all prompts and procedures were identical. A research assistant verbally explained all instructions and encouraged participants to sign up at a later date if they did not think they could complete all tasks.

Participants then exited the laboratory with their study materials and the experiment began. Each evening at 5:00 p.m., we sent each participant a personalized e-mail containing a link to that evening's survey, along with a personalized text-message reminder to complete the survey as soon as possible before bed. Participants had to enter their ID code to access each survey. Access expired at 5:00 a.m.

Key dependent measures: hedonic adaptation. In the nightly survey, participants began by reporting what they spent the money on and how much it cost, who they spent the money on (giving condition only), where they spent the money, and at what time they spent the money. In the Day 1 survey, these items were presented as fillable text boxes; in the surveys on Days 2 through 5, participants reported whether each original response was indeed repeated the same exact way as on Day 1 (forced choice: *yes, no [please explain]*).

Participants then completed our dependent variables, divided into three blocks. First, participants were asked, "Overall, how did you feel today?" and rated four items—"I felt good," "I felt happy," "I felt satisfied," and "I was in a positive mood"—on a scale from 1 (*not at all*) to 7 (*extremely*). This was our day's-end happiness block. Next, participants were asked, "Right after your study experience, how did it feel?" and rated four items—"It

made me feel good," "It made me feel happy," "It made me feel satisfied," and "It put me in a positive mood"—on a scale from 1 (*not at all*) to 7 (*extremely*). This was our recalled-happiness block. Third, participants were asked, "As you're moving along in the study, how are you now feeling about it at this point?" and rated three items—"It's boring" (low anchor) to "It's exciting" (high anchor), "It's unenjoyable" (low anchor) to "It's enjoyable" (high anchor), and "It's not rewarding" (low anchor) to "It's rewarding" (high anchor)—each on unnumbered 7-point scales. This was our study-happiness block.

We included a variety of blocks to tap into more general assessments of hedonic adaptation, although we suspected any effects to be strongest for the day's-end happiness block because the study we modeled (Dunn et al., 2008, Study 3) assessed day's-end happiness. Therefore, we presented the blocks in this order on separate pages. Each nightly survey ended with a fillable text box inviting participants to report anything else on their minds.

Other measures: exit survey. After completing all 5 days of the experiment, participants returned to the laboratory to pick up their payment. Then they were asked to complete an exit survey about their overall experience. In the variety block, they were asked, "How different were each of the daily experiences from each other?" and "How much did you build variety into the daily experiences?" In the specialness block, they were asked, "How much did you like [do you think the recipient liked] the kinds of gifts you got?" "How special were the kinds of gifts you got?" "How much time, thought, etc. did you put into figuring out the gifts?" and "How much of an impact did these gifts have on your life [do you think they had on the recipient's life] last week?" And in the task block, they were asked, "How difficult did you find this study?" "How awkward did you find this study?" "How confusing did you find your study instructions?" "How common is it for you to do things like this in your daily life?" and "How 'big' of a gift is $5.00 for you to spend like this?" Each item was rated on a scale from 1 (*not at all*) to 7 (*extremely*). Blocks were presented in this order on separate pages. These items assessed potential differences beyond the self/other component, per se, that might affect adaptation.

Participants rated the extent to which they followed instructions (forced choice: *not at all, a little, mostly, exactly*), reported demographic information, and could report anything else on their minds via a fillable text box.

Results

Retention was high and did not vary by condition. For the getting condition, we dropped 3 participants for failing to get the same thing in the same way each day and 5 participants who failed to complete all nightly surveys (get retention: 85.19%, or 46 of 54). For the giving condition, we dropped 6 participants for failing to give the same thing in the same way each day and 3 participants who failed to complete all nightly surveys (give retention: 84.75%, or 50 of 59). For our analyses, this left a final sample size of 96 (4 of whom did not complete the exit survey). Our data file retains all 113 participants.

Key analyses: hedonic adaptation. We collapsed the day's-end happiness items (each day, αs $\geq$ .95), recalled-happiness items (each day, αs $\geq$ .96), and study-happiness items (each day, αs $\geq$ .85) into scales, as per our preregistered intention. The three scales turned out to be highly correlated themselves (each day, αs $\geq$ .72); therefore, for ease of interpretation, we also collapsed them into an overall-happiness index. We did not include the possibility that the scales may be highly correlated in our preregistration, so this overall analysis should be seen as exploratory.

For the best controlled test, we analyzed the data in the context of growth-curve modeling, which tests for changes in happiness within each condition accounting for the nonindependence (nesting) of time points within individuals. We specified a multilevel random coefficient model using the SPSS mixed command. Condition, the linear effect of time, and the interaction between condition and time were entered as predictors of happiness, which varied over time. For reference, we also report Time 1 versus Time 5 paired-samples *t* tests within each condition. In addition, although we preregistered our intention to analyze the data via a repeated measures general linear model (see https://osf.io/njea2/), we thereafter learned that growth-curve modeling affords a more appropriate, more controlled test for our designs (results are unchanged using the general linear model). In Experiment 2, we preregistered our intention to use growth-curve analyses.

First, we report the exploratory analyses for overall happiness (the most general, highest powered test), followed by the preregistered analyses for each individual block.

Overall happiness (exploratory). We observed a main effect of time, $F(1, 474.64) = 15.42$, $p < .001$, and a marginal main effect of condition, $F(1, 105.57) = 3.23$, $p = .075$. These effects were qualified by the hypothesized interaction, $F(1, 474.64) = 5.78$, $p = .017$ (see Fig. 1a; descriptive statistics are shown in Table 1).

Participants in the getting condition experienced standard adaptation; specifically, getting the same thing over and over again significantly grew old (change from Day 1 to Day 5: $M = -0.86$, $SD = 1.35$)—model effects: $b = -0.22$, $SE = 0.05$, 95% confidence interval (CI) =

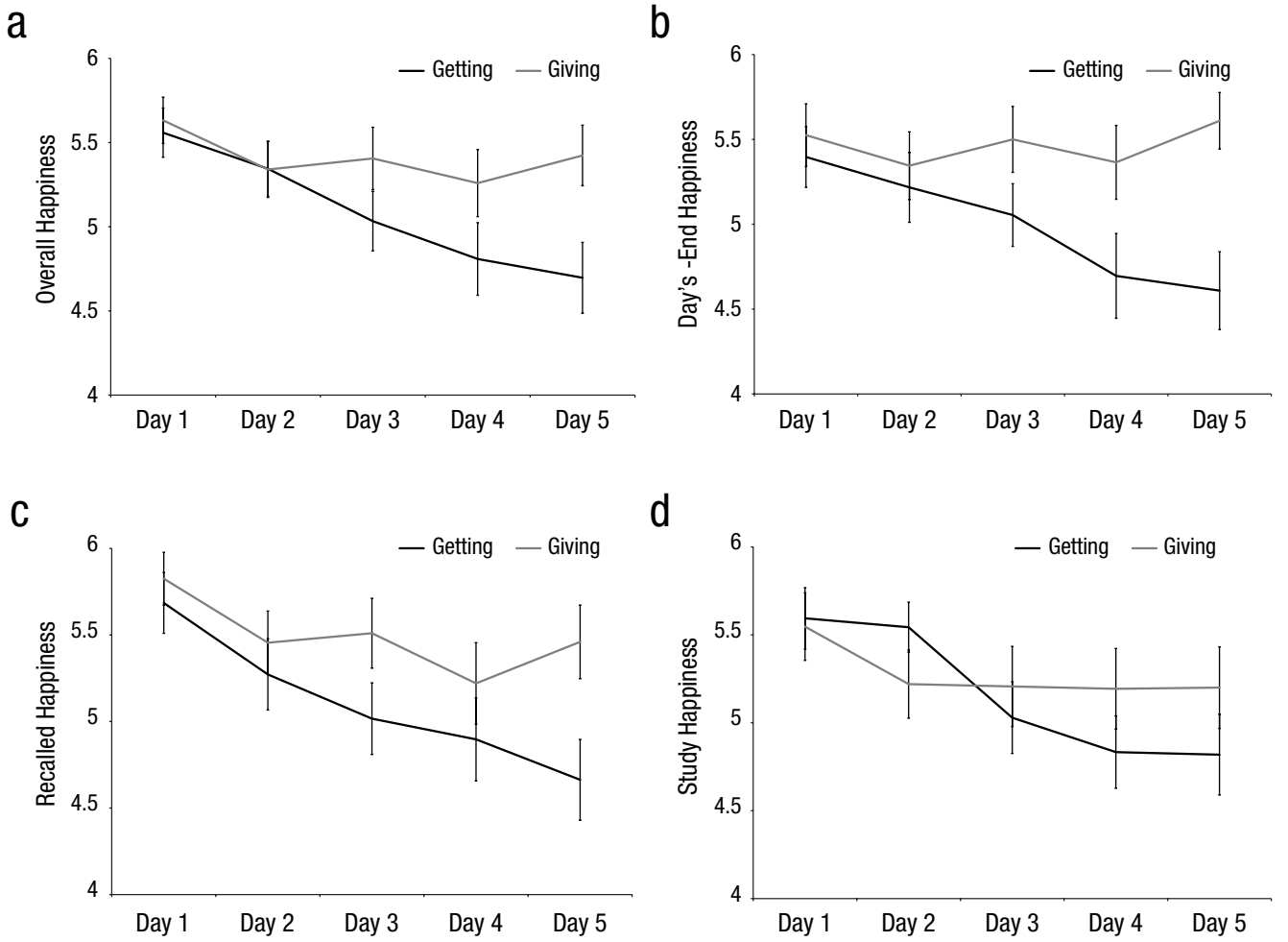

Fig. 1. Results from Experiment 1: mean (a) overall happiness, (b) day's-end happiness, (c) recalled happiness, and (d) study happiness. Results are shown separately for the giving and getting conditions in each day. Error bars indicate ±1 *SE*.

[−0.31, −0.12]; $t(227.55) = -4.35$, $p < .001$; paired-samples t test Day 1 versus Day 5: $t(45) = 4.32$, $p < .001$, $d = 0.66$, 95% CI for the mean difference = [0.46, 1.26]. However, participants in the giving condition did not adapt: Giving the same thing over and over again did not significantly grow old (change from Day 1 to Day 5: $M = -0.21$, $SD = 0.91$)—model effects: $b = -0.05$, $SE = 0.05$, 95% CI = [−0.14, 0.04]; $t(247.06) = -1.11$, $p = .269$; paired-samples t test Day 1 versus Day 5: $t(49) = 1.63$, $p = .110$, $d = 0.24$, 95% CI for the mean difference = [−0.05, 0.47].

Individual blocks (preregistered). This same asymmetric pattern held for each individual block, with varying effect sizes. For happiness at day's end, we observed a marginal main effect of time, $F(1, 420.73) = 3.65$, $p = .057$, a main effect of condition, $F(1, 123.97) = 6.73$, $p = .011$, and the critical interaction, $F(1, 420.73) = 5.50$, $p = .019$ (see Fig. 1b and Table 1). Again, whereas participants in the getting condition experienced standard adaptation (change from Day 1 to Day 5: $M = -0.79$, $SD = 1.36$)—model effects: $b = -0.20$, $SE = 0.07$, 95% CI = [−0.33, −0.07]; $t(208.23) = -2.93$, $p = .004$; paired-samples t test Day 1 versus Day 5: $t(45) = 3.94$, $p < .001$, $d = 0.59$, 95% CI for the mean difference = [0.39, 1.19]—participants in the giving condition did not significantly adapt (change from Day 1 to Day 5: $M = 0.09$, $SD = 1.45$)—model effects: $b = 0.02$, $SE = 0.06$, 95% CI = [−0.11, 0.15]; $t(213.04) = 0.32$, $p = .753$; paired-samples t test Day 1 versus Day 5: $t(49) = -0.42$, $p = .679$, $d = 0.06$, 95% CI for the mean difference = [−0.50, 0.33].

For recalled happiness right after the event, we observed a main effect of time, $F(1, 467.69) = 16.88$, $p < .001$, a marginal main effect of condition, $F(1, 108.81) = 3.57$, $p = .062$, and a marginal interaction, $F(1, 467.69) = 3.70$, $p = .055$ (see Fig. 1c and Table 1). However, the patterns of results were the same as they were for day's-end happiness: Participants in the getting condition experienced quicker adaptation (change from

Table 1. Descriptive Statistics for Experiment 1

Item and condition	Day 1	Day 2	Day 3	Day 4	Day 5
Overall happiness					
Getting	5.56 (0.99)	5.34 (1.11)	5.03 (1.20)	4.81 (1.46)	4.70 (1.43)
Giving	5.63 (0.97)	5.34 (1.17)	5.41 (1.31)	5.26 (1.41)	5.42 (1.27)
Day's-end happiness					
Getting	5.40 (1.22)	5.22 (1.40)	5.05 (1.25)	4.70 (1.70)	4.61 (1.56)
Giving	5.53 (1.31)	5.35 (1.41)	5.50 (1.37)	5.37 (1.54)	5.61 (1.18)
Recalled happiness					
Getting	5.68 (1.19)	5.27 (1.39)	5.02 (1.40)	4.90 (1.63)	4.66 (1.58)
Giving	5.83 (1.08)	5.46 (1.30)	5.51 (1.43)	5.22 (1.66)	5.46 (1.50)
Study happiness					
Getting	5.59 (1.18)	5.54 (0.97)	5.03 (1.38)	4.83 (1.40)	4.82 (1.56)
Giving	5.55 (1.36)	5.22 (1.37)	5.21 (1.61)	5.19 (1.62)	5.20 (1.64)

Note: Values are mean ratings, with standard deviations in parentheses.

Day 1 to Day 5: $M = -1.02$, $SD = 1.62$)—model effects: $b = -0.25$, $SE = 0.06$, 95% CI = [−0.38, −0.13]; $t(222.94) = -4.05$, $p < .001$; paired-samples t test Day 1 versus Day 5: $t(45) = 4.28$, $p < .001$, $d = 0.64$, 95% CI for the mean difference = [0.54, 1.50]—than did participants in the giving condition (change from Day 1 to Day 5: $M = -0.37$, $SD = 1.20$)—model effects: $b = -0.09$, $SE = 0.06$, 95% CI = [−0.20, 0.02]; $t(244.67) = -1.63$, $p = .105$; paired-samples t test Day 1 versus Day 5: $t(49) = 2.15$, $p = .037$, $d = 0.33$, 95% CI for the mean difference = [0.02, 0.71].

The weakest effects emerged for study happiness. We observed a main effect of time, $F(1, 473.99) = 15.46$, $p < .001$, no main effect of condition, $F(1, 97.53) = 0.34$, $p = .561$, and no interaction, $F(1, 473.99) = 2.34$, $p = .127$ (see Fig. 1d and Table 1). Again, however, the patterns were the same: Participants in the getting condition experienced directionally quicker adaptation (change from Day 1 to Day 5: $M = -0.78$, $SD = 1.60$)—model effects: $b = -0.20$, $SE = 0.06$, 95% CI = [−0.31, −0.09]; $t(225.86) = -3.52$, $p = .001$; paired-samples t test Day 1 versus Day 5: $t(45) = 3.29$, $p = .002$, $d = 0.49$, 95% CI for the mean difference = [0.30, 1.25]—than did participants in the giving condition (change from Day 1 to Day 5: $M = -0.35$, $SD = 1.12$)—model effects: $b = -0.09$, $SE = 0.04$, 95% CI = [−0.17, −0.001]; $t(238.67) = -2.00$, $p = .047$; paired-samples t test Day 1 versus Day 5: $t(49) = 2.19$, $p = .033$, $d = 0.32$, 95% CI for the mean difference = [0.03, 0.67].

Other variables. We checked the remaining data for as many other potential differences between the conditions as we could test, which might otherwise explain this asymmetry in adaptation for incidental reasons beyond the self/other component, per se.

Exit survey: variety block. Participants rated their overall experience from day to day as similarly different (getting: $M = 1.64$, $SD = 1.45$; giving: $M = 1.42$, $SD = 1.15$), $t(90) = 0.81$, $p = .420$, $d = 0.17$, 95% CI for the mean difference = [−0.32, 0.76], and similarly variable (getting: $M = 1.80$, $SD = 1.79$; giving: $M = 1.63$, $SD = 1.55$), $t(90) = 0.49$, $p = .626$, $d = 0.10$, 95% CI for the mean difference = [−0.52, 0.86] (further bolstered by the fact that we retained only those participants who reported in all five nightly surveys that they indeed repeated the same exact experience).

Exit survey: specialness block. Participants rated their kinds of gifts as similarly likable (getting: $M = 5.57$, $SD = 1.27$; giving: $M = 5.44$, $SD = 1.30$), $t(90) = 0.49$, $p = .627$, $d = 0.10$, 95% CI for the mean difference = [−0.40, 0.66]; similarly special (getting: $M = 3.75$, $SD = 1.99$; giving: $M = 4.06$, $SD = 1.92$), $t(90) = -0.77$, $p = .445$, $d = 0.16$, 95% CI for the mean difference = [−1.12, 0.50]; similarly thoughtful (getting: $M = 3.50$, $SD = 1.92$; giving: $M = 3.52$, $SD = 1.85$), $t(90) = -0.05$, $p = .958$, $d = 0.01$, 95% CI for the mean difference = [−0.80, 0.76]; and of similar impact (getting: $M = 4.70$, $SD = 1.61$; giving: $M = 4.60$, $SD = 1.82$), $t(90) = 0.28$, $p = .781$, $d = 0.06$, 95% CI for the mean difference = [−0.61, 0.81].

Exit survey: task block. Participants rated the study as similarly difficult (getting: $M = 1.93$, $SD = 1.35$; giving: $M = 2.00$, $SD = 1.37$), $t(90) = -0.24$, $p = .811$, $d = 0.05$, 95% CI for the mean difference = [−0.63, 0.50]; involving similarly large amounts of money (getting: $M = 3.68$, $SD = 1.68$; giving: $M = 3.25$, $SD = 1.76$), $t(90) = 1.20$, $p = .232$, $d = 0.25$, 95% CI for the mean difference = [−0.28, 1.15]; and involving similarly common spending behavior (getting: $M = 3.00$, $SD = 1.68$; giving: $M = 2.52$, $SD = 1.68$), $t(90) = 1.37$, $p = .175$, $d = 0.29$, 95% CI for the mean

difference = [−0.22, 1.18]. Interestingly, participants in the giving condition did find the study to be more confusing (getting: $M = 1.43$, $SD = 1.04$; giving: $M = 2.13$, $SD = 1.65$), $t(90) = -2.39$, $p = .019$, $d = 0.51$, 95% CI for the mean difference = [−1.27, −0.12], as well as marginally more awkward (getting: $M = 1.77$, $SD = 1.10$; giving: $M = 2.35$, $SD = 1.78$), $t(90) = -1.87$, $p = .065$, $d = 0.40$, 95% CI for the mean difference = [−1.20, 0.04].

Adherence and timing. In the exit survey, participants reported similar adherence to instructions (getting: $M = 3.52$, $SD = 0.79$; giving: $M = 3.65$, $SD = 0.53$), $t(90) = -0.89$, $p = .378$, $d = 0.19$, 95% CI for the mean difference = [−0.40, 0.15]. We also checked the spending and time data from the nightly surveys. Participants in the giving condition reported spending a higher daily amount ($M = \$4.85$, $SD = \$0.98$) than participants in the getting condition ($M = \$4.43$, $SD = \$0.88$), $t(94) = -2.23$, $p = .028$, $d = 0.45$, 95% CI for the mean difference = [−0.81, −0.05] (all participants received and kept all $25.00). Participants took the surveys at a similar time (getting: $M = 57.17$ min after 5:00 p.m., or 5:57 p.m., $SD = 76.40$; giving: $M = 57.27$ min after 5:00 p.m., or 5:57 p.m., $SD = 59.66$), $t(94) = -0.01$, $p = .994$, $d = 0.001$, 95% CI for the mean difference = [−27.77, 27.55]; reported a similar time of receiving the gift ($M = 155.33$ min prior to 5:00 p.m., or 2:25 p.m., $SD = 107.43$) and of giving the gift ($M = 120.92$ min prior to 5:00 p.m., or 3:00 p.m., $SD = 115.66$), $t(94) = 1.51$, $p = .135$, $d = 0.30$, 95% CI for the mean difference = [−10.94, 79.76]; and thus had a similar delay between the gift experience and taking the surveys (the sum of these two times; getting: $M = 212.49$, or 3.5 hr, $SD = 138.96$; giving: $M = 178.19$, or 3 hr, $SD = 127.64$), $t(94) = 1.26$, $p = .211$, $d = 0.25$, 95% CI for the mean difference = [−19.73, 88.32].

These findings suggest no obvious incidental differences beyond our intended manipulation. When rerunning our hedonic-adaptation analyses, entering all demographic variables and exit-survey variables as covariates, we found that all reported effects remained unchanged—critical interactions for overall happiness, $F(1, 389.11) = 6.07$, $p = .014$; day's-end happiness, $F(1, 333.93) = 6.91$, $p = .009$; recalled happiness, $F(1, 380.46) = 3.17$, $p = .076$; and study happiness, $F(1, 417.25) = 2.05$, $p = .153$.

Experiment 1 suggests that people may be slow to adapt to certain things, without having to change the content or context across exposures. Showing the pervasive effect, getting grew old: The same good thing provided less happiness the more participants helped themselves. But giving grew old slowly, if at all: The happiness experienced in helping the same other remained relatively high over the course of repetition.

Next, we sought to replicate this effect by testing immediate reactions under more controlled conditions. The naturalistic, stimulus-sampling design of Experiment 1 is ideal for external validity but could not account for all task-specific features no matter how many control variables we assessed. For example, because participants freely chose the task, there may have been incidental location effects. Also, participants who chose foods may have experienced consumption satiation, spoiling more general judgments of happiness. Such effects could be genuine features of the asymmetry in daily life but are not ideal for between-conditions precision. Experiment 2 held constant all objective task features.

Experiment 2: Identical Tasks

Method

Participants played repeated rounds of the same incentivized game. We simply manipulated the fruits of their success: Participants won the same amount of money each round, but the money went to themselves or to a charity of their choice. We hypothesized that happiness from winning would drop less steeply when winning for other people.

We report all measures, manipulations, and exclusions. Data, materials, and preregistration files may be found at https://osf.io/njea2/. Sample size was guided by the recommendation to replicate effects by recruiting 2.5 times the original sample (Simonsohn, 2015). Experiment 1 had an effective sample size of 98 (about 50 per cell). However, because the results in the giving condition reflected a null effect (which may have been statistically significant with a larger sample size), we doubled this recommendation to maximize accuracy of interpretation. As per our preregistered intention, sample size was predetermined at 500 (about 250 per cell).

Participants. We recruited 500 participants from Amazon's Mechanical Turk, yielding 502 participants (age: $M = 37.17$ years, $SD = 12.02$; 52.59% female; 77.89% Caucasian American/White, 6.37% Asian American/Asian, 9.16% African American/Black, 3.98% mixed ethnicity, 2.59% other ethnicity) who completed the experiment for a set payment of $1.00 (plus the bonus winnings in the game). To reduce selection concerns, we advertised the experiment as the "Word Search Study," with no mention of bonus money, giving, happiness, and so on.

Procedure. Participants played a game that we developed for this experiment. First, participants were informed that they would complete 10 short word search puzzles, with each puzzle containing a matrix of random letters that hid three actual words (shown in a key). The words appeared in a variety of directions across puzzles (e.g.,

diagonally, backward). Their task was to find all three words, for each puzzle, by highlighting the letters with a click. Hence, one puzzle counted as 1 round, making 10 rounds in total.

After explaining the general task instructions, we informed participants that they would be asked to report their reactions after each win. They read the following passage:

> Please be honest! As you're going along: If each win feels just as great as the last, please report the same high rating of happiness. However, if your later and later wins start to feel slightly less good/"fresh" as the first few times, please report that too. We're really just curious about your own honest reactions as you repeatedly win the same thing.

We included this prompt to maximize the chances of observing adaptation in this context (e.g., Mechanical Turk users may find it odd or risky to report lower happiness to a bonus, even if they do react less intensely). Critically, this prompt was identical for all participants.

Each participant then was randomly assigned to one of two conditions; all prompts and procedures were identical except where noted. Participants in the getting condition (n = 253) were informed that for each round, they would win a $0.05 bonus for themselves, to be added to their account 3 to 7 days after participation. Participants in the giving condition (n = 249) were informed that for each round, they would win a $0.05 bonus for "a cause they personally care about," to be donated online by us and with us sending the receipt to their account 3 to 7 days after participation. They chose who they wanted to play for from five charities: Brain & Behavior Research Foundation, Breast Cancer Research Foundation, CARE USA, Scholarship America, and Semper Fi Fund. We provided information about each charity and links to their websites, plus the following prompt about our (true) vetting process using descriptions from CharityWatch:

> These charities allow for easy online donations, which we'll use for this study. Each of these charities receives an A+ from CharityWatch, an independent watchdog (see www.charitywatch.org/top-rated-charities). This means you should feel confident in each charity: these charities are popular, reputable, and well established; extremely transparent with their spending; extremely effective in actual aid relief; and extremely efficient (all donation amounts really do help).

Participants then played the same game. The puzzles were presented one round at a time in random order. We designed the puzzles to be solvable. Participants received real-time feedback about their answers and could proceed to the next round only when they found all three words (all participants therefore won all rounds, as designed).

Key dependent measures: hedonic adaptation. After each win, participants read, "You just won 5 cents [You just won 5 cents for (name of choice)]. At this point of the study, how does this act of winning feel?" and rated three items—"making me feel happy," "making me feel elated," and "making me feel joy"—on a scale from 1 (*weak*) to 7 (*strong*). This was our sole happiness block, repeated in this set order in real time after participants won each of the 10 rounds. We phrased the question in this way and used these scale items and anchors to more clearly capture participants' ongoing affective reactions to the same specific outcome as identical exposure increased (e.g., as opposed to participants interpreting the question as a general agreement that getting or giving money is desirable).

Other measures: postgame survey. Participants then completed a postgame survey. First, they were asked, "How challenging were the word puzzles?" "How much time did the word puzzles take to complete?" and "How confusing was the task?" Each item was rated on a scale from 1 (*not at all*) to 7 (*extremely*) in this order. Second, participants rated an attention-check question regarding the number of puzzles they completed (fillable text box). Third, they were asked, "How helpful do you feel like your bonus winnings are, all things considered?" (forced choice: *not at all, a little bit, extremely*), and participants in the giving condition were also asked, "How meaningful do you actually find your chosen charity? (i.e., you personally find it important, consequential, care about what they do, etc.)" (forced choice: *not at all, a little bit, extremely*). Last, all participants reported any technical difficulties (forced choice: *no, yes [please explain]*) and demographic information.

After completing data collection, we calculated and donated the earned amounts to each charity and then sent participants in the giving condition their donation receipts and awarded participants in the getting condition their earned amounts as advertised.

Results

Key analyses: hedonic adaptation. We collapsed the happiness items into scales (each round, αs $\geq$ .96). We conducted the same analyses as in Experiment 1. We observed a main effect of time, $F(1, 4998.88) = 85.35$, $p < .001$, and a marginal main effect of condition, $F(1, 510.63) = 2.91$, $p = .089$. Critically, these effects were qualified by the hypothesized interaction, $F(1, 4998.88) = 9.39$, $p = .002$ (for a visual depiction, see Fig. 2; for descriptive statistics, see Table 2).

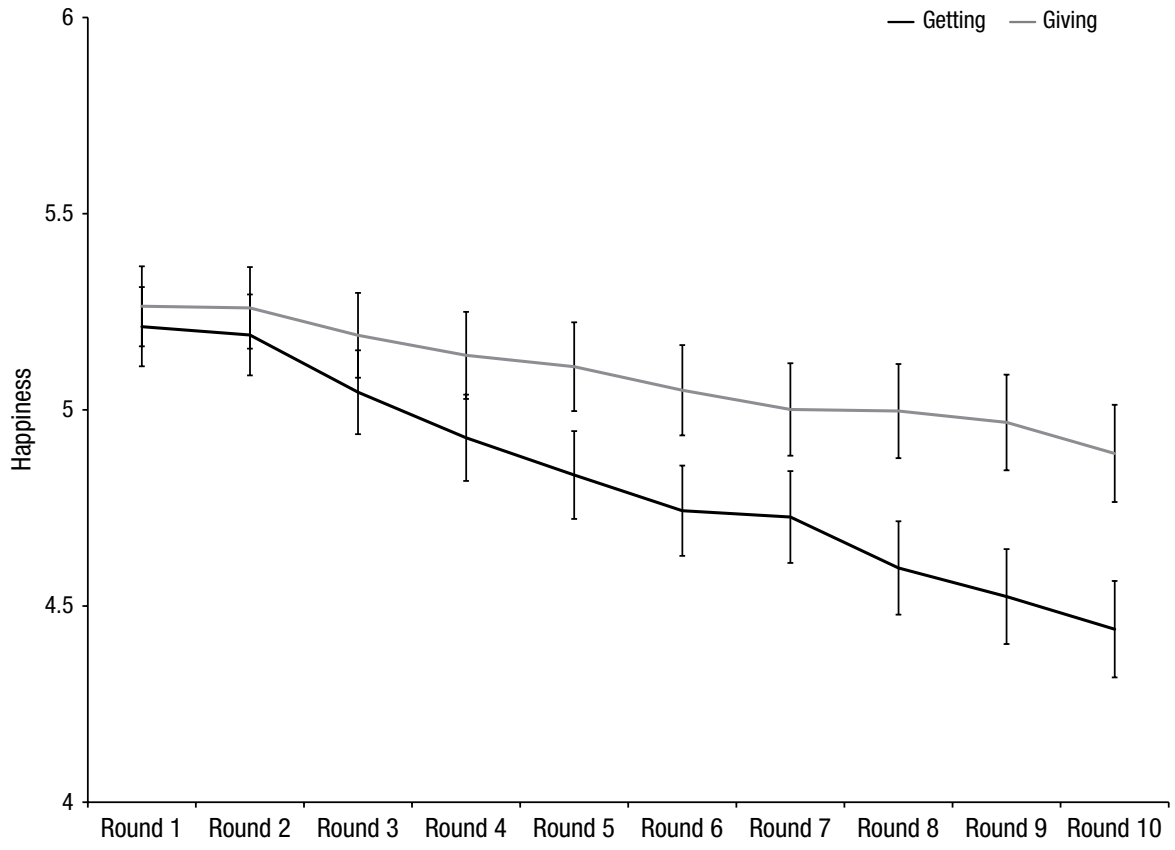

Fig. 2. Results from Experiment 2: mean happiness in the giving and getting conditions in each round. Error bars indicate ±1 *SE*.

Participants in the getting condition experienced standard adaptation; specifically, getting the same thing over and over again significantly grew old (change from Round 1 to Round 10: $M = -0.77$, $SD = 1.44$)—model effects: $b = -0.15$, $SE = 0.02$, 95% CI = [−0.18, −0.11]; $t(2451.65) = -7.82$, $p < .001$; paired-samples t test Round 1 versus Round 10: $t(252) = 8.50$, $p < .001$, $d = 0.56$, 95% CI for the mean difference = [0.59, 0.95]. These findings replicated those of Experiment 1. However, we found that participants in the giving condition also adapted: Giving the same thing over and over again significantly grew old, unlike in Experiment 1 (change from Round 1 to Round 10: $M = -0.37$, $SD = 1.25$)—model effects: $b = -0.07$, $SE = 0.01$, 95% CI = [−0.10, −0.05]; $t(2477.08) = -5.17$, $p < .001$; paired-samples t test Round 1 versus Round 10: $t(248) = 4.74$, $p < .001$, $d = 0.30$, 95% CI for the mean difference = [0.22, 0.53]. This result among givers may have reached statistical significance in this experiment but not in the previous experiment because of the larger sample size. More critically, the significant interaction indicates that the rate of adaptation was significantly slower for giving than it was for identical getting: Happiness from repeated giving declined at about half the rate as happiness from repeated getting.

Other variables. This asymmetry was not explained by incidental differences in the task. The task was rated as similarly challenging (getting: $M = 2.58$, $SD = 1.53$; giving: $M = 2.63$, $SD = 1.62$), $t(500) = -0.38$, $p = .704$, $d = 0.03$, 95% CI for the mean difference = [−0.33, 0.22]; as taking a similarly large amount of time (getting: $M = 2.91$, $SD = 1.42$; giving: $M = 3.06$, $SD = 1.61$), $t(500) = -1.06$, $p = .291$, $d = 0.10$, 95% CI for the mean difference = [−0.41, 0.12]; and as similarly confusing (getting: $M = 1.60$, $SD = 1.20$; giving: $M = 1.74$, $SD = 1.40$), $t(500) = -1.26$, $p = .209$, $d = 0.11$, 95% CI for the mean difference = [−0.38, 0.08]. Only

Table 2. Descriptive Statistics for Experiment 2

Condition	Round 1	Round 2	Round 3	Round 4	Round 5	Round 6	Round 7	Round 8	Round 9	Round 10
Getting	5.21 (1.48)	5.19 (1.54)	5.04 (1.65)	4.93 (1.72)	4.83 (1.77)	4.74 (1.81)	4.73 (1.86)	4.60 (1.88)	4.52 (1.95)	4.44 (1.98)
Giving	5.26 (1.72)	5.26 (1.72)	5.19 (1.75)	5.14 (1.78)	5.11 (1.80)	5.05 (1.83)	5.00 (1.85)	5.00 (1.89)	4.97 (1.90)	4.89 (1.93)

Note: Values are mean ratings, with standard deviations in parentheses.

2.39% of participants (12 of 502) failed the attention check, and only 1.99% of participants (10 of 502) reported technical difficulties. The majority of participants reported that the bonus winnings were indeed helpful: 98.42% of participants in the getting condition (249 of 253) and 83.94% of participants in the giving condition (209 of 249) indicated *a little bit* or *extremely*. The majority of participants in the giving condition reported that their chosen charity was indeed personally meaningful: 96.39% (240 of 249) indicated *a little bit* or *extremely*. When we again ran our hedonic-adaptation analyses, entering all demographic variables and postgame variables as covariates, all reported effects remained unchanged, critical interaction: $F(1, 4981.45) = 9.45$, $p = .002$.

Experiment 2 replicated the effect under more controlled conditions. Winning the same exact reward in the same way 10 times in a row dulled one's positive reactions to the experience of winning but to a significantly lesser extent when winning for other people.

Discussion

> This point cannot be overstated: *Every* desirable experience is transitory.
>
> —Myers (1992, p. 53)

> Hedonic adaptation can be resisted, but only with conscious, active efforts.
>
> —Lyubomirsky (2010, p. 219)

Two common assumptions about happiness are that the more we experience good things, the less happiness they provide, and thus, to sustain happiness, we must change how they are experienced. The present research suggests that the kind of thing may matter more than assumed, extending experiments on hedonic adaptation beyond strict getting: Happiness from giving appears to sustain itself.

Our findings are preliminary but robust. Perhaps one's hundredth donation falls flat, but if all happiness declines equally, happiness in our experiments should have declined just as quickly for repeated giving as it did for repeated getting because content and context were held constant across exposures. Perhaps consumption satiation was needed to spoil happiness in our experiments, but this cannot explain Experiment 2 or other research showing that people can and do adapt to cognitive pleasures (Morewedge, Huh, & Vosgerau, 2010). Perhaps prosocial experiences generally are more impactful than self-oriented experiences in daily life, but many such features were accounted for between, and particularly within, our conditions (e.g., we enabled and matched spending amounts, and we found no differences between conditions at Time 1 and did not change any features within conditions thereafter). Giving for giving's sake may feel good for longer than does comparable getting, even when repeatedly helping in identical ways.

Why? This question invites novel directions for research. Recent research on hedonic adaptation has emphasized context (e.g., mapping strategies for how to repeat things with less emphasis on what things to repeat; Sheldon et al., 2013), whereas other well-being research has emphasized content (e.g., documenting that people feel happy after helping or socializing with less emphasis on whether people remain happy repeating such events; Dunn et al., 2008; Kahneman, Krueger, Schkade, Schwarz, & Stone, 2004). Our findings bridge these foci, highlighting the need for a clearer taxonomy of things that do versus do not deteriorate from exposure. Previous research suggests that adaptation may be slower for complex stimuli (Bornstein et al., 1990), experiential goods (Nicolao et al., 2009), and sentimental goods (Yang & Galak, 2015). Perhaps such features underlie interpersonal events, generally focusing people away from the positive outcome and toward the positive act (potentially slowing adaptation). More broadly, to the extent that the psychology of adaptation reflects a functional reprioritization of what is presently relevant and beneficial, acts that confer advantages by virtue of continued exposure may be relatively resistant on their own. This idea raises intriguing questions, such as whether converse experiences are also slow to adapt (e.g., the pain of harming other people, receiving prosociality), whether prosocial framings combat self-adaptation (e.g., eating identical foods, but their profits were donated), and potential reciprocation effects (e.g., giving to known or close others versus giving to strangers).[1]

Future research should also address important constraints on generality. First, we used relatively small spending amounts. Although the asymmetry may well hold using larger amounts (e.g., winning the lottery five times in a row may feel great, but our key comparison is relative to having that same profound impact on other people), it generally may be easier to extract value from small acts of giving than small acts of getting. Likewise, people with financial burdens or who spend personal savings rather than windfalls may show different patterns. The current findings do not warrant universal recommendation. Second, other measures beyond self-report should be tested (e.g., physiological reactions to winning). Third, other repetition cycles should be tested. Long temporal windows presumably should reset people's experiences (e.g., taking breaks; Quoidbach & Dunn, 2013), yet Experiment 1 elicited adaptation with a full day between exposures. What counts as long? We suspect that framing matters. The fact that Experiment 1 was described as a 5-day study with one survey per day likely conveyed to participants that the diagnostic unit of time was 1 day, and thus, immediate repetition means

back-to-back days. This idea is echoed in the constructed nature of food satiation (e.g., merely thinking that one's last meal occurred recently can lead people to feel full; Redden & Galak, 2013). Regardless, disentangling these boundaries would be practically informative (e.g., if people take 1 personal day a month, they may remain just as happy using that time to treat themselves).

Finally, future research should further clarify the extent to which prosociality comes with objective features that slow adaptation. For example, perhaps our prosocial conditions involved more variety (e.g., giving to different people, assuming that each donation serves different needs), and variety slows adaptation (Sheldon et al., 2013). We sought to account for this possibility (e.g., in Experiment 1, we analyzed only those participants who treated the same other the same way each day, and we found no differences in Time 1 happiness or in end ratings of variety, impact, and so on; in Experiment 2, exposure was held constant, and we found no differences in Time 1 happiness or in end ratings of the helpfulness of the bonuses). Moreover, participants in the getting condition could have just as well construed the experience in such terms, too (e.g., viewing each bonus as serving different needs of their own). Nonetheless, follow-up research could fruitfully unpack these features (e.g., versions of Experiment 2 in which participants play for one specific person's one specific need vs. framing one's own needs as a collective). Until these possibilities are tested, the present research encourages an initial step toward broadening the boundaries of adaptation. Some gold things might stay.

Action Editor

Leaf Van Boven served as action editor for this article.

Author Contributions

Both authors contributed to the study idea and methodological designs and performed testing and data collection. E. O'Brien drafted the manuscript. S. Kassirer provided critical revisions. Both authors approved the final manuscript for submission.

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Open Practices

All data and materials have been made publicly available via the Open Science Framework (OSF) and can be accessed at osf.io/njea2. Both experiments were preregistered; the preregistration forms can be found on the OSF at osf.io/njea2. The complete Open Practices Disclosure for this article can be found at http://journals.sagepub.com/doi/suppl/10.1177/0956797618814145. This article has received the badge for Open Data, Open Materials, and Preregistration. More information about the Open Practices badges can be found at http://www.psychologicalscience.org/publications/badges.

Note

1. Before launching Experiment 1, we conducted a pilot version for 1 week (pilot data were discarded before launching the actual experiment). In the pilot, participants in the giving condition were explicitly instructed to find a stranger and give the money in person (e.g., handing the money to someone around campus who appeared to be in need). In hindsight, these pilot instructions were not ideal because they likely involved variety in the specific target from day to day. More interesting, pilot givers reported extremely high awkwardness, reported feeling like they were patronizing the stranger, and did not report feeling particularly happy. Therefore, as can be seen in the instructions that we ended up using for Experiment 1, we encouraged participants to give in various other ways (e.g., giving to anonymous online outlets and to close others were eligible). Given our pilot experience, however, we suspect that enforced, unsolicited giving to strangers may elicit a level of discomfort that dilutes any immediate warm glow.

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