

Does Spending Money on Others Promote Happiness?: A Registered Replication Report

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Research indicates that spending money on others—*prosocial spending*—leads to greater happiness than spending money on oneself (e.g., Dunn, Aknin, & Norton, 2008, 2014). These findings have received widespread attention because they offer insight into why people engage in costly prosocial behavior, and what constitutes happier spending more broadly. However, most studies on prosocial spending (like most research on the emotional benefits of generosity) utilized small sample sizes ($n < 100/\text{cell}$). In light of new, improved standards for evidentiary value, we conducted high-powered registered replications of the central paradigms used in prosocial spending research. In Experiment 1, 712 students were randomly assigned to make a purchase for themselves or a stranger in need and then reported their happiness. As predicted, participants assigned to engage in prosocial (vs. personal) spending reported greater momentary happiness. In Experiment 2, 1950 adults recalled a time they spent money on themselves or someone else and then reported their current happiness; contrary to predictions, participants in the prosocial spending condition did not report greater happiness than those in the personal spending condition. Because low levels of task engagement may have produced these null results, we conducted a replication with minor changes designed to increase engagement; in this Experiment 3 ($N = 5,199$), participants who recalled a prosocial (vs. personal) spending memory reported greater happiness but differences were small. Taken together, these studies support the hypothesis that spending money on others does promote happiness, but demonstrate that the magnitude of the effect depends on several methodological features.

Keywords: prosocial spending, generosity, happiness, well-being, replication


People face countless spending choices in everyday life. Do some purchases offer greater happiness returns than others?

In “Spending Money on Others Promotes Happiness” (Dunn et al., 2008), we presented a series of studies demonstrating that spending money on others (i.e., *prosocial spending*) was associated with self-reported happiness. The strongest evidence for causality came from Study 3, in which 46 undergraduate students at

the University of British Columbia were randomly assigned to spend a small windfall of either \$5 or \$20 on themselves or someone else by the end of the day. In the evening, all participants were called on the phone and asked to report their happiness. Participants randomly assigned to spend money on others—whether \$5 or \$20—were happier.

Subsequent research has offered converging evidence for the emotional rewards of prosocial (vs. personal) spending. Aknin and colleagues (2013) examined the hedonic consequences of acts of prosocial spending in rich and poor nations, assessing whether the link between prosocial spending and happiness was limited to relatively wealthy nations. In one study, a sample of 207 students from Canada ($n = 86$) and South Africa ($n = 121$) earned a small monetary sum that they could use to purchase edible treats. They were randomly assigned either to a personal spending condition in which they purchased the treats for themselves, or a prosocial spending condition in which they purchased treats for a sick child at a local children’s hospital. Afterward, all participants reported their happiness. In both Canada and South Africa, students who engaged in prosocial spending were happier. In another study, people from Canada ($n = 140$) and Uganda ($n = 680$) who were

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randomly assigned to think about a time they spent \$20 (or its equivalent in Ugandan shillings) of their own money on someone else reported greater happiness than those randomly assigned to think about a time they spent \$20 on themselves. Similar results were observed in a separate study conducted with 101 participants from India in which respondents recalled a time they spent money on either themselves, spent money on others, or did not recall a spending memory.

Research examining the well-being consequences of prosocial spending has received a large amount of attention in the academic literature and beyond. The two papers mentioned above have been cited nearly 2,000 times collectively (1,475 for Dunn et al., 2008; 516 for Aknin et al., 2013 according to Google Scholar on December 11, 2019). These findings are described in widely used introductory textbooks (e.g., Aronson, Wilson, Fehr, & Akert, 2013; Myers, 2010; Smith, Mackie, & Claypool, 2014) and have been discussed in media outlets worldwide (e.g., *The Guardian*, *The Wall Street Journal*, and *The New York Times*). These findings have also been featured in popular science books (e.g., “Give and Take,” Grant, 2014; “Drive,” Pink, 2011) and utilized in providing behavioral insights for policymakers (e.g., the U.K. Cabinet’s *Charitable Giving Assessment*).

Given the wide dissemination of research on the happiness benefits of prosocial spending, replicating the foundational studies is likely to be of interest to a wide range of social scientists. In the years since these studies were conducted, our field has undergone substantial change, and there is a growing recognition of the importance of using larger samples and preregistration (Button et al., 2013; Ioannidis, 2005; Nosek, Ebersole, DeHaven, & Mellor, 2018). According to a recent meta-analysis of existing studies, generosity (broadly defined) has a small to medium causal effect on happiness (Curry et al., 2018) and, thus, sample sizes of at least 200 participants per condition are needed to detect a main effect of prosocial behavior. Unfortunately, very few studies examining the hedonic consequences of prosocial spending meet this threshold (including our own). In fact, most experiments on this topic have used cell sizes of 100 or less (see Table 1) and, thus, should be interpreted with caution, given that underpowered studies carry an increased risk of false positives (Button et al., 2013; Fraley & Vazire, 2014).

Very recently, several relevant studies have utilized larger sample sizes, although these studies were designed primarily to test novel hypotheses about prosocial spending rather than to replicate earlier findings. In a preregistered study ($n = 150$ per condition), O’Brien and Kassirer (2019, Study 2) examined whether the emotional benefits of giving are resistant to hedonic adaptation. Amazon Mechanical Turk (MTurk) workers earned a series of five-cent bonus payments for solving 10 puzzles, and depending on condition, all the payments went to participants themselves or to a charity of their choice. The pleasure of winning money declined at a lower rate when participants got money for charity versus for themselves; in fact, participants in the charity condition showed no evidence of hedonic adaptation at all. Another large study ($n > 210$ per condition) investigated whether individual differences in oxytocin receptor genes predict the emotional rewards derived from prosocial spending (Whillans, Aknin, Ross, Chen, & Chen, 2019). Students were randomly assigned to purchase treats for either themselves or a sick child at a local hospital before reporting their happiness and providing a saliva sample to collect genetic

information. Although this research revealed no effects of oxytocin receptor genes, individuals randomly assigned to buy treats for a sick child reported greater happiness than those who bought for themselves. This study was not preregistered, however, and it is appropriate to treat this replication with caution.

Finally, Hanniball and colleagues (2019) conducted three large experiments with cell sizes ranging from 250 to 588 among ex-offender samples reporting elevated levels of antisocial tendencies. Participants were asked to either recall or engage in an act of personal or prosocial spending before reporting their momentary well-being. In each study, participants assigned to the prosocial spending condition reported higher levels of happiness when controlling for baseline well-being, but the effect sizes were very small ($d_s = .11$ to $.16$). In light of these findings, a plausible conclusion (drawn by some reviewers of that paper) would be that the happiness benefits of prosocial spending border onto trivial, and that past research greatly overestimated the size of this effect. However, another plausible interpretation is that people with antisocial tendencies should be least likely to exhibit the warm glow of giving, and observing even a small effect with this special population underscores the robustness of the hedonic benefits of giving.

To resolve this debate and enable an accurate estimate of effect size, we replicated the key paradigms in this research stream using well-powered, preregistered studies with participants drawn from the broader population. The first experiment investigated the immediate emotional rewards of prosocial (vs. personal) spending by providing participants the opportunity to spend a small monetary windfall on edible treats for themselves or an unknown sick child at a local children’s hospital (see Aknin et al., 2013, Study 3). Afterward, participants reported their current well-being. The second experiment used a recollection procedure (as in Aknin et al., 2013, Studies 2a and b) in which participants were randomly assigned to reflect upon a previous purchase made for themselves or others before reporting their well-being. In both experiments, we predicted that participants randomly assigned to engage in or to reflect upon prosocial (vs. personal) spending would report greater momentary happiness when controlling for baseline well-being. Experiment 1 provided robust evidence for the immediate emotional rewards of generous spending, but Experiment 2 revealed a null result, possibly because participants did not fully engage with the task. Therefore, in a third experiment, we conducted an additional test of the recollection paradigm in which participants were required to provide longer spending descriptions to encourage vivid recollections and greater engagement. Materials, data, hypotheses, and syntax for all studies are posted on the Open Science Framework (OSF; <https://osf.io/d6ymu/>).

Experiment 1

Method

Sample. We preregistered recruiting a maximum sample of 896 individuals across two Canadian universities in exchange for course credit. According to G*Power, this sample size allowed us to detect an effect size of $d = .22$ with 95% power using $\alpha = .05$, one-tailed. The effect size estimate of $d = .22$ was based on the average effect observed in past prosocial spending studies with cell sizes of $n > 100$ ($d = .22$; see Table 1). However, given the

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Table 1
Observed Effect Sizes in Prosocial Spending Experiments and Other Prosocial Intervention Experiments on Happiness

Source	Study	Dependent variable	Experimental (E)	Control (C)	n_E	n_C	Sample (location)	Cohen's d [95% CI]	Baseline happiness controlled
Aknin, Barrington-Leigh, et al. (2013)	2a ^b	H	Recall prosocial spending	Recall personal spending	410	410	Student (Canada; Uganda)	0.20 [0.06, 0.34]	No
	2b	H	Recall prosocial spending	Recall personal spending	22	18	Community (India)	0.55 [-0.10, 1.19]	No
	3 ^a	PA	Prosocial spending	Personal spending	104	103	Student (Canada; South Africa)	0.46 [0.18, 0.74]	Yes
	3 ^a	SWLS	Prosocial spending	Personal spending	104	103	Student (Canada; South Africa)	0.13 [-0.14, 0.40]	No
Aknin, Broesch, Hamlin, and Van de Vondervoort (2015)	1	PA	Prosocial spending	Personal spending	13	13	Community (Vanuatu)	0.87 [0.01, 1.70]	Yes
	2	Smiling	Donate sweets (own)	Donate sweets (other)	20	20	Children (Vanuatu)	0.30 [-0.33, 0.92]	No
Aknin, Dunn, et al. (2013)		WB	Prosocial spending	Personal spending	25	25	Community (Canada)	0.24 [-0.32, 0.80]	No
Aknin, Fleckers, and Hamlin (2014)		PA	Prosocial spending	Personal spending	60	59	Student (Canada)	0.38 [0.01, 0.74]	Yes
		ORH	Prosocial spending	Personal spending	60	59	Research Assistant (Canada)	0.44 [0.07, 0.81]	Yes
Aknin, Dunn, and Norton (2012)		H	Recall prosocial spending	Recall personal spending	26	25	Students (Canada)	0.61 [0.03, 1.18]	No
Aknin, Hamlin, and Dunn (2012)		Smiling	Donate sweets (own)	Donate sweets (other)	20	20	Children (Canada)	0.46 [-0.18, 1.09]	No
Alden and Trew (2013)		PA	Prosocial behavior	Safety behavior experiment	43	40	Socially anxious student (Canada)	0.59 [0.14, 1.04]	No
		PA	Prosocial behavior	Life details tracking	43	43	Socially anxious student (Canada)	0.54 [0.10, 0.98]	No
Anik, Aknin, Norton, Dunn, and Quidbach (2013)	1	PA	Prosocial spending (\$25)	None	41	48	Community (Australia)	-0.15 [-0.57, 0.27]	Yes
	1	PA	Prosocial spending (\$50)	None	41	48	Community (Australia)	0.49 [0.06, 0.92]	No
Buchanan and Bardi (2010)	1	SWLS	Prosocial behavior	New behavior	28	28	Community (U.K.)	0.41 [-0.13, 0.94]	No
	1	SWLS	Prosocial behavior	No behavior	28	28	Community (U.K.)	0.62 [0.07, 1.16]	No
Chancellor, Margolis, Jacobs Bao, and Lyubomirsky (2018)		SHS	Prosocial behavior	Prosocial receiver	16	34	Community (Spain)	^c	—
		SWLS	Prosocial behavior	Prosocial receiver	16	34	Community (Spain)	^c	—
		SHS	Prosocial behavior	None	16	33	Community (Spain)	^c	—
		SWLS	Prosocial behavior	None	16	33	Community (Spain)	^c	—
Donnelly, Lambertson, Reezek, and Norton (2017)	1	H	Social recycling	Trash/recycling	59	56	Student (U.S.)	0.77 [0.38, 1.16]	No
	1	H	Social recycling	Take item	59	59	Student (U.S.)	0.85 [0.45, 1.24]	No
	2b	PA	Social recycling	Trash	107	108	Community (U.S.)	1.25 [0.93, 1.56]	No
Dunn, Aknin, and Norton (2008)	3	H	Prosocial spending	Personal spending	23	23	Student (Canada)	0.67 [0.05, 1.27]	Yes
Geenen et al. (2014)		H	Prosocial spending	Personal spending	34	34	Student (Germany)	0.70 [0.19, 1.20]	Yes
Hamiball and Aknin (2016)		PA	Prosocial behavior	Self-helping behavior	51	56	Students (Canada)	-0.46 [-0.84, -0.07]	No
Hamiball, Aknin, Douglas, and Vijjoen (2019)	1 ^b	PA	Recall prosocial spending	Recall personal spending	250	251	Ex-offender adults (U.S.)	0.20 [0.02, 0.38]	Yes
	2	PA	Prosocial spending	Personal spending	31	33	Delinquent youth (Canada)	0.70 [0.17, 1.22]	Yes
	3 ^a	PA	Prosocial spending	Personal spending	357	420	Ex-offender adults (U.S.)	0.16 [0.02, 0.30]	Yes
	4 ^a	PA	Prosocial spending	Personal spending	588	707	Ex-offender adults (U.S.)	0.11 [0.00, 0.22]	Yes
Layous, Kurtz, Margolis, Chancellor, and Lyubomirsky (2017)	1	SHS	Prosocial behavior	Track daily activity	70	69	Student (U.S.)	0.08 [-0.25, 0.41]	No
	1	WB	Prosocial behavior	Track daily activity	70	69	Student (U.S.)	0.20 [-0.13, 0.53]	No
	1	EWB	Prosocial behavior	Track daily activity	70	69	Student (U.S.)	0.26 [-0.08, 0.59]	No
	2	SHS	Prosocial behavior	Make self happier	178	81	Student (U.S.)	0.30 [0.04, 0.56]	No
	2	WB	Prosocial behavior	Make self happier	178	81	Student (U.S.)	0.12 [-0.14, 0.38]	No
Layous, Lee, Choi, and Lyubomirsky (2013)		WB	Prosocial behavior	Track locations	213	104	Student (U.S./Korea)	0.18 [-0.06, 0.41]	No

(table continues)

Table 1 (continued)

Source	Study	Dependent variable	Experimental (E)	Control (C)	n_E	n_C	Sample (location)	Cohen's d [95% CI]	Baseline happiness controlled
Layous, Nelson, Oberle, Schonert-Reichl, and Lyubomirsky (2012)		SHS	Prosocial behavior	Whereabouts	208	208	Youth (Canada)	-0.05 [-0.24, 0.14]	No
		PA	Prosocial behavior	Whereabouts	208	208	Youth (Canada)	-0.12 [-0.31, 0.07]	No
		SWLS	Prosocial behavior	Whereabouts	208	208	Youth (Canada)	0.07 [-0.12, 0.26]	No
		PA	Benevolence	Neutral activity	34	42	Students (U.S.)	0.55 [0.08, 1.02]	No
Martela and Ryan (2016)		SHI	Prosocial behavior	Memory	237	237	Community (Canada)	0.01 [-0.17, 0.19]	No
Mongrain, Chin, and Shapira (2011)		SHS	Prosocial behavior	Work activity	101	117	Students (U.S.; Korea)	0.23 [-0.15, 0.61]	No
Nelson et al. (2015)		SWLS	Prosocial behavior	Work activity	101	117	Students (U.S.; Korea)	0.27 [-0.11, 0.65]	No
		PE	Prosocial behavior	Work activity	101	117	Students (U.S.; Korea)	0.09 [-0.28, 0.46]	No
Nelson, Layous, Cole, and Lyubomirsky (2016)		PE	Prosocial behavior	Track activities	238	116	Community/student (U.S.)	0.30 [0.08, 0.52]	No
		PE	Prosocial behavior	Self	238	116	Community/student (U.S.)	0.20 [-0.02, 0.42]	No
O'Brien and Kassirer (2019)	1	H	Prosocial spending	Personal spending	59	54	Student (U.S.)	0.35 [-0.03, 0.73]	No
	1	H	Prosocial spending	Personal spending	59	54	Student (U.S.)	0.46 [0.08, 0.84]	No
	1	H	Prosocial spending	Personal spending	59	54	Student (U.S.)	0.35 [-0.03, 0.72]	No
	1	H	Prosocial spending	Personal spending	59	54	Student (U.S.)	0.09 [-0.28, 0.46]	No
	2 ^a	H	Prosocial spending	Personal spending	249	253	Community (U.S.)	0.20 [0.02, 0.38]	No
O'Connell, O'Shea, and Gallagher (2016)		SHS	Prosocial behavior	List activities	28	12	Community (U.S.)	0.02 [-0.66, 0.70]	No
		SHS	Prosocial behavior	Self	28	31	Community (U.S.)	0.12 [-0.39, 0.63]	No
Ouweneel, Le Blanc, Schaufeli, and Schaufeli (2014)	2	PE	Prosocial behavior	Neutral activity	25	24	Student (Netherlands)	0.27 [-0.30, 0.83]	No
Trew and Alden (2015)		PA	Prosocial behavior	Social exposure	38	41	Socially anxious student (Canada)	-0.05 [-0.49, 0.39]	Yes
		PA	Prosocial behavior	List activities	36	41	Socially anxious student (Canada)	-0.33 [-0.78, 0.13]	Yes
Whillans, Dunn, Sandstrom, Dickerson, and Madden (2016)		WB	Prosocial spending	Personal spending	36	37	Hypertense older adults (Canada)	0.19 [-0.27, 0.65]	No
Whillans, Aknin, Ross, Chen, and Chen (2019)	^a	PA	Prosocial spending	Personal spending	218	219	Students (Canada)	0.23 [0.04, 0.42]	Yes
			Average prosocial spending effect size ($n_s > 100$)	Average prosocial spending				$d = .22$	
			Average recall prosocial spending effect size ($n_s > 100$)	Average recall prosocial spending				$d = .20$	

Note. EWB = Eudaimonic well-being; H = happiness; ORH = other rated happiness; PA = positive affect; PE = positive emotion; SHS = Subjective Happiness Scale; SHI = Steen Happiness Index; SWLS = Satisfaction With Life Scale; WB = well-being.

^a Prosocial spending intervention with $n_s > 100$; entered in estimate of Average Prosocial Spending Effect Size ($n_s > 100$). ^b Recall prosocial spending intervention with $n_s > 100$; entered in estimate of Average Recall Prosocial Spending Effect Size ($n_s > 100$). ^c Statistics needed to calculate effect size were not reported in the article, nor available from the authors.

practical limitations surrounding subject pool size, cost, and time, we preregistered our intention to perform sequential analyses (Lakens, 2014). This strategy allowed us to examine our primary preregistered hypothesis using an alpha of 0.0387 after collecting data from 694 participants. The preregistration for Experiment 1 can be found on the OSF (<https://osf.io/gz7a6/>).

We ended up recruiting a sample of 730 individuals ($M_{\text{age}} = 19.91$, $SD = 2.71$; 73.6% female, 25.6% male, 0.4% other, 0.4% missing). This sample is slightly higher than our interim target of 694 because we replaced participants who were excluded (based on our preregistered criteria) with random assignment. As in the original study (Aknin et al., 2013, Study 3), participants were run in small group sessions to facilitate timely data collection. Importantly, all responses were provided in private behind desk dividers to minimize self-presentation concerns. This study was approved by our institutional review boards.

Procedure. Baseline emotion was assessed using the same items as Study 3 in Aknin et al. (2013). Participants reported their baseline level of happiness on a state (“Do you feel happy right now?”; from 1 = *not at all*, to 5 = *extremely*) and trait (“In general, I consider myself . . .”; from 1 = *not a very happy person*, to 7 = *a very happy person*) measure (Lyubomirsky & Lepper, 1999). As expected and specified in our preregistered analysis plan, these scores were correlated, $r(710) = .423$, $p < .001$, so we standardized and averaged them to create a baseline measure of happiness. Baseline happiness items were presented among a few filler questions (e.g., “How tired are you feeling right now?”) to disguise our interest in happiness.

Goody-bag paradigm. After completing the baseline measures, participants were provided with a questionnaire informing them that they had earned \$2.50 in addition to course credit for their participation. Funds were presented in the form of a paper voucher and participants were asked to sign a receipt to encourage feelings of ownership over the funds. The questionnaire then invited participants to use their voucher to purchase a goody-bag filled with chocolate, juice, or both, valued at \$3.00. Critically, participants were randomly assigned to one of two spending conditions. In the *personal spending* condition, participants were told that the goody-bag they purchased was for them and available for pickup at the conclusion of the experiment. In the *prosocial spending* condition, participants were told that the goody-bag they purchased would be donated to a sick child at a local children’s hospital.

Participants made their spending choice (two chocolates, two juice boxes, or one chocolate and one juice box) in private by selecting their preferred option on a purchase card. Once selected, participants took the purchase card to a research assistant in a private room. Here, the research assistant packaged the requested items so the participant could see their purchase was real. The packaged goody bag containing the purchased items was then marked with the appropriate participant number and set-aside until the completion of the study. The research assistant then handed each participant a preprepared thank you note reinforcing their condition assignment. Specifically, participants in the personal condition received a note saying, “Thanks for your purchase! Your items will be available for pickup at the end of the study!” Meanwhile, participants in the prosocial condition received a note saying, “Thanks for your purchase! Your items will be donated to a sick child at Children’s Hospital at the end of the study!”

Critically, the research assistant did not know what condition participants had been assigned to, and hence the recipient of the goody bag, to ensure similar interactions with all participants. Thus, because all study materials were identical, research assistants were blind to condition assignment during the experiment. Research assistants only learned of a participant’s condition assignment at the end of the experimental session so that they could return items to individuals in the personal spending condition. Gifts purchased in the prosocial spending condition were donated to a local charity for sick children and their families.

Opt-out. Past research has shown that a sense of volition is essential for experiencing the emotional rewards of prosocial behavior (Weinstein & Ryan, 2010). Therefore, participants in both conditions had the opportunity to opt-out of purchasing a goody-bag and take the cash value (\$2.50) for themselves. This option ensured that participants in the prosocial spending condition felt as though they had chosen to give a gift. To discourage participants from opting out, cash collections were only available on one early morning at the end of the semester. As noted in the preregistration and consistent with past research, participants assigned to the prosocial spending condition who chose to opt out of making a purchase ($n = 18$, $< 2.5\%$ of the sample) were excluded from the primary analysis because they did not engage in a prosocial act. This meant our final sample, after exclusions, included 712 individuals ($M_{\text{age}} = 19.91$, $SD = 2.73$; 74.2% female, 25.1% male, 0.3% other, 0.4% missing). However, we also conducted additional analyses to examine the consequences of prosocial spending among the full sample of participants, including opt-outs (see below).

After the purchase, all participants were asked to report their current positive affect on the Positive and Negative Affect Schedule (PANAS; Watson, Clark, & Tellegen, 1988), which included the additional word “happy” (consistent with our more recent work, e.g., Aknin, Dunn, Sandstrom, & Norton, 2013; Aknin, Dunn, Whillans, Grant, & Norton, 2013; Aknin, Mayraz, & Helliwell, 2017; Whillans et al., 2019). As noted in the preregistration, positive affect was computed by taking the average of the 11 positive items (10 original positive affect items from the PANAS and happy), which served as the primary dependent variable of interest. In addition, participants were asked to report their positive and negative emotion on the Scale of Positive and Negative Experience (SPANE; Diener et al., 2009), which assessed both general and specific positive and negative states. Positive emotion on the SPANE served as a second and exploratory dependent variable; positive emotion was computed by summing together responses for all positive items. Finally, participants reported their demographic information (gender, age, and household income).

Hypotheses and Preregistered Analyses

In line with past research, we predicted that participants who purchased a goody bag for a sick child would report higher levels of positive affect than participants who purchased a goody bag for themselves when controlling for baseline happiness. We tested this preregistered directional hypothesis with an analysis of covariance (ANCOVA) in which spending condition (personal vs. prosocial spending) was entered as the independent variable, average post-spending positive affect was entered as the dependent variable, and baseline happiness was entered as a covariate. As predicted, par-

Participants who purchased a goody bag for a sick child reported higher positive affect ($M = 2.972$, $SD = .78$, $n = 344$) than participants who purchased a goody bag for themselves ($M = 2.732$, $SD = .69$, $n = 359$), $F(1, 700) = 22.767$, $p < .001$, $d = .36$, $r = .18$; see Table 2 for summary of all experiment results. Results are similar when we included participants in the prosocial spending condition who opted out of purchasing a goody bag; participants in the prosocial spending condition reported higher positive affect ($M = 2.945$, $SD = .79$, $n = 362$) than participants who purchased a goody bag for themselves ($M = 2.732$, $SD = .69$, $n = 359$), $F(1, 718) = 19.627$, $p < .001$, $d = .33$, $r = .16$.

We also conducted a secondary exploratory analysis to examine whether participants randomly assigned to purchase a goody bag for others reported higher levels of positive emotion on the SPANE than participants who purchase a goody bag for themselves when controlling for baseline happiness. We tested this hypothesis with an ANCOVA in which spending condition (personal vs. prosocial spending) was entered as the independent variable, average postspending positive emotion on the SPANE was entered as the dependent variable, and baseline happiness was entered as a covariate. Analyses revealed that participants who purchased a goody bag for a sick child reported higher positive emotion on the SPANE ($M = 21.028$, $SD = 5.29$, $n = 341$) than participants who purchased a goody bag for themselves ($M = 19.654$, $SD = 4.56$, $n = 358$), $F(1, 696) = 17.822$, $p < .001$, $d = .32$, $r = .16$. Results are similar when we included participants in the prosocial spending condition who opted out of purchasing a goody bag; participants in the prosocial spending condition reported higher positive emotion ($M = 20.784$, $SD = 5.40$, $n = 359$) than participants who purchased a goody bag for themselves ($M = 19.654$, $SD = 4.56$, $n = 358$), $F(1, 714) = 13.430$, $p < .001$, $d = .27$, $r = .14$.

Discussion

Experiment 1 provides clear evidence that participants felt happier after purchasing a goody bag for a sick child than after purchasing a goody bag for themselves, consistent with the hypothesis that spending money on others promotes happiness. We opted to use the goody bag paradigm from Aknin et al. (2013)

because it offers numerous methodological advances over the original paradigm used by Dunn and colleagues (2008) in which participants were assigned to spend \$5 or \$20 on themselves or others. Specifically, the goody bag paradigm requires that all participants purchase identical items (i.e., juice and/or treats). This consistency ensures that any emotional differences observed across conditions are not a result of purchasing differential content (e.g., experiences vs. material goods). In addition, because participants do not interact with the recipient of their gift and their choice is made privately, the benefits of prosocial spending cannot easily be explained by gratitude or praise.

Experiment 1 compared the immediate emotional consequences of personal and prosocial spending. In Experiment 2, we investigated the long-term outcomes of personal and prosocial spending by comparing how people felt when they reflected on a previous purchase made with their own money.

Experiment 2

Method

Sample. We recruited a final sample of 1,950 participants ($M_{\text{age}} = 47.72$, $SD = 15.19$; 68.7% female, 31.0% male, 0.3% other, 0.1% prefer not to say) using Qualtrics' online national panel (see Table 3 for complete sample demographics of participants in Experiments 2 and 3). This sample size was slightly higher than our target sample of 1,926 (calculated using G*Power with $\alpha = .05$, one-tailed, power = .95, and an average effect size of $d = .15$) because we oversampled slightly to exclude participants who did not recall a spending experience; this exclusion criterion is explained in our preregistration available on the OSF (<https://osf.io/x39cu/>). As shown in Table 1, past research using the prosocial spending recollection paradigm indicates an average effect size of $d = .20$, which requires a sample of 1,084 with $\alpha = .05$, one-tailed, and power = .95. However, given that published research may have overestimated the true effect and the costs of online data collection are relatively low, we were able to collect a larger sample enabling us to capture a true effect size of $d = .15$ with .95 power. This study was approved by our institutional review boards.

Table 2
Summary of Experimental Results

Experiment	n_{personal}	$n_{\text{prosocial}}$	$M_{\text{personal}} (SD)$	$M_{\text{prosocial}} (SD)$	F	p	η_p^2	Cohen's d	r
Experiment 1 ($N = 712$)									
Outcome									
Current PANAS	359	344	2.73 (.69)	2.97 (.78)	22.77	<.001	.03	.36	.18
Current SPANE	358	341	19.65 (4.56)	21.03 (5.29)	17.82	<.001	.02	.32	.16
Experiment 2 ($N = 1,950$)									
Outcome									
Current PANAS	983	963	3.08 (.91)	3.10 (.90)	0.457	.499	.00	.03	.02
Current SPANE	983	964	19.99 (5.93)	19.93 (6.01)	0.118	.732	.00	.02	.01
Experiment 3 ($N = 5,199$)									
Outcome									
Current PANAS	2,610	2,584	3.01 (.91)	3.04 (.90)	5.34	.010	.00	.06	.03
Current SPANE	2,613	2,584	19.49 (6.19)	19.64 (6.20)	4.57	.016	.00	.06	.03
Retrospective SPANE	2,613	2,584	21.90 (5.85)	22.69 (5.69)	37.93	<.001	.01	.17	.09

Note. PANAS = Positive and Negative Affect Schedule (Watson, Clark, & Tellegen, 1988); SPANE = Scale of Positive and Negative Experience (Diener et al., 2009).

Table 3
Summary of Sample Demographics (Recall Studies)

Demographic factor	Experiment 2 (N = 1,950)		Experiment 3 (N = 5,199)		Age	Experiment 2 (N = 1,950)		Experiment 3 (N = 5,199)	
	N	%	N	%		M (SD)	Mdn (range)	M (SD)	Mdn (range)
Race/ethnicity									
First Nation/Native American	21	1.1%	34	0.7%					
African American/Black	236	12.1%	443	8.5%					
Hispanic	140	7.2%	252	4.8%					
Caucasian/White	1419	72.8%	3998	76.9%					
Asian	77	3.9%	322	6.2%					
Middle Eastern	6	0.3%	11	0.2%					
Multi-racial	36	1.8%	103	2.0%					
Other	10	0.5%	16	0.3%					
Prefer not to answer	5	0.3%	18	0.3%					
Missing data	0	0.0%	2	0.0%					
Gender									
Male	605	31.0%	2205	42.4%					
Female	1339	68.7%	2962	57.0%					
Other	5	0.3%	22	0.4%					
Prefer not to answer	1	0.1%	8	0.2%					
Missing data	0	0.0%	2	0.0%					
					Annual pretax household income				
					Less than \$10,000	42.72 (15.19)	40.00 (14–90)	39.00 (11.77)	36.00 (18–90)
					\$10,000–\$19,999	N	%	N	%
					\$20,000–\$29,999	124	6.4%	223	4.3%
					\$30,000–\$39,999	167	8.6%	341	6.6%
					\$40,000–\$49,999	245	12.6%	526	10.1%
					\$50,000–\$59,999	222	11.4%	650	12.5%
					\$60,000–\$69,999	196	10.1%	529	10.2%
					\$70,000–\$79,999	186	9.5%	595	11.4%
					\$80,000–\$89,999	148	7.6%	443	8.5%
					\$90,000–\$99,999	128	6.6%	423	8.1%
					\$100,000–\$149,999	82	4.2%	281	5.4%
					More than \$150,000	105	5.4%	280	5.4%
					Prefer not to answer	219	11.2%	589	11.3%
					Missing data	113	5.8%	222	4.3%
						15	0.8%	95	1.8%
						0	0.0%	2	0.0%

Procedure. Participants were asked to report their baseline well-being using the same items and procedure described above. Specifically, participants reported their current level of happiness on a single-item (“Do you feel happy right now?”; from 1 = *not at all*, to 5 = *extremely*) and completed a measure of trait-level happiness (“In general, I consider myself . . .”; from 1 = *not a very happy person*, to 7 = *a very happy person*; Lyubomirsky & Lepper, 1999). As expected and specified in our preregistered analysis plan, these scores were highly correlated, $r(1,948) = .600$, $p < .001$, so we standardized and averaged them to create a baseline measure of happiness. Baseline happiness items were presented among a few filler questions (e.g., “How tired are you feeling right now?”) to disguise our interest in happiness.

After reporting their baseline happiness, participants were randomly assigned to recall and describe a previous spending experience in which they used \$20 to benefit themselves or someone else. Specifically, participants randomly assigned to the *personal spending condition* saw the prompt:

Please think back to and describe as vividly and in as much detail as possible the last time you spent approximately \$20 on yourself. In the space provided below please describe this event.

Participants randomly assigned to the *prosocial spending condition* saw the prompt:

Please think back to and describe as vividly and in as much detail as possible the last time you spent approximately \$20 on someone else. In the space provided below please describe this event.

All participants then reported their current positive affect on the PANAS (Watson et al., 1988) and the additional word happy (as in Experiment 1). Positive affect was computed by taking the average of the 11 positive items (10 original positive affect items from the PANAS and happy); this served as the primary dependent variable of interest. As in Experiment 1, participants were also asked to report their positive and negative emotion on the SPANE (Diener et al., 2009). Positive emotion on the SPANE served as a second and exploratory dependent variable; positive emotion was computed by summing together responses for all positive items. Finally, participants reported their demographic information (gender, age, and household income).

Although we originally used the Subjective Happiness Scale (Lyubomirsky & Lepper, 1999)—a relatively trait like measure of happiness—our more recent work and current best practice suggests that measuring state happiness is preferred. As such, we measured current positive affect and positive emotion as our dependent variables.

Hypotheses and Preregistered Analyses

In line with past research, we predicted that participants randomly assigned to reflect upon a previous instance of prosocial spending would report higher current levels of positive affect than participants assigned to reflect upon a previous instance of personal spending. As outlined in our preregistered analysis plan, we tested this directional hypothesis with an ANCOVA in which spending condition (personal vs. prosocial spending) was entered as the independent variable, average levels of current positive affect reported on the PANAS were entered as the dependent variable, and baseline happiness was entered as a covariate. Con-

trary to our predictions and past work, participants in the prosocial spending condition did not report higher levels of current positive affect ($M = 3.104$, $SD = .90$, $n = 963$) than participants in the personal spending condition ($M = 3.083$, $SD = .91$, $n = 983$), $F(1, 1943) = .457$, $p = .499$, $d = .03$, $r = .02$.

As in Experiment 1, we conducted a second exploratory analysis to examine whether participants randomly assigned to recall spending money on others reported higher levels of current positive emotion on the SPANE than participants assigned to recall spending money on something for themselves. Consistent with the preregistered analysis plan, we tested this hypothesis with an ANCOVA in which spending recall condition (personal vs. prosocial spending) was entered as the independent variable, current postrecall positive emotion on the SPANE was entered as the dependent variable, and baseline happiness was entered as a covariate. Analyses revealed that participants in the prosocial spending condition did not report higher levels of positive emotion on the SPANE ($M = 19.925$, $SD = 6.01$, $n = 964$) than participants in the personal spending condition ($M = 19.986$, $SD = 5.93$, $n = 983$), $F(1, 1944) = .118$, $p = .732$, $d = .02$, $r = .01$.

Discussion

Experiment 2 did not replicate the long-term emotional rewards of prosocial (vs. personal) spending with the recollection paradigm. Specifically, participants assigned to recall a time they spent approximately \$20 on someone else reported similar happiness levels in the current moment to participants assigned to recall a time they spent approximately \$20 on themselves. One reason for the null effect may be that many participants did not provide detailed spending recollections despite the request to describe their purchase “as vividly and in as much detail as possible.” Indeed, participants in Experiment 2 wrote an average of 22 words in response to the recollection prompts, which is less than half the length of responses captured in a recent study demonstrating the emotional benefits of prosocial spending upon reflection (an average of 45 words in Hanniball et al., 2019), signaling a possible lack of engagement in the task. Whereas participants in our past studies have offered vivid descriptions of their experiences, many participants in the present study wrote just a brief phrase, such as “as a birthday gift” (in the prosocial spending condition) or “buy coffee” (in the personal spending condition).

In interpreting the results of Experiment 2, it is also worth noting that we measured how participants *felt in the present* after recalling a past spending experience. Theoretically, recalling a positive past experience should only make people feel happy in the present if they vividly recall it, akin to mentally reliving the experience (Strack, Schwarz, & Gschneidinger, 1985). Indeed, studies on buying experiences (vs. material things) typically ask participants to recall how they felt at the time of the purchase, rather than asking about their current feelings, which bypasses the problem of having to get participants to vividly relive the past event.

Therefore, in Experiment 3 we conducted an additional test of the recollection paradigm, using the same design as Experiment 2 with three small but important methodological improvements. First, to ensure that participants engaged with the open-ended recollection task, we recruited only MTurk participants who had provided high-quality responses in past studies. Second, to encour-

age participants to engage in vivid reflection on this task, we required them to write at least 150 characters when describing their spending experience. Finally, as well as measuring participants' current feelings (as in Experiment 2), participants also recalled their feelings at the time of the purchase; this measurement strategy was not used in the original work and, therefore, does not represent a replication. However, we included this measure because it allowed us to examine the emotional benefits of prosocial spending even for participants who did not vividly relive the past experience. To examine the potential value of these methodological improvements, we conducted an exploratory study incorporating these changes (materials and data for this exploratory study are available on the OSF at <https://osf.io/d6ymu/>). After obtaining promising results in this exploratory study, we conducted a third preregistered study in Experiment 3.

Experiment 3

Method

Sample. We recruited a final sample of 5,199 participants ($M_{\text{age}} = 39.00$, $SD = 11.77$; 57.0% female, 42.4% male, 0.4% other, 0.04% prefer not to say) online using MTurk system. We used MTurk as our recruitment platform because it gave us the ability to reach respondents with a high-quality record (e.g., 97% approval rating in >5,000 HITs), increasing the likelihood that participants would engage with the detailed recollection task. Our target sample was 5050 participants. This target was based on G*Power calculations using $\alpha = .05$, one-tailed, power = .80, and an effect size of $d = .07$ (as observed in the identical exploratory study mentioned above). However, we oversampled to 5,300 participants so we could exclude those who did not recall a spending experience; this exclusion criterion is explained in our preregistration available on the OSF at <https://osf.io/y9s5f/>. This study was approved by our institutional review boards.

Procedure. Participants reported their current happiness and trait happiness on the same single-item scales used in Experiments 1 and 2. Consistent with our earlier studies and our preregistered analysis plan, these scores were highly correlated, $r(5,197) = .688$, $p < .001$, so we standardized and averaged them to create a baseline measure of happiness. Happiness items were presented among a few filler questions (e.g., “How tired are you feeling right now?”) to disguise our interest in happiness.

Participants were randomly assigned to recall and describe a previous spending experience in which they used \$20 to benefit themselves or someone else. Recollection prompts were identical to those used in Experiment 2; however, participants were required to write at least 150 characters (equivalent to approximately 25 words) when describing their spending experience to encourage vivid recollection. Most participants wrote more than the minimum requirement; the average spending recollection contained 60.52 words ($SD = 33.23$).

After describing their spending memory, all participants reported their *current* well-being on the PANAS and SPANE (as before), as well their positive emotion *at the time of purchase* using the SPANE. Consistent with the previous studies and our preregistration, positive affect was computed by taking the average of the 11 positive items (10 original positive affect items from the PANAS and happy) and positive emotion on the SPANE was

computed by summing together responses for all positive items. Finally, participants reported their demographic information (gender, age, and household income).

Hypotheses and Preregistered Analyses

We predicted that participants randomly assigned to reflect upon a previous instance of prosocial spending would report higher current levels of positive affect than participants assigned to reflect upon a previous instance of personal spending. As outlined in our preregistered analysis plan, we tested this directional hypothesis with an ANCOVA in which spending condition (personal vs. prosocial spending) was entered as the independent variable, average levels of current positive affect reported on the PANAS were entered as the dependent variable, and baseline happiness was entered as a covariate. Consistent with predictions and past work, participants in the prosocial spending condition reported higher levels of current positive affect ($M = 3.038$, $SD = .90$, $n = 2,584$) than participants in the personal spending condition ($M = 3.006$, $SD = .91$, $n = 2,610$), $F(1, 5191) = 5.341$, $p = .011$, $d = .06$, $r = .03$.

We also examined whether participants randomly assigned to recall spending money on others reported higher levels of current positive emotion on the SPANE than participants assigned to recall spending money on something for themselves. Following our preregistered analysis plan, we tested this directional hypothesis using an ANCOVA in which spending recall condition (personal vs. prosocial spending) was entered as the independent variable, current postrecall positive emotion on the SPANE was entered as the dependent variable, and baseline happiness was entered as a covariate. As predicted, participants in the prosocial spending condition reported higher levels of positive emotion on the SPANE ($M = 19.638$, $SD = 6.20$, $n = 2,584$) than participants in the personal spending condition ($M = 19.491$, $SD = 6.19$, $n = 2,613$), $F(1, 5194) = 4.571$, $p = .017$, $d = .06$, $r = .03$.

Finally, we examined whether participants randomly assigned to recall spending money on others remember having felt happier after spending than participants assigned to recall spending money on themselves. As stated in the preregistration, we tested this directional hypothesis using an ANCOVA in which spending recall condition (personal vs. prosocial spending) was entered as the independent variable, ratings of positive emotion on the SPANE at the time of purchase were entered as the dependent variable, and baseline happiness was entered as a covariate. As predicted, participants recalled experiencing higher levels of positive emotion (on the SPANE) after spending money on others ($M = 22.696$, $SD = 5.69$, $n = 2,584$) than after spending money on themselves ($M = 21.901$, $SD = 5.85$, $n = 2,613$), $F(1, 5194) = 37.926$, $p < .001$, $d = .17$, $r = .09$.

Discussion

Using a modified version of our recollection paradigm that was designed to ensure participants exhibited at least a moderate level of engagement with the task, we replicated the emotional rewards of prosocial (vs. personal) spending. On both measures (the PANAS and the SPANE), participants reported more positive feelings when they reflected on a time they spent their own money on others versus themselves. However, this effect was very small

($d = .06$). We also asked participants to recall how they had felt at the time of their purchase, which revealed a larger positive effect of prosocial spending ($d = .17$). The greater difference between conditions reported at the time of purchase may reflect the more immediate outcomes of personal and prosocial spending choices, but may also stem from recollection biases or demand characteristics. That is, asking participants to recall their past emotions may have led them to rely on their naïve theories of how giving *should* make them feel, or their guesses of how the experimenters expected them to feel.

General Discussion

This Registered Replication Report offers evidence that spending money on others promotes happiness. Experiment 1 showed that university students randomly assigned to purchase treats for a sick child reported feeling happier afterward than participants assigned to purchase treats for themselves. In Experiment 2, however, American adults recruited through Qualtrics did not report greater current happiness when they thought about spending their own money on others (vs. themselves) in the past. We suspected that these null results might have stemmed from a lack of participant engagement and, thus, we made several modifications to enhance engagement with the task in Experiment 3. Using this improved recollection paradigm, we found that participants in the prosocial (vs. personal) spending condition reported slightly higher happiness after recalling a time they spent money on others versus themselves. We observed a similar, but larger difference between conditions when participants recalled their feelings immediately after making the purchase. Given that these studies were preregistered and well-powered, they provide the most conclusive evidence to date for the emotional benefits of prosocial spending.

The present studies also offer the best available estimates of effect sizes to guide future experimental research on prosocial spending. In Experiment 1, we observed an effect size ($d = .36$, $r = .18$) that was similar in magnitude to other well-established effects in social psychology—such as the foot-in-the-door effect (average $r = .16$) and the effect of self-disclosure on liking (average $r = .16$; Richard, Bond, & Stokes-Zoota, 2003)—as well as the typical effect reported in the published social psychology literature more broadly ($r = .21$; Fraley & Marks, 2007). While we observed null effects using a recollection paradigm in Experiment 2, the modified recollection paradigm used in Experiment 3 yielded significant, but small effects on participants' happiness in the moment after recalling a past spending experience ($d = .06$, $r = .03$), and a larger effect on participants' recall of their happiness immediately after making the purchase ($d = .17$, $r = .09$). The latter effect is similar in magnitude to other well-known effects, such as the tendency to attribute failure to external factors (average $r = .09$) and the tendency for people to be more aggressive toward men than women (average $r = .06$; Richard et al., 2003).

As this discussion underscores, there is no one effect size of prosocial spending on happiness. Instead, the magnitude of the effect critically depends on methodological choices, including the experimental paradigm used and the manner in which happiness is assessed. More generally, the search for a single effect size to capture any phenomenon is challenging for at least two reasons. First, the simple fact that moderators increase and decrease the

magnitude of an effect suggests that any phenomenon is inherently variable. In the context of prosocial spending, for example, factors such as perceived prosocial impact, a sense of volition, and connection with recipients critically influence the extent to which spending money on others promotes happiness (Aknin et al., 2013, 2013; Dunn, Aknin, & Norton, 2014; Lok & Dunn, 2019; Weinstein & Ryan, 2010). Second, broader contextual factors are also likely to influence the observed effect size of a psychological phenomenon. Our correlational research using the Gallup World Poll data provides a clear example; while the effect of prosocial spending on life satisfaction is positive in over 88% of countries surveyed, the correlation ranges from 1.33 (in Montenegro) to $-.63$ (in Tunisia; Aknin et al., 2013).

Perhaps most importantly, our results suggest that at least a moderate level of participant engagement may be necessary to detect the beneficial effects of prosocial spending. Traditionally, psychologists from Milgram (1975) to Batson (e.g., Toi & Batson, 1982) to Latané and Darley (1968) conducted elegant, high-impact experiments that immersed participants in the phenomenon under study. More recently, in a well-justified effort to collect much larger samples in less time, social psychologists have increasingly conducted studies online. Our findings point to the conclusion that slow, costly lab studies still have an important place in social psychology; using a highly involved lab paradigm in Experiment 1, we were able to detect a robust effect of prosocial spending on happiness, which required enormous samples to detect using a more pallid online procedure. Of course, it is possible to design highly engaging experiments that can be completed online (e.g., O'Brien & Kassirer, 2019, Experiment 2) and, thus, an important—and perhaps underrecognized—challenge for psychologists lies in importing the strengths of traditional high-impact labs studies into modern online data collection platforms.

It is also important to consider how these effects might shift over time. Taken together, our studies suggest that people experience a clear happiness benefit immediately after spending on others. When people look back on a past prosocial spending experience, they remember feeling happy—and just thinking about this past experience produces a detectable, though very small boost to happiness in the present.¹ Once again, it is worth noting, that original research on prosocial spending did not ask participants to report how they felt immediately after their purchase (we imported this paradigm from the experiential vs. material spending literature; e.g., Carter & Gilovich, 2010), so this finding should not be considered a direct replication of past work.

That said, while the warm glow of any one generous purchase appears to fade over time, daily life offers numerous opportunities to spend money on others. Funder and Ozer (2019) argue that rather than apologizing for small effects, researchers should consider how these small effects may accumulate over time. For example, someone who spends small amounts of money helping others each day for a week may get a small happiness boost each time, which may compound into a larger benefit for well-being. Although it is also possible that people may habituate to repeated behaviors, recent research suggests that people adapt more slowly to treating others than to treating themselves (O'Brien & Kassirer, 2019). Consistent with the notion that small boosts in happiness from prosocial spending may aggregate into greater well-being, recent analyses of correlational data from over a million people around the globe show that prosocial spending—in the form of

charitable giving—is one of the top six predictors of life satisfaction around the world (Helliwell, Huang, & Wang, 2019).

Finally, our hope is that this research—and our learning along the way about the importance of key factors for consideration such as engagement—serves as a model for other researchers interested in examining the robustness of their previously published findings. While daunting, we found it immensely valuable to reflect upon our past research in selecting the paradigms we felt were most important to replicate, while being transparent about our selection criteria and our methodological choices. New norms of transparency and rigor are already improving psychological science; our results suggest that revisiting old paradigms with higher research standards are one such tool to ensure that our science rests on a solid foundation.

¹ It is worth noting that another recent experiment found that simply recalling acts of kindness led to well-being benefits that were comparable with the effects of actually engaging in acts of kindness (Ko, Margolis, Revord, & Lyubomirsky, 2019). This online study was conducted with university students, who were required to spend at least 5 min on the recall task, further underscoring the potential importance of promoting participants' engagement.

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