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# Healing through helping: an experimental investigation of kindness, social activities, and reappraisal as well-being interventions

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**ARTICLE HISTORY**
Received 26 February 2022
Accepted 22 November 2022

**KEYWORDS**
Kindness; social connection; positive psychology; CBT; well-being; randomized trial; anxiety; depression

## Introduction

### *The importance of social connection to well-being*

Social connection has been defined as an internal sense of belonging and interpersonal closeness with other individuals and groups (Lee & Robbins, 1998). Social connection is a core feature of many prominent theories of well-being (e.g., Ryff, 1995), and empirical evidence suggests this is for a good reason. For example, Diener and Seligman (2002) found evidence of the importance of social connection in their investigation of the lives of 'very happy people,' defined as undergraduates who scored in the top 10% of a composite of well-being measures. Diener and Seligman found the trait that most clearly separated the very happy group from their less happy peers was their high degree of social connection. The very happy group reported the most frequent social interactions and reported the strongest quality romantic, friend, and family relationships. Importantly, the very happy group did *not* differ from their less happy peers in wealth, number of negative life events, GPA, or physical attractiveness – suggesting that it was strong social connection that was the key predictor of well-being. Similar findings on the importance of social connection have been reported by other researchers (e.g., Holt-Lunstad et al., 2015; Rohrer et al., 2018).

Social connection has important implications for the well-being of individuals from clinical populations as well. For example, Saris et al. (2017) conducted a longitudinal investigation of social functioning in a sample composed of patients with anxiety and/or depressive disorders and healthy controls. They found patients with these disorders had elevated social dysfunction on a variety of measures. Furthermore, measures of social dysfunction remained pronounced even amongst patients whose symptoms had gone into remission. These results imply that social connection may be an important treatment target for anxious or depressed individuals. To date, cognitive-behavioral therapy (CBT) has been one of the primary treatments for anxiety and depression. However, evidence suggests traditional CBT approaches may have a relatively small effect on social connectedness among those with anxiety or depressive disorders.

For example, Hofmann et al. (2014) conducted a meta-analysis examining the effect of CBT on quality of life for adults diagnosed with anxiety disorders. Examining the pre-to-post (within-group) effect sizes, they found CBT had a large effect on anxiety symptoms ($g = 0.95$, 95% CI = 0.80–1.10), but a relatively weak effect on the social domain of the World Health Organization Quality of Life Assessment (WHOQOL-BREF, $g = 0.24$, 95% CI = 0.15–0.32). Hofmann et al.'s results suggest

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that, on average, CBT is relatively ineffective at improving social quality of life among those with anxiety disorders, although it is quite efficacious for reducing anxiety symptoms. Similarly, Renner et al. (2014) found in their meta-analysis that psychotherapy (including CBT) had a medium-to-large effect on depression symptoms, but only a small-to-medium effect on social functioning. Although Hofmann et al.'s (2014) and Renner et al.'s (2014) studies did not include measures of social connection per se, the constructs of social quality of life and social functioning used in these studies are closely related to the construct of social connection (Maitland et al., 2016), and explicit measures of social connection have been found to be highly correlated with measures of social functioning and social quality of life. For example, the social domain of the WHOOL-BREF measure included in Hofmann et al. (2014) has been found to be strongly correlated with the Social Connectedness Scale-Revised (SCS-R, Lee et al., 2001) at a magnitude of $r = .64$ (Flack et al., 2021).

### *Acts of kindness: a pathway to improving social connection?*

Given that prevailing CBT techniques may not be especially effective at improving social connection, it is important to explore alternative treatment approaches. One promising candidate for improving social connection and related dimensions of well-being is acts of kindness. Acts of kindness can be defined as actions that (1) benefit others or make others happy and (2) typically involve some cost to oneself (e.g., Alden & Trew, 2013). Several experimental studies have demonstrated that performing acts of kindness promotes well-being.

For example, Kerr et al. (2015) recruited 48 clients on a waitlist for outpatient psychological treatment. Clients presented with a variety of problems including depression, anxiety, relationship difficulties, post-traumatic stress, substance use, and eating disorders. Clients were randomly assigned to three activities for a 14-day period: (1) a control group (daily mood monitoring); (2) a kind acts group (one act of kindness a day); or (3) a gratitude list group (keeping a daily list of things for which one is grateful). All groups reported comparable reductions on the Outcome Questionnaire-45.2 (a measure of general psychiatric symptoms) and the stress scale of the Depression Anxiety and Stress Scales 21-item version (DASS-21). However, only the prosocial behavior conditions (i.e., the kind acts and gratitude groups) reported significant reductions in anxiety symptoms, whereas the control group showed no significant change. Additionally, the kind acts and gratitude groups reported greater increases in life satisfaction than the control group. Critically for the current study, engaging in kind acts and expressing gratitude led to greater ratings of social connection than the control task ($d = 1.27$ for kind acts versus control and $d = 2.07$ for gratitude versus control). However, the short duration and lack of a long-term follow-up assessment in this study are important limitations to address.

Perhaps the most compelling evidence for the clinical utility of acts of kindness comes from a study by Alden and Trew (2013). They recruited undergraduates with elevated social anxiety symptoms, who were then randomly assigned to three conditions: (1) a control group (recording life events); (2) a kind acts group; and (3) a behavioral experiments group. In the behavioral experiments condition, participants engaged in an exposure-based procedure to eliminate safety behaviors, a technique that is often used in CBT for social anxiety disorder (e.g., Clark et al., 2006). All groups reported comparable improvements in negative affect (NA), anxiety symptoms, and approach goals (motivation to approach social interactions). However, between-group differences were found for positive affect (PA), relationship satisfaction (RS), and social avoidance goals (SAG, i.e., motivation to avoid social interactions). Specifically, only the kind acts group reported significant improvements on these three measures. Furthermore, these group differences were maintained after controlling for potential confounds such as compliance with the activity, frequency of social interaction, and baseline depressive symptoms. Altogether, the results of this study suggest that acts of kindness may perform as well as behavioral experiments for treating social anxiety, and acts of kindness may even improve social and positive affect outcomes (i.e., PA, SAG, and RS) better than behavioral experiments. However, like Kerr et al. (2015), an important limitation of this study is the lack of a follow-up assessment. Additionally, the behavioral experiments technique used in this study is a staple of treating anxiety disorders, but it is not typically used for depression. Furthermore, this technique is primarily behavioral in nature, but there was no comparison in this study to a cognitively-oriented CBT technique. Therefore, in the current study we included CBT-based techniques for comparison to acts of kindness that are used for both depression and anxiety and that represent both the cognitive and behavioral traditions of CBT.

### *Self-focused attention as a mechanism of change for acts of kindness*

Consistent with the emerging process-based model of intervention science (Hofmann & Hayes, 2019), it is

important to determine not only whether an intervention is effective, but *why* it is effective. Therefore, we were also interested in investigating a mechanism of change for acts of kindness. One such potential mechanism is self-focused attention.

Prior theorists have argued that when individuals experience negative affective states – such as anxiety or depression – there is a 'self-regulatory cycle' in which repeated, failed attempts to reduce negative emotions result in a chronic preoccupation with one's own suffering (Pyszczynski & Greenberg, 1987). This chronic self-focus may then exacerbate difficulties with social functioning, as interactions with individuals who are depressed or anxious may be perceived as excessively self-referential (Jacobson & Anderson, 1982) or awkward and unpleasant (for reviews, see Hames et al., 2013; Segrin, 2000). Additionally, researchers have found that self-focused attention regarding negative information about oneself is correlated with a variety of indices of emotional distress (Mor & Winquist, 2002).

Given that self-focused attention is linked with emotional distress and impaired social functioning, reducing self-focused attention may result in subsequent improvements in social connection and well-being. Performing acts of kindness likely requires a shift in attention away from oneself and onto the needs of other people, at least in the moment. Therefore, completing acts of kindness may reduce self-focused attention, which in turn may drive improvements in social connection and other dimensions of well-being. Accordingly, in the current study we included a measure of self-focused attention (the Self-Absorption Scale) as a potential mediator of the effects of acts of kindness.

## The current study

The purpose of the current study was to evaluate the impact of acts of kindness on social connection and other dimensions of well-being. We placed an emphasis on social connection due to its strong associations with multiple well-being measures, as well as the evidence suggesting that CBT techniques may be less effective at improving social connection. Our study was designed to address four gaps in the literature:

(1) With the two exceptions reported above (Alden & Trew, 2013; Kerr et al., 2015), the majority of studies on acts of kindness have been conducted with samples that did not have clinically significant symptoms. Therefore, there is still a limited evidence base to demonstrate the benefits of acts of kindness as a clinical technique. The rationale for testing acts of kindness is especially strong for individuals with anxiety or depression symptoms, as social connection is often impaired in individuals with these symptoms and it is predictive of the course of symptoms (Saris et al., 2017). Therefore, we recruited a sample with elevated anxiety and/or depression symptoms, and we included a five-week follow-up period for all conditions to track the trajectory of symptoms after the intervention.

(2) To determine whether acts of kindness has advantages over prevailing CBT techniques, it is important to test acts of kindness against CBT techniques that are used broadly for anxiety *and* depression symptoms to maximize the generalizability of the results. Furthermore, we were interested in comparing acts of kindness to both cognitive and behaviorally-oriented CBT techniques to represent both major domains of CBT strategies. Thought records are an example of one evidence-based CBT technique that is primarily cognitive in orientation (Beck, 2011; McManus et al., 2012; Persons & Burns, 1985). Thought records are used to guide individuals through a process of cognitive reappraisal, which involves modifying maladaptive thought patterns and replacing them with more realistic and flexible thoughts. Thought records are a core component of many CBT practices. For example, in a survey of 816 CBT practitioners accredited by the British Association for Behavioural and Cognitive Psychotherapies, 98% reported they use thought records when conducting CBT, and on average practitioners rated thought records as among the top five most effective CBT techniques (Tallon et al., 2019). Thought records are also a prominent technique in CBT-based self-help programs, such as *Mind Over Mood* (Greenberger & Padesky, 1995), thus making them an ideal comparison condition for our study. Accordingly, in the current study, we included a condition labeled cognitive reappraisal in which participants completed thought records.

(3) A technique representing the behavioral tradition of CBT is behavioral activation, which has long been used as an evidence-based treatment, particularly for depression (Jacobson et al., 2006). Like thought records, behavioral activation is used by 99% of accredited CBT practitioners, and on average it was also rated as among the top five most effective CBT techniques (Tallon et al., 2019). A large component of behavioral activation involves planning pleasurable social interactions with others. Because performing acts of kindness necessarily involves social interaction, it is important to differentiate the effects of general social interaction from prosocial behavior. In other words, previous research has not ruled out the alternative explanation that the benefits of acts of kindness are due to general social interaction (behavioral activation) as opposed to acts of kindness per se. To rule out this possibility, we included a social activities task as a second comparison condition, thus

isolating the effects of acts of kindness and providing a more behaviorally-oriented contrast for acts of kindness.

(4) Finally, the mechanisms of the effects of acts of kindness are unknown. There is evidence that self-focused attention exacerbates mental health difficulties, and acts of kindness may reduce self-focused attention. Therefore, in the current study we included repeated measurements of self-focused attention (using the Self-Absorption Scale) as a mediator of change for acts of kindness.

### *Hypotheses*

To address these gaps in the literature, we made the following hypotheses prior to analyzing data:

H1: Based on previous literature (e.g., Nelson et al., 2016), we predicted participants in all conditions would demonstrate comparable reductions in anxiety and depression symptoms and NA over time, with predicted pre-to-post within-group effect sizes of at least medium magnitude ($d \geq 0.50$) and pre-to-follow-up effect sizes of at least a small magnitude ($d \geq .20$).

H2: We predicted there would be significant between-group contrasts for measures of positive well-being: social connection, PA, and life satisfaction. We predicted that the acts of kindness group would show greater improvements on these three measures than the cognitive reappraisal and social activities groups. Because we expected all three conditions to improve these outcomes to some degree, we expected the between-group effect sizes to be of small-to-medium magnitude ($d \approx .20 - .50$), with the largest difference occurring between acts of kindness and cognitive reappraisal. On an exploratory basis, we also compared social activities to cognitive reappraisal.

H3: We predicted that deviations in self-absorption would mediate group differences on social connection, PA, and life satisfaction over time (see Figure 1 for a conceptual diagram of the analyses conducted). That is, we predicted the acts of kindness group would demonstrate a greater reduction in self-absorption over time than cognitive reappraisal and social activities, and deviations in self-absorption would predict subsequent values of social connection, PA, and life satisfaction. Although we did not expect to find between-group differences for NA or depression/anxiety symptoms (Hypothesis 1), we nevertheless tested self-absorption as a mediator for these outcomes as well if a significant between-group difference was found.

## Methods

### *Participants*

Participants were adults from a large midwestern city and undergraduate students enrolled in an introductory psychology course at a large midwestern university. Undergraduate psychology students were given partial

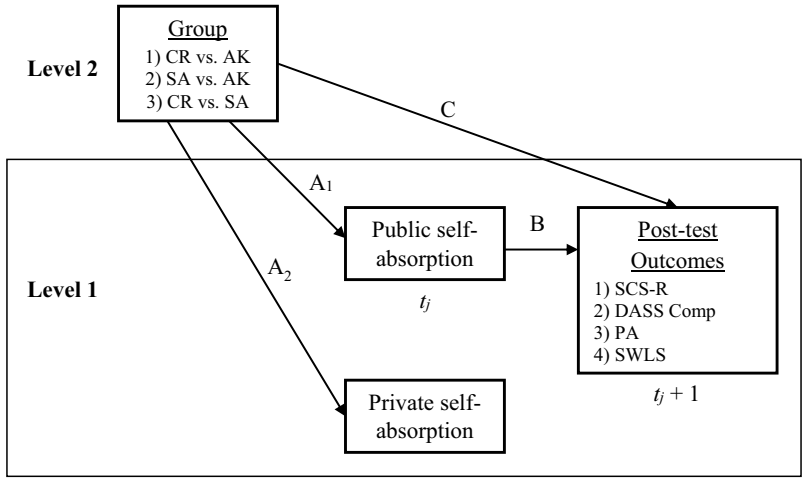


**Figure 1.** Model Depicting a 2-1-1 Multilevel Mediation, in which Group Differences in Post-test Outcomes Over Time are Mediated by Deviations in Self-absorption. Note. This figure is a conceptual representation of the multilevel mediation analyses, not a statistical diagram. Each path of this model was estimated independently of one another. Only variables that significantly differed between groups at post-test (path C) were included in the mediation analyses. Only public self-absorption significantly differed between groups (path $A_1$). Therefore, only public self-absorption was tested as a mediator of outcomes (path B), whereas private self-absorption was excluded from this leg of the model. Path B (the pathway from public self-absorption to the outcomes) represents a time-lagged analysis in which deviations in public self-absorption for person *j* at time *t* predict subsequent values of the outcomes for person *j* at time $t + 1$. CR = Cognitive reappraisal, SA = social activities, AK = acts of kindness.

course credit for participation. Community members were paid $10 in Amazon gift cards for approximately each hour they spent completing study measures.

Participants were excluded if they were younger than 18 years old or did not meet at least one of the three cutoffs for a mild severity of anxiety, depression, or stress[1] symptoms on the DASS-21 according to the thresholds recommended by the test developers (Lovibond & Lovibond, 1995; see thresholds in Measures section below). We did not use any other exclusion criteria. All study procedures were approved by the university's Institutional Review Board.

Three-hundred and fifty-five individuals completed the DASS-21 pre-screening survey, of which 266 individuals were eligible for the study. Eligible participants were contacted to schedule a baseline session in the laboratory. One-hundred and twenty-six individuals initiated a baseline session. Of these 126 participants, two were excluded from participating due to failing embedded attention check items on baseline materials, and two individuals elected to withdraw their participation in the study due to (1) developing a serious illness and (2) a family emergency. The remaining 122 participants were all randomly allocated to conditions at baseline. See Figure 2 for group allocation by condition and the flow of participants through the study. Data from all 122 baseline participants were included in the final sample.

See Table 1 for baseline sample characteristics. The final sample mostly consisted of individuals who were paid community participants (58%), female (76%), White (62%), and who had at least some college education (84%) and no religious affiliation (57%). There was a substantial minority of Asian participants (17%), followed by equal numbers of Black (7%), Hispanic or Latino/Latina (7%), and Multiracial (7%) participants. Participants ranged in age from 18–78 ($M$ = 24.70, $SD$ = 10.40). Eighty participants (66% of the sample) reported a history of some form of mental health treatment, and 30 participants (25% of the sample) reported

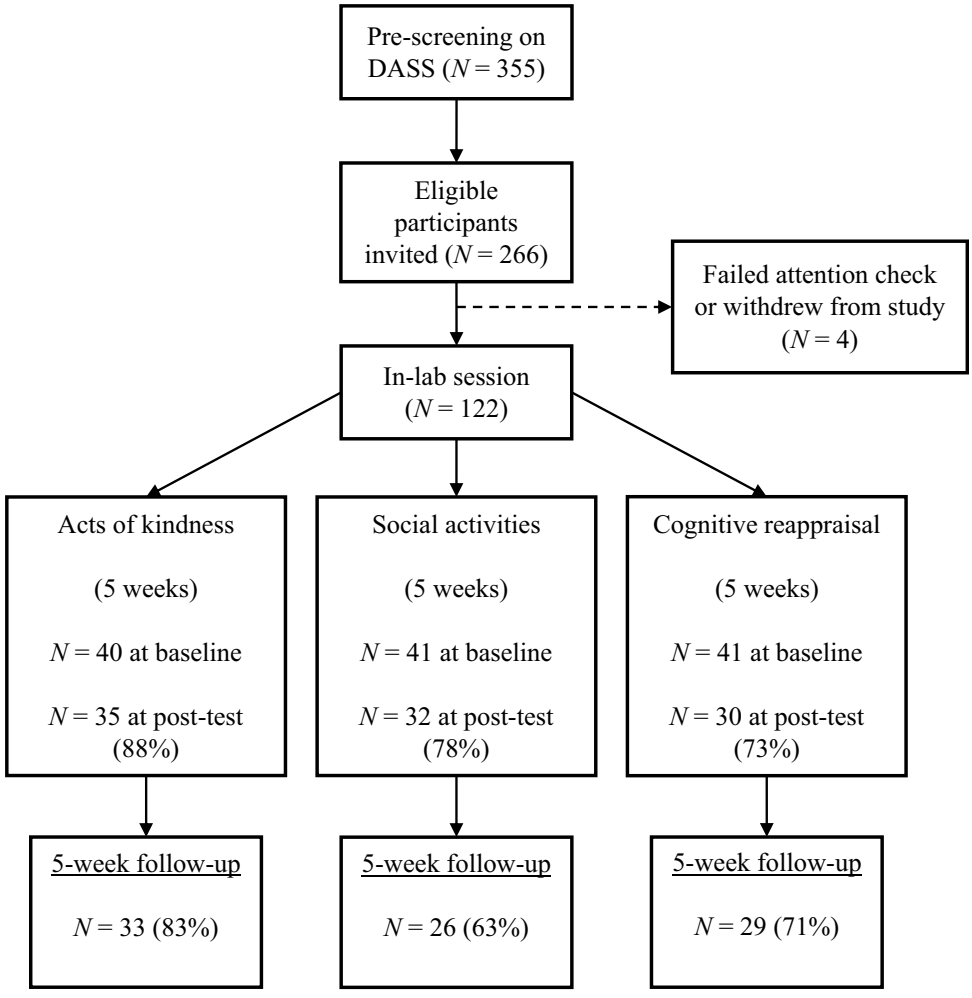


**Figure 2.** Random Allocation to Experimental Conditions and Flow of Participants Through the Study. Percentages are the portion of the original sample remaining from baseline.

**Table 1.** Baseline Sample Characteristics.

| Variable | Full Sample ($N$ = 122) |
|---|---|
| Sex: *n* (% female) | 93 (76) |
| Age: *M* (*SD*) | 24.7 (10.4) |
| Race: *n* (% White) | 75 (62) |
| Income: mode (%) | Over $100,000 (34) |
| Not employed: *n* (%) | 7 (6) |
| Any college education: *n* (%) | 103 (84) |
| Current college student: *n* (%) | 51 (42) |
| Married or cohabiting: *n* (%) | 18 (15) |
| Any religious affiliation: *n* (%) | 53 (43) |
| Current mental health treatment: *n* (%) | 30 (25) |
| History of any mental health treatment: *n* (%) | 80 (66) |
| Estimated number of friends: mode (%) | 2–5 (34) |
| Estimated size of social network: mode (%) | More than 20 (33) |
| Hours spent with others in last week: *M* (*SD*) | 25.3 (45.6) |
| COVID stay-at-home order: *n* (%) | 24 (20) |
| DASS depression: *M* (*SD*) | 16.0 (10.3) |
| DASS anxiety: *M* (*SD*) | 12.2 (8.5) |
| DASS stress: *M* (*SD*) | 19.2 (8.5) |
| MSPSS: *M* (*SD*) | 5.0 (1.2) |

Note. No differences were found between conditions for any variables (all *p*s > .12). Estimated size of social network = number of individuals with whom the participant has regular contact with. COVID stay-at-home = individuals who participated in the active phase of the study (the first 5 weeks) after the stay-at-home order went into effect.

currently receiving some form of mental health treatment.[2] According to the DASS-21 thresholds, on average participants reported a medium severity of baseline depression ($M$ = 16.00, $SD$ = 10.30), anxiety ($M$ = 12.20, $SD$ = 8.50) and stress ($M$ = 19.20, $SD$ = 8.5) symptoms. Notably, 24% of the sample met criteria for severe depression symptoms (a score of ≥ 21), 37% met criteria for severe anxiety symptoms (≥ 15), and 24% met criteria for severe stress symptoms (≥ 26).

Late in the stages of data collection, the global COVID-19 pandemic resulted in a stay-at-home order and shutdown of all on-campus research activities in March 2020. All 122 participants included at baseline initiated the study under normal (pre-COVID) conditions. However, a minority of participants ($N$ = 24, or 20% of the sample) completed some portion of the five-week intervention during the stay-at-home order. For these individuals, instructions were modified slightly to ensure compliance with safety guidelines (e.g., maintaining six feet of social distancing, instructions to plan social activities among members of one's own residence). We controlled for effects of the COVID shutdown in all analyses reported below.

## Measures

### *Depression Anxiety And Stress Scales 21-item version (DASS-21;* Lovibond & Lovibond, 1995)

The DASS-21 is a 21-item scale that measures the emotional states of depression, anxiety, and stress. It is composed of three subscales (one for each emotional state), each with seven items rated on a four-point Likert-type scale. Participants rate the extent to which statements apply to them over the past week. Total scores are summed from each subscale and multiplied by two to be comparable to the norms developed for the full 42-item scale, with higher scores indicating greater severity. The developers recommended the following values for detecting at least a 'mild' severity of symptoms (corresponding to the 78$^{th}$ percentile, or 0.5 *SD*s above the mean of the normative sample): ≥ 10 for depression, ≥ 8 for anxiety, and ≥ 15 for stress. In the current sample, internal consistency at baseline for the three subscales ranged from acceptable to good: depression α = .88; anxiety α = .77; stress α = .78. For the composite measure that we created with all three subscales (DASS Comp; see Data Analytic Plan below), internal consistency at baseline was acceptable (α = .74).

### *Multidimensional Scale of Perceived Social Support (MSPSS;* Zimet et al., 1988)

The MSPSS is a 12-item measure that assesses perceived social support from three major sources: friends, family, and a significant other. Items are rated with a seven-point Likert-type scale, with higher scores indicating a higher level of perceived support. Internal consistency at baseline for the current sample was excellent (α = .91).

### *Positive and Negative Affect Schedule* (PANAS; Watson et al., 1988)

The PANAS is a measure of affect that contains subscales for PA and NA. Each subscale is composed of 10 items rated on a five-point Likert-type scale. In the current study, participants rated the extent to which they felt

each of the 20 emotions in the past week. Scores were summed for each subscale to obtain separate values for PA and NA, with higher scores indicating a greater intensity of that affect type. Internal consistency at baseline for the current sample was good for NA (α = .86) and excellent for PA (α = .90).

#### *Satisfaction With Life Scale* (SWLS; Diener et al., 1985)

The SWLS is a five-item scale that measures life satisfaction. Items are rated on a seven-point Likert-type scale, with higher scores indicating greater life satisfaction. Internal consistency at baseline for the current sample was good (α = .87).

#### *Self-Absorption Scale* (SAS; McKenzie & Hoyle, 2008)

The SAS is a 17-item scale that assesses self-absorption, conceptualized as an excessive, sustained, and rigid focus on the self. The SAS is composed of two subscales, private self-absorption (SAS Private) with eight items and public self-absorption (SAS Public) with nine items. Public self-absorption measures preoccupation with attention to self from the imagined perspective of others (e.g., 'I have difficulty focusing on what others are talking about because I wonder what they're thinking of me.'), whereas private self-absorption is concerned with aspects of self that are inaccessible to others (e.g., 'My mind never focuses on things other than myself for very long.'). Items are rated on a five-point Likert-type scale, with higher scores indicating greater self-absorption. In the current study, participants rated items based on how they felt in the past week. Internal consistency at baseline for the current sample was good for both subscales (α = .89 for SAS Public and α = .83 for SAS Private).

#### *Social Connectedness Scale – Revised* (SCS-R; Lee et al., 2001)

The SCS-R is a 20-item measure of perceived social connection, and it served as our key outcome variable. Items are rated on a six-point Likert-type scale, with higher scores indicating a greater sense of social connection. Internal consistency at baseline for the current sample was excellent (α = .94).

### *Summary of study procedures*

For a detailed account of study procedures, see Online Supplement 1. Eligible participants were invited to complete an in-lab baseline session. During this baseline session, participants completed baseline measures and were randomly assigned to one of three conditions: acts of kindness ($N = 40$), social activities ($N = 41$), or cognitive reappraisal ($N = 41$). After randomization, a researcher met with the participant to review the instructions packet for their condition (see Online Supplement 1 for study packets). For the acts of kindness condition, participants were instructed to perform three acts of kindness each day for two days out of the week, a frequency that is based on previous investigations of acts of kindness showing that clustering kind acts together on discrete days is more impactful than distributing those actions throughout the week (e.g., Alden & Trew, 2013; Lyubomirsky et al., 2005). Acts of kindness were defined as 'big or small acts that benefit others or make others happy, typically at some cost to yourself in terms of time or resources.' For the social activities condition, participants were instructed to plan social activities for two days a week. Note that we did not specify a frequency of social activities for these two days (e.g., 3 social activities a day), as we thought such a requirement might overwhelm participants given that many social activities involve a greater amount of time and planning than most acts of kindness. Social activities were defined as 'big or small activities you intentionally plan with other people for the purpose of enjoyment.' For the cognitive reappraisal condition, participants were instructed to complete thought records for at least two days each week, though they were permitted to complete them more frequently if they chose (in line with research indicating that more frequent completion of thought records predicts symptom change, Rees et al., 2005). Participants followed a step-by-step guide for using thought records based on material from *Mind Over Mood* (Greenberger & Padesky, 1995) and *Cognitive Behavior Therapy: Basics and Beyond* (Beck, 2011). Participants across all three conditions were asked to engage in their assigned activity for five weeks.

Participants completed weekly measures of all outcomes online using Qualtrics (https://www.qualtrics.com/). After two weeks of participation, a researcher contacted the participant by telephone to answer questions and ensure fidelity to the condition instructions. Five weeks after completing their activity, participants received an email invitation to complete a final follow-up assessment.

## Summary of data analytic plan

### *General analytic setup*

For full details of our data analytic strategy, see Online Supplement 2.

Recent evidence indicates that the three subscales of the DASS-21 load onto a single negative affectivity factor that explains the data more parsimoniously than analyzing each subscale independently (Kia-Keating et al.,

2018). Therefore, we created a composite measure of DASS-21 symptoms (DASS Comp) by standardizing each subscale of the DASS-21 and summing the subscales together at each time point. Data for PA, NA, the SCS-R, and the two subscales of the SAS were analyzed independently.

We conducted all analyses using multilevel modeling (MLM) within the mixed models procedure in SPSS. Time (in weeks) and repeated measurements of the outcome variables were entered as level-one variables, and participants were entered as level-two variables. For between-group analyses, dummy codes for experimental group and time x group cross-level interaction terms were entered as additional level-two variables to test group differences in the slope of change (growth curves). We analyzed data on an intent-to-treat basis, with responses from all 122 participants across all time points included. Baseline MSPSS was entered as a level-two covariate in all analyses to control for individual differences in social support as a potential confound.[3] Additionally, to minimize the influence of the COVID-19 shutdown on the data, we created a dichotomous COVID-19 variable to capture whether participants engaged in a portion of the study after the COVID stay-at-home order went into effect. This COVID variable was entered as another level-two covariate in all analyses.

Most outcomes followed a slightly curvilinear pattern of change (see Figures 3 and 4). Therefore, we conducted a square root transformation of linear time to capture this slight curvilinearity across analyses (Singer & Willett, 2003). The only exception was for PA, which exhibited a distinct quadratic pattern of change. Therefore, we used a quadratic coding of time to capture the quadratic shape of change for PA analyses.

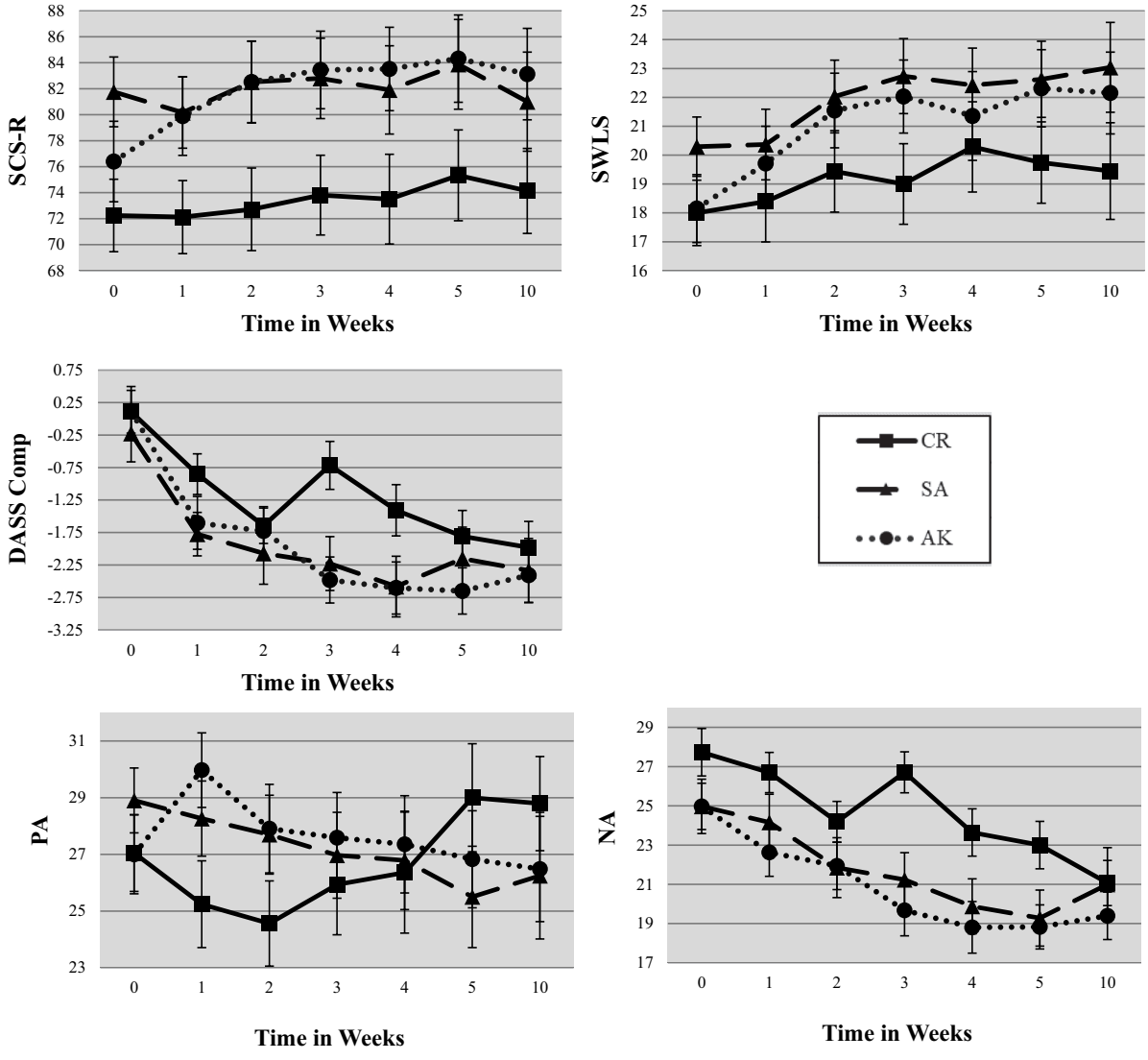


**Figure 3.** Means of main study outcomes plotted at each time point by experimental condition. Means are unadjusted raw scores (not adjusted for covariates). Error bars are standard errors. CR = Cognitive reappraisal, SA = Social activities, AK = Acts of kindness.

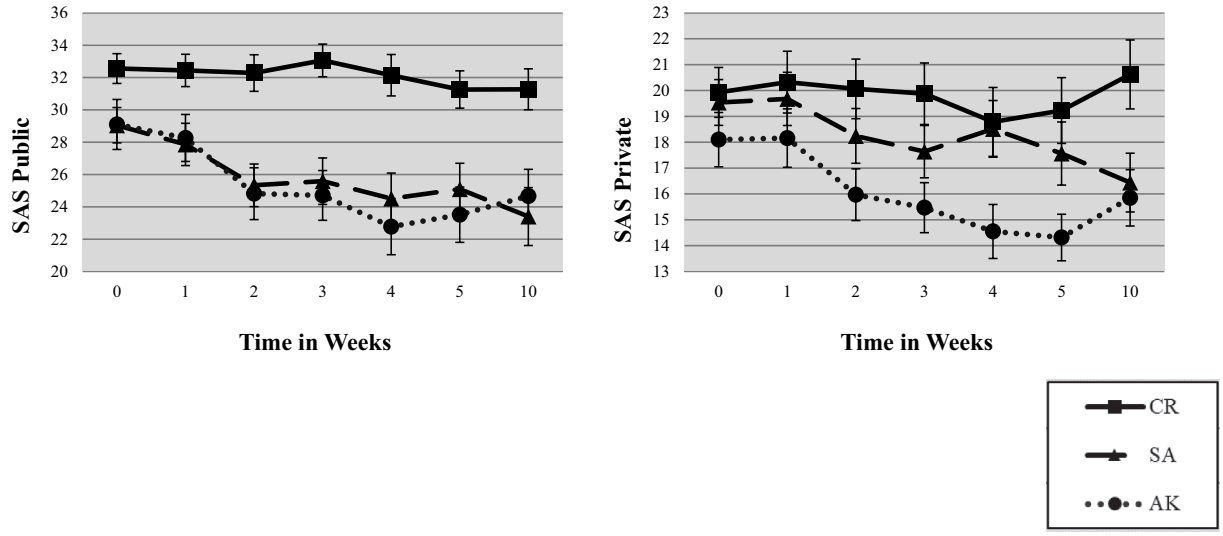


**Figure 4.** Means of self-absorption subscales plotted at each time point by experimental condition. Means are unadjusted raw scores (not adjusted for covariates). Error bars are standard errors. CR = Cognitive reappraisal, SA = Social activities, AK = Acts of kindness.

Across analyses, we used the restricted maximum likelihood method of estimation, a first-order autoregressive (AR-1) repeated measures covariance structure, and a scaled identity (random intercepts only) covariance structure for the random effects. The AR-1 repeated measures covariance structure is optimal for longitudinal designs, as it assumes higher correlations between adjacent time points and decreasing correlations with greater distance between time points. Note that we also tested an unstructured covariance structure for the random effects (i.e., a correlation between random slopes and random intercepts), but we found it did not significantly improve the model fit. Therefore, participants' baseline values did not appear to be associated with their slope of change over time. We used an iterative process to confirm these modeling decisions with model fit statistics. For non-nested models, we selected the parameters with the lowest AIC and BIC values.[4] For nested models, we used the likelihood ratio test to determine if a parameter significantly improved model fit. If the parameter did not significantly improve the model's fit, that parameter was not retained. See Online Supplement 2 for a detailed account of the model-building process. Model fit statistics are available from the authors upon request.

### *Within-group analyses*

We first assessed the degree of change occurring separately within each condition. For each condition, we analyzed a growth curve from baseline (week 0) to follow-up (week 10). As an additional check on the results, we tested whether changes were maintained from post-test (week 5) to follow-up (week 10) with a piecewise linear regression model, as participants may revert toward baseline values after the intervention ends. For the piecewise regression models, we simultaneously fit two linear[5] segments to the model: one capturing change from baseline through post-test (weeks 0 to 5), and a second capturing change from post-test to follow-up (weeks 5 to 10). Significance tests for the second segment indicate whether values significantly changed from post-test to follow-up.

We used a within-subjects Cohen's *d* as an estimate of effect size for change from (a) baseline to post-test and (b) baseline to follow-up. A positive Cohen's *d* indicates scores increased over time, whereas a negative Cohen's *d* indicates scores decreased over time.

### *Between-group analyses*

We used acts of kindness as the reference group for dummy coding: contrast 1 compared acts of kindness to cognitive reappraisal, and contrast 2 compared acts of kindness to social activities. On an exploratory basis, we also tested the contrast between cognitive reappraisal and social activities by recoding the dummy codes to make social activities the reference group, thus providing all pairwise comparisons. To distinguish between-group differences in growth occurring during the active phase of the study (baseline to post-test) from between-group differences emerging at the follow-up period, we analyzed data separately for (1) baseline to post-test (weeks 0 to 5) and (2) baseline to follow-up (weeks 0 to 10). Because our interest was only in a single time point (the follow-up session) for the baseline to follow-up

analyses, baseline values of outcomes were entered as level-two covariates and time was recentered so that the intercept of the model represents residual change occurring from baseline to follow-up in the reference group. Therefore, the group dummy codes become the variables of interest and represent between-group differences at follow-up controlling for baseline values, and these between-group differences are not conflated with the group differences in slopes occurring during the baseline to post-test period (see Online Supplement 2 for full details of this procedure).

We used a between-subjects Cohen's *d* as an estimate of effect size for between-group differences from (a) baseline to post-test and (b) baseline to follow-up. A positive Cohen's *d* indicates that the effect size favors the reference group, whereas a negative value indicates the effect size does not favor the reference group.

### *Mediation analyses with self-absorption*

To test our third hypothesis about the mediating role of self-absorption, we tested each leg of the hypothesized mediation pathway depicted in Figure 1. Note that each path of this model was tested independently due to problems with simultaneously including time and mediator variables as predictors in multilevel mediation models (see Online Supplement 2 for a full explanation). If all three components of the model (paths A – C) depicted in Figure 1 were significant, we considered the results to support our hypothesis. We only included baseline to post-test time points (weeks 0 to 5), as we were primarily interested in whether self-absorption mediated between-group differences during the active phase of the intervention.

Note that path C of the model (group → post-test outcomes) is identical to the between-group analyses described above. For path A (group → SAS), we conducted separate analyses for each subscale of the SAS (SAS Public and SAS Private). We used the same data analytic procedure to test path A as path C (i.e., the between-group analyses). Only SAS subscales that significantly differed between groups were retained as mediators for path B. For path B, we tested whether deviations in the relevant SAS subscale at time *t* predicted subsequent values of the outcome at time $t + 1$, but only for those outcomes that significantly differed between groups in path C. We used a one-week lag between SAS subscales and the outcomes. Following the recommendation of Wang and Maxwell (2015), we within-person centered each SAS subscale to disaggregate the between-person and within-person effects. For each outcome, we initially included group x SAS interaction terms in the model to assess whether group assignment moderated the relationship between the SAS subscales and the outcome. If a group x SAS interaction term was significant, we estimated the relationship between SAS and the outcome separately within each group. If no group x SAS interaction terms were significant, we reran the model and excluded group variables to estimate the relationship between SAS and the outcome in the sample as a whole. We used a liberal alpha value of $p < .10$ to assess for significant group x SAS interactions.

## Results

### *Baseline differences and missing data*

Prior to conducting the main analyses, we tested for any baseline differences between groups for the variables listed in Table 1. No baseline differences were found (all $ps \geq .12$). Note also that individuals participating during the COVID-19 pandemic did not significantly differ from individuals who participated prior to the pandemic on any outcomes at baseline. That is, the intercept testing baseline differences between COVID and non-COVID participants ($b_{00\ x\ COVID}$) was non-significant across outcomes (see Table 3).

We assessed for group differences in missing data with a dichotomous variable coding data as not missing (0) or missing (1) at each time point for the primary outcome, SCS-R.[6] A chi-square test revealed that there were different proportions of missing data between groups, $X^2$ (2, $N = 854$) = 7.58, $p = .02$. Post-hoc *z*-tests confirmed that cognitive reappraisal had a greater proportion of missing data than acts of kindness (cognitive reappraisal = 21.60% missing; acts of kindness = 12.86% missing), whereas the proportion of missing data for social activities (18.12% missing) did not significantly differ from either acts of kindness or cognitive reappraisal.

Although missing data cannot be retrieved, multiple imputation can be used to estimate missing values. All analyses reported below were repeated with multiple imputation. Results were nearly identical between the standard dataset and the multiple imputation dataset, with only one exception. That is, all coefficients classified as significant in the original dataset remained significant either at a value of $p \leq .05$ or marginally significant at a value of $p < .10$, and all non-significant coefficients remained non-significant and in the same direction. The one exception was for the time-lagged mediation analyses, which is noted in the results below. See Online Supplement 2 for the full multiple imputation methods and results.

### Within-group results

Fixed effect estimates for all within-group analyses are reported in Table 2, and plots of change can be observed in Figures 3 and 4. See Online Supplement 3 for full results tables containing random effects.

As can be seen in Table 2, all conditions showed significant linear improvement from baseline to follow-up for DASS Comp, NA, and SWLS. However, only the social activities and acts of kindness groups showed significant linear reductions in SAS Public and SAS Private scores. Furthermore, only participants in the acts of kindness group exhibited a significant linear increase in SCS-R scores. Finally, no groups demonstrated a significant quadratic effect of time for PA at the within-group level.

All linear slopes from post-test to follow-up were non-significant (all *p*s ≥ .07), thus suggesting that any changes from baseline were maintained five weeks after the study ended.

### Between-group results

Results of fixed effects for between-group analyses at post-test are reported in Table 3 and results for fixed effects at follow-up are reported in Table 4. See Figure 3 for plots of change over time by condition. See Online

**Table 2.** Fixed Effects Estimates for Within-Group Analyses from Baseline to Follow-up (Week 10).

| SCS-R | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| CR | | | | | SA | | | | AK | | | |
| Parameter | *b* (*SE*) | *df* | *t* value | *d* (pt/fu) | *b* (*SE*) | *df* | *t* value | *d* (pt/fu) | *b* (*SE*) | *df* | *t* value | *d* (pt/fu) |
| $b_{00\ \text{intercept}}$ | 72.30 (2.39) | 50.61 | 30.25** | | 80.96 (2.24) | 56.02 | 36.15** | | 78.12 (3.05) | 45.89 | 25.63** | |
| $b_{00\ \text{x MSPSS}}$ | 10.11 (1.97) | 39.20 | 5.14** | | 10.89 (1.73) | 38.44 | 6.28** | | 5.97 (1.85) | 36.61 | 3.23** | |
| $b_{00\ \text{x COVID}}$ | −0.34 (5.02) | 37.57 | −0.07 | | −1.26 (4.35) | 37.97 | −0.29 | | −1.82 (6.98) | 36.32 | −0.26 | |
| $b_{10\ \text{time-SR}}$ | 0.49 (0.64) | 80.00 | 0.77 | 0.26/0.17 | 0.15 (0.65) | 82.71 | 0.23 | 0.18/-0.06 | 2.21 (0.66) | 81.32 | 3.37** | 0.63/0.53 |
| DASS Comp | | | | | | | | | | | | |
| $b_{00\ \text{intercept}}$ | 0.09 (0.29) | 77.45 | 0.32 | | −0.51 (0.43) | 57.33 | −1.18 | | −0.44 (0.40) | 54.10 | −1.10 | |
| $b_{00\ \text{x MSPSS}}$ | −0.88 (0.21) | 38.70 | −4.18** | | −0.48 (0.33) | 36.52 | −1.48 | | −0.34 (0.23) | 33.77 | −1.49 | |
| $b_{00\ \text{x COVID}}$ | −0.41 (0.53) | 34.72 | −0.78 | | 0.09 (0.82) | 35.99 | 0.11 | | 0.95 (0.87) | 33.16 | 1.10 | |
| $b_{10\ \text{time-SR}}$ | −0.65 (0.12) | 90.93 | −5.62** | −0.85 /-0.92 | −0.70 (0.14) | 74.30 | −5.18** | −0.66 /-0.74 | −0.89 (0.12) | 76.69 | −7.35** | −1.17 /-1.00 |
| PA | | | | | | | | | | | | |
| $b_{00\ \text{intercept}}$ | 26.28 (1.50) | 62.88 | 17.47** | | 28.61 (1.42) | 65.30 | 20.12** | | 29.09 (1.51) | 58.78 | 19.30** | |
| $b_{00\ \text{x MSPSS}}$ | 3.00 (1.18) | 39.84 | 2.55* | | 2.14 (1.04) | 37.30 | 2.05* | | 1.77 (0.86) | 37.40 | 2.07* | |
| $b_{00\ \text{x COVID}}$ | −0.85 (2.98) | 37.03 | −0.28 | | 0.29 (2.62) | 36.49 | 0.11 | | −6.35 (3.22) | 36.65 | −1.97^ | |
| $b_{10\ \text{time-linear}}$ | −0.08 (0.48) | 94.69 | −0.17 | 0.21/0.20 | −0.96 (0.46) | 86.09 | −2.11* | −0.39 /-0.28 | −0.07 (0.43) | 98.35 | −0.17 | −0.02 /-0.05 |
| $b_{20\ \text{time-quad}}$ | 0.03 (0.04) | 138.57 | 0.63 | | 0.08 (0.04) | 135.14 | 1.80^ | | 0.00 (0.04) | 149.71 | 0.09 | |
| NA | | | | | | | | | | | | |
| $b_{00\ \text{intercept}}$ | 28.51 (0.91) | 86.11 | 31.24** | | 24.63 (1.40) | 59.04 | 17.55** | | 24.15 (1.32) | 51.07 | 18.36** | |
| $b_{00\ \text{x MSPSS}}$ | −2.37 (0.64) | 34.76 | −3.72** | | −1.10 (1.05) | 35.84 | −1.03 | | −0.63 (0.75) | 32.36 | −0.83 | |
| $b_{00\ \text{x COVID}}$ | −1.17 (1.57) | 30.25 | −0.75 | | 1.12 (2.64) | 35.16 | 0.42 | | 2.42 (2.84) | 31.59 | 0.85 | |
| $b_{10\ \text{time-SR}}$ | −2.03 (0.40) | 101.72 | −5.12** | −0.68 /-0.80 | −1.51 (0.46) | 79.38 | −3.27** | −0.77 /-0.40 | −1.89 (0.39) | 92.85 | −4.91** | −0.81 /-0.60 |
| SWLS | | | | | | | | | | | | |
| $b_{00\ \text{intercept}}$ | 18.15 (1.23) | 44.49 | 14.80** | | 19.46 (1.00) | 50.00 | 19.48** | | 19.54 (1.26) | 42.39 | 15.51** | |
| $b_{00\ \text{x MSPSS}}$ | 3.64 (1.04) | 39.32 | 3.49** | | 3.56 (0.79) | 37.61 | 4.50** | | 2.16 (0.78) | 36.67 | 2.76** | |
| $b_{00\ \text{x COVID}}$ | −1.27 (2.68) | 38.37 | −0.47 | | 1.15 (1.99) | 37.24 | 0.58 | | −3.93 (2.95) | 36.32 | −1.33 | |
| $b_{10\ \text{time-SR}}$ | 0.49 (0.23) | 77.69 | 2.10* | 0.39/0.27 | 1.02 (0.25) | 100.34 | 4.06** | 0.56/0.60 | 1.06 (0.22) | 96.76 | 4.87** | 0.93/0.83 |
| SAS Public | | | | | | | | | | | | |
| $b_{00\ \text{intercept}}$ | 32.10 (1.04) | 51.08 | 30.84** | | 29.44 (1.26) | 59.47 | 23.32** | | 27.17 (1.64) | 45.26 | 16.54** | |
| $b_{00\ \text{x MSPSS}}$ | −0.78 (0.85) | 37.13 | −0.92 | | −2.69 (0.96) | 37.47 | −2.82** | | −1.14 (0.99) | 35.68 | −1.15 | |
| $b_{00\ \text{x COVID}}$ | 2.04 (2.15) | 35.28 | 0.95 | | −0.07 (2.39) | 36.79 | −0.03 | | 6.97 (3.75) | 35.34 | 1.86^ | |
| $b_{10\ \text{time-SR}}$ | −0.56 (0.30) | 86.91 | −1.85^ | −0.28 /-0.25 | −1.83 (0.40) | 88.38 | −4.56** | −0.62 /-0.84 | −1.49 (0.36) | 90.27 | −4.12** | −0.74 /-0.57 |
| SAS Private | | | | | | | | | | | | |
| $b_{00\ \text{intercept}}$ | 19.47 (1.11) | 49.30 | 17.59** | | 20.38 (0.94) | 52.00 | 21.63** | | 18.28 (1.15) | 43.27 | 15.90** | |
| $b_{00\ \text{x MSPSS}}$ | −1.31 (0.92) | 38.81 | −1.43 | | −1.78 (0.74) | 37.46 | −2.41* | | −0.31 (0.70) | 34.24 | −0.45 | |
| $b_{00\ \text{x COVID}}$ | 1.03 (2.34) | 37.39 | 0.44 | | −1.56 (1.85) | 36.92 | −0.84 | | −1.78 (2.63) | 33.79 | −0.68 | |
| $b_{10\ \text{time-SR}}$ | −0.09 (0.28) | 80.37 | −0.31 | −0.12/0.12 | −0.76 (0.25) | 86.18 | −2.98** | −0.36 /-0.63 | −0.75 (0.25) | 79.82 | −3.03** | −0.73 /-0.40 |

Note. Regression coefficients are unstandardized. MSPSS is grand-mean centered. ^ $p < .10$, * $p < .05$, ** $p < .01$. $b_{00\ \text{x COVID}}$ = participation in study after the stay-at-home order (0 = before COVID, 1 = after COVID).
$b_{10\ \text{time-SR}}$ = slope for square-root transformation of linear time.
$b_{10\ \text{time-linear}}$ = slope for linear time.
$b_{20\ \text{time-quad}}$ = coefficient for quadratic time. *d* = within-group Cohen's *d* (pt = baseline to post-test; fu = baseline to follow-up; positive values indicate an increase in scores over time and negative values indicate a decrease in scores over time). CR = Cognitive reappraisal, SA = Social activities, AK = Acts of kindness.

Supplement 3 for full results tables containing random effects.

There were significant baseline differences for SCS-R (cognitive reappraisal started lower than social activities) and SAS Public (cognitive reappraisal started higher than both acts of kindness and social activities).

Those in the acts of kindness group exhibited a greater reduction in DASS Comp symptoms than those in the cognitive reappraisal group at post-test ($d$ = 0.38) and follow-up ($d$ = 0.19). The acts of kindness group showed a greater increase in SWLS than the cognitive reappraisal group at post-test ($d$ = 0.33), and with a similarly sized, but non-significant, effect at follow-up ($d$ = 0.35, $p$ = .07). The acts of kindness group showed a greater improvement in SCS-R scores than the social activities group at post-test ($d$ = 0.32), and with a similarly sized, but non-significant, effect at follow-up ($d$ = 0.41, $p$ = .08). Although acts of kindness and cognitive reappraisal did not significantly differ in SCS-R at post-test ($d$ = 0.26, $p$ = .12), there was a significant difference, with the same effect size, favoring acts of kindness over cognitive reappraisal at follow-up ($d$ = 0.26). The social activities and cognitive reappraisal groups did not differ on DASS Comp, SWLS, or SCS-R at either post-test or follow-up.

As can be seen in Figure 3, the acts of kindness and cognitive reappraisal groups displayed opposite patterns of change for PA. PA initially increased in the acts of kindness condition and then declined over the remainder of the study, whereas PA initially decreased in the cognitive reappraisal condition and then increased over the remainder of the study. These patterns are reflected in the linear and quadratic coefficients for PA in Table 3. Note that when a quadratic coefficient is entered simultaneously with a linear coefficient for time, the linear coefficient becomes the instantaneous rate of change (simple slope) at the time point coded 0. In this case, the linear coefficient represents the simple slope for change in PA estimated from week 0 (baseline). Accordingly, there is a significant difference between the linear slope of acts of kindness and cognitive reappraisal that favors the acts of kindness group (linear $b$ = −3.19), which reflects the early increase in PA scores in the acts of kindness group while PA scores decreased in the cognitive reappraisal group. However, there is also a significant difference in the quadratic effect between acts of kindness and cognitive reappraisal that favors the cognitive reappraisal group (quadratic $b$ = 0.73), which reflects the eventual acceleration in PA scores in the cognitive reappraisal group while PA scores declined in the acts of kindness group later in the study. Likewise, there is a significant difference in the quadratic effect between cognitive reappraisal and social activities that favors the cognitive reappraisal group (quadratic $b$ = 0.50), which again reflects the accelerated increase in PA scores in the cognitive reappraisal group while PA scores declined for the social activities group. None of the between-group differences in PA remained significant at follow-up (see Table 4).

## Results of mediation analyses with self-absorption

### Path C

Path C of Figure 1 was established from the between-group analyses, for which we found between-group differences for DASS Comp, SWLS, PA, and SCS-R at post-test. Therefore, we tested the mediation pathway for each of these four outcomes only.

### Path A

As reported in Table 3, the acts of kindness group reported a greater reduction in SAS Public than the cognitive reappraisal group at post-test ($d$ = 0.53), whereas no difference was found between acts of kindness and social activities. No significant between-group differences were obtained for SAS Private, therefore it was not retained as a mediator in path B. Social activities and cognitive reappraisal did not differ from each other for changes in either SAS Public or SAS Private scales at post-test.

### Path B

We limited path B analyses to the SAS Public subscale, as there were no between-group differences on SAS Private. Significant ($p$ < .10) group x SAS Public interactions were obtained for all outcomes except PA. Because there was no significant group x SAS Public interaction in predicting PA, we reran the path B model for PA and excluded group variables, therefore estimating the relationship between SAS Public and PA in the sample as a whole. SAS Public was not a significant predictor of PA in the sample as a whole, $b$ = −0.02, $SE$ = 0.06, $t$(345.27) = −0.32, $p$ = .75.

For the remaining outcomes, we ran analyses separately within groups for DASS Comp, SWLS, and SCS-R due to the significant group x SAS Public interactions. SAS Public significantly predicted these three outcomes in the expected directions, but *only* within the acts of kindness group: DASS Comp [$b$ = 0.08, $SE$ = 0.03, $t$(110.40) = 2.91, $p$ < .01], SWLS [$b$ = −0.12, $SE$ = 0.05, $t$(93.93) = −2.25, $p$ = .03], and SCS-R [$b$ = −1.39, $SE$ = 0.69, $t$(119.29) = −2.03, $p$ < .05]. There was no significant relationship between SAS Public and these three outcomes in the cognitive reappraisal or social activities groups.

**Table 3.** Fixed Effects Estimates for Between-Group Contrasts from Baseline to Post-test (Week 5).

| | SCS-R | | | | DASS Comp | | | | SWLS | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Parameter | *b* (*SE*) | *df* | *t* value | *d* | *b* (*SE*) | *df* | *t* value | *d* | *b* (*SE*) | *df* | *t* value | *d* |
| $b_{00\ \text{intercept}}$ | 77.75 (2.49) | 166.78 | 31.25** | | −0.08 (0.37) | 204.11 | −0.22 | | 18.81 (1.10) | 146.53 | 17.04** | |
| $b_{00\ \text{x MSPSS}}$ | 8.28 (1.08) | 117.93 | 7.71** | | −0.46 (0.15) | 112.89 | −3.12** | | 2.80 (0.49) | 117.87 | 5.66** | |
| $b_{00\ \text{x COVID}}$ | −1.48 (3.18) | 115.91 | −0.47 | | 0.34 (0.44) | 109.42 | 0.79 | | −1.53 (1.46) | 116.18 | −1.04 | |
| $b_{00\ \text{x CR vs. AK}}$ | −5.55 (3.40) | 170.61 | −1.63 | | 0.11 (0.50) | 211.96 | 0.23 | | −0.68 (1.51) | 149.32 | −0.45 | |
| $b_{00\ \text{x SA vs. AK}}$ | 3.32 (3.41) | 170.25 | 0.98 | | −0.32 (0.50) | 210.68 | −0.63 | | 1.21 (1.51) | 148.77 | 0.80 | |
| $b_{00\ \text{x CR vs. SA}}$† | −8.88 (3.38) | 170.80 | −2.63** | | 0.43 (0.50) | 212.65 | 0.86 | | −1.89 (1.50) | 149.57 | −1.26 | |
| $b_{10\ \text{time-SR}}$ | 2.91 (0.84) | 208.12 | 3.48** | | −1.25 (0.15) | 207.72 | −8.19** | | 1.50 (0.29) | 250.91 | 5.21** | |
| $b_{10\ \text{x CR vs. AK}}$ | −1.87 (1.20) | 211.20 | −1.55 | 0.26 | 0.50 (0.22) | 212.82 | 2.27* | 0.38 | −0.91 (0.41) | 254.61 | −2.20* | 0.33 |
| $b_{10\ \text{x SA vs. AK}}$ | −2.47 (1.18) | 208.92 | −2.09* | 0.32 | 0.36 (0.22) | 209.43 | 1.66^ | 0.32 | −0.35 (0.41) | 252.00 | −0.86 | 0.26 |
| $b_{10\ \text{x CR vs. SA}}$† | 0.60 (1.20) | 211.92 | 0.50 | −0.06 | 0.14 (0.22) | 214.43 | 0.62 | 0.00 | −0.56 (0.42) | 255.58 | −1.36 | 0.09 |

| | SAS Public | | | | SAS Private | | | | NA | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Parameter | *b* (*SE*) | *df* | *t* value | *d* | *b* (*SE*) | *df* | *t* value | *d* | *b* (*SE*) | *df* | *t* value | *d* |
| $b_{00\ \text{intercept}}$ | 28.58 (1.29) | 169.89 | 22.23** | | 18.35 (1.02) | 161.13 | 17.96** | | 24.93 (1.20) | 200.77 | 20.76** | |
| $b_{00\ \text{x MSPSS}}$ | −1.31 (0.55) | 114.44 | −2.39* | | −0.92 (0.44) | 113.78 | −2.07* | | −1.06 (0.48) | 108.28 | −2.20* | |
| $b_{00\ \text{x COVID}}$ | 2.88 (1.62) | 112.09 | 1.78^ | | −0.42 (1.31) | 111.56 | −0.32 | | 1.04 (1.42) | 104.53 | 0.73 | |
| $b_{00\ \text{x CR vs. AK}}$ | 3.56 (1.76) | 174.53 | 2.02* | | 1.89 (1.40) | 165.26 | 1.35 | | 2.85 (1.65) | 209.77 | 1.73^ | |
| $b_{00\ \text{x SA vs. AK}}$ | −0.03 (1.76) | 174.01 | −0.02 | | 1.43 (1.40) | 164.75 | 1.02 | | 0.40 (1.65) | 207.80 | 0.24 | |
| $b_{00\ \text{x CR vs. SA}}$† | 3.59 (1.75) | 174.87 | 2.06* | | 0.46 (1.39) | 165.65 | 0.33 | | 2.45 (1.64) | 210.67 | 1.49 | |
| $b_{10\ \text{time-SR}}$ | −2.29 (0.45) | 231.49 | −5.10** | | −1.27 (0.33) | 204.83 | −3.81** | | −2.56 (0.50) | 234.53 | −5.11** | |
| $b_{10\ \text{x CR vs. AK}}$ | 1.49 (0.64) | 235.79 | 2.31* | 0.53 | 0.63 (0.48) | 208.85 | 1.33 | 0.48 | 0.76 (0.72) | 240.78 | 1.06 | 0.17 |
| $b_{10\ \text{x SA vs. AK}}$ | 0.54 (0.64) | 232.86 | 0.86 | 0.19 | 0.77 (0.47) | 206.15 | 1.62 | 0.29 | 0.39 (0.71) | 236.27 | 0.55 | 0.06 |
| $b_{10\ \text{x CR vs. SA}}$† | 0.94 (0.64) | 237.05 | 1.46 | 0.41 | −0.13 (0.48) | 210.10 | −0.28 | 0.22 | 0.38 (0.72) | 242.40 | 0.52 | 0.13 |

| PA | | | | |
|---|---|---|---|---|
| Parameter | *b* (*SE*) | *df* | *t* value | *d* |
| $b_{00\ \text{intercept}}$ | 28.13 (1.41) | 212.86 | 19.89** | |
| $b_{00\ \text{x MSPSS}}$ | 1.92 (0.57) | 118.78 | 3.36** | |
| $b_{00\ \text{x COVID}}$ | −2.52 (1.69) | 115.19 | −1.50 | |
| $b_{00\ \text{x CR vs. AK}}$ | −0.61 (1.94) | 220.94 | −0.31 | |
| $b_{00\ \text{x SA vs. AK}}$ | 1.02 (1.94) | 219.61 | 0.53 | |
| $b_{00\ \text{x CR vs. SA}}$† | −1.63 (1.93) | 221.27 | −0.85 | |
| $b_{10\ \text{time-linear}}$ | 0.89 (0.85) | 368.70 | 1.05 | |
| $b_{20\ \text{time-quad}}$ | −0.21 (0.16) | 389.38 | −1.31 | |
| $b_{10\ \text{x CR vs. AK}}$ | −3.19 (1.21) | 370.65 | −2.63** | −0.24 |
| $b_{10\ \text{x SA vs. AK}}$ | −1.58 (1.20) | 370.96 | −1.32 | 0.40 |
| $b_{10\ \text{x CR vs. SA}}$† | −1.61 (1.21) | 372.89 | −1.33 | −0.67 |
| $b_{20\ \text{x CR vs. AK}}$ | 0.73 (0.23) | 388.59 | 3.12** | |
| $b_{20\ \text{x SA vs. AK}}$ | 0.23 (0.23) | 391.49 | 1.00 | |
| $b_{20\ \text{x CR vs. SA}}$† | 0.50 (0.23) | 390.71 | 2.14* | |

Note. Regression coefficients are unstandardized. MSPSS is grand-mean centered. ^ $p < .10$, * $p < .05$, ** $p < .01$. $b_{00\ \text{x COVID}}$ = participation in study after the stay-at-home order (0 = before COVID, 1 = after COVID).
$b_{10\ \text{time-SR}}$ = slope for square-root transformation of linear time.
$b_{10\ \text{time-linear}}$ = slope for linear time.
$b_{20\ \text{time-quad}}$ = coefficient for quadratic time. *d* = between-group Cohen's *d* (from baseline to post-test; positive values indicate the effect size favors the reference group, whereas negative values indicate the effect size does not favor the reference group). CR = Cognitive reappraisal, SA = Social activities, AK = Acts of kindness. AK is the reference group for CR vs. AK and SA vs. AK. † dummy codes were recoded to make SA the reference group for CR vs. SA.

**Table 4.** Fixed Effects Estimates for Between-Group Contrasts at Follow-up (Week 10).

| | SCS-R | | | | DASS Comp | | | | PA | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Parameter | *b* (*SE*) | *df* | *t* value | *d* | *b* (*SE*) | *df* | *t* value | *d* | *b* (*SE*) | *df* | *t* value | *d* |
| $b_{00\ \text{intercept}}$ | 82.56 (1.60) | 161.11 | 51.49** | | −2.62 (0.29) | 162.28 | −9.20** | | 28.39 (1.19) | 162.44 | 23.88** | |
| $b_{00\ \text{x baseline}}$ | 0.79 (0.06) | 104.42 | 13.66** | | 0.52 (0.06) | 103.24 | 8.30** | | 0.60 (0.08) | 103.59 | 7.59** | |
| $b_{00\ \text{x MSPSS}}$ | −0.07 (0.89) | 104.89 | −0.08 | | −0.10 (0.13) | 102.77 | −0.80 | | 0.25 (0.55) | 105.60 | 0.45 | |
| $b_{00\ \text{x COVID}}$ | 2.66 (2.05) | 102.30 | 1.30 | | −0.29 (0.36) | 97.52 | −0.81 | | −0.17 (1.51) | 101.45 | −0.11 | |
| $b_{00\ \text{x CR vs. AK}}$ | −4.40 (2.05) | 107.67 | −2.15* | 0.26 | 0.78 (0.35) | 102.36 | 2.24* | 0.19 | −1.60 (1.48) | 106.16 | −1.08 | −0.26 |
| $b_{00\ \text{x SA vs. AK}}$ | −3.56 (1.98) | 107.49 | −1.79^ | 0.41 | 0.31 (0.35) | 102.58 | 0.88 | 0.16 | −2.07 (1.46) | 107.17 | −1.41 | 0.26 |
| $b_{00\ \text{x CR vs. SA}}$ † | −0.84 (2.09) | 107.64 | −0.40 | −0.15 | 0.48 (0.35) | 103.08 | 1.35 | 0.01 | 0.46 (1.49) | 106.92 | 0.31 | 0.55 |

| | NA | | | | SWLS | | | | SAS Public | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Parameter | *b* (*SE*) | *df* | *t* value | *d* | *b* (*SE*) | *df* | *t* value | *d* | *b* (*SE*) | *df* | *t* value | *d* |
| $b_{00\ \text{intercept}}$ | 18.92 (0.97) | 170.17 | 19.61** | | 22.00 (0.66) | 156.58 | 33.48** | | 24.98 (0.90) | 168.77 | 27.88** | |
| $b_{00\ \text{x baseline}}$ | 0.48 (0.06) | 111.47 | 7.48** | | 0.92 (0.06) | 108.91 | 16.34** | | 0.75 (0.06) | 108.24 | 12.19** | |
| $b_{00\ \text{x MSPSS}}$ | −0.21 (0.41) | 102.12 | −0.52 | | −0.20 (0.34) | 110.26 | −0.58 | | −0.23 (0.39) | 108.02 | −0.59 | |
| $b_{00\ \text{x COVID}}$ | −0.06 (1.18) | 97.65 | −0.05 | | 1.15 (0.85) | 105.41 | 1.35 | | −0.71 (1.14) | 102.21 | −0.62 | |
| $b_{00\ \text{x CR vs. AK}}$ | 2.17 (1.18) | 103.05 | 1.84^ | −0.13 | −1.53 (0.84) | 110.06 | −1.83^ | 0.35 | 3.07 (1.12) | 107.82 | 2.75** | 0.39 |
| $b_{00\ \text{x SA vs. AK}}$ | 0.93 (1.14) | 102.75 | 0.81 | 0.20 | −0.69 (0.82) | 110.10 | −0.84 | 0.18 | 0.24 (1.07) | 106.66 | 0.23 | −0.14 |
| $b_{00\ \text{x CR vs. SA}}$ † | 1.24 (1.18) | 103.32 | 1.05 | −0.35 | −0.84 (0.85) | 110.11 | −0.99 | 0.19 | 2.83 (1.12) | 108.03 | 2.52* | 0.67 |

| | SAS Private | | | |
|---|---|---|---|---|
| Parameter | *b* (*SE*) | *df* | *t* value | *d* |
| $b_{00\ \text{intercept}}$ | 16.84 (0.75) | 145.33 | 22.55** | |
| $b_{00\ \text{x baseline}}$ | 0.71 (0.07) | 113.48 | 10.67** | |
| $b_{00\ \text{x MSPSS}}$ | −0.23 (0.34) | 108.74 | −0.67 | |
| $b_{00\ \text{x COVID}}$ | −1.03 (0.97) | 103.13 | −1.06 | |
| $b_{00\ \text{x CR vs. AK}}$ | 1.31 (0.97) | 108.95 | 1.35 | 0.46 |
| $b_{00\ \text{x SA vs. AK}}$ | 0.97 (0.95) | 108.44 | 1.03 | −0.14 |
| $b_{00\ \text{x CR vs. SA}}$ † | 0.34 (0.96) | 108.31 | 0.36 | 0.64 |

Note. Regression coefficients are unstandardized. ^ $p < .10$, * $p < .05$, ** $p < .01$. $b_{00\ \text{intercept}}$ = follow-up value adjusted for baseline mean of outcome. $b_{00\ \text{x baseline}}$ = baseline value of outcome. Baseline value is grand-mean centered. MSPSS is grand-mean centered. $b_{00\ \text{x COVID}}$ = participation in study after the stay-at-home order (0 = before COVID, 1 = after COVID). *d* = between-group Cohen's *d* (from baseline to follow-up; positive values indicate the effect size favors the reference group, whereas negative values indicate the effect size does not favor the reference group). CR = Cognitive reappraisal, SA = Social activities, AK = Acts of kindness. AK is the reference group for CR vs. AK and SA vs. AK. † dummy codes were recoded to make SA the reference group for CR vs. SA. Note that the slope for the square-root transformation of linear time ($b_{10\ \text{time-SR}}$) is omitted in this table, as the focus of the analyses was on the intercepts.

### *Multiple imputation results*

As we mentioned earlier, the results of our standard analyses and the multiple imputation analyses were identical, with only one exception (see Online Supplement 2 for full multiple imputation results). The one exception was that for the time-lagged mediation analyses, the relationship between SAS Public and SWLS was no longer significant within the acts of kindness group, $b = -0.08$, $SE = 0.06$, $t = -1.32$, $p = .19$. However, note that this coefficient of $b = -0.08$ is consistent in direction with the original estimate of $b = -0.12$ reported above.

## Discussion

We made three hypotheses at the outset of this study and, with some caveats, found support for all three hypotheses. All groups demonstrated significant reductions in negative affect and the composite symptom scores (H1). Additionally, all groups demonstrated significant improvement in life satisfaction. For all three of these outcomes (negative affect, composite symptom scores, and life satisfaction), gains were maintained at follow-up across conditions. These results are encouraging, as they suggest that all three study interventions are effective at reducing distress and improving life satisfaction, and the benefits lasted even after the active phase of the intervention ended. Furthermore, the fact that both cognitive reappraisal and social activities showed significant improvement on these outcomes suggests they were rigorous comparisons for acts of kindness. That is, it was not the case that acts of kindness were being contrasted with inert study conditions or conditions that resulted in worse mental health (e.g., focusing on daily hassles).

Yet even with two strong comparison conditions, acts of kindness showed advantages over both cognitive reappraisal and social activities for social connection, which was our primary study outcome (H2). Neither the social activities nor the cognitive reappraisal conditions resulted in significant change in social connection. Thus, our findings replicate previous research that suggests CBT has a small effect on social well-being variables (Hofmann et al., 2014), whereas acts of kindness may be a promising candidate for improving social well-being. Moreover, the contrast between acts of kindness and social activities suggests that prosocial behavior confers unique benefits that cannot be reduced to general social interaction. Therefore, incorporating specifically prosocial behavior into behavioral activation plans (Jacobson et al., 2006) might increase the effectiveness of these interventions. Additionally, the acts of kindness group exhibited greater improvements than the cognitive reappraisal group for life satisfaction and composite symptom scores. This difference on the composite measure of symptoms is particularly surprising given that thought records are specifically designed to target depression/anxiety symptoms, and they have a strong track record of success in this area (e.g., McManus et al., 2012; Persons & Burns, 1985).

Finally, we found support for H3, our hypothesized mediation pathway depicted in Figure 1. Those in the acts of kindness group exhibited a greater degree of improvement than those in the cognitive reappraisal group on composite symptom scores, satisfaction with life, and social connection (path C). The acts of kindness group also showed a greater reduction in public self-absorption than the cognitive reappraisal group (path $A_1$), and deviations in public self-absorption predicted subsequent values of composite symptom scores, satisfaction with life, and social connection scores – but only within the acts of kindness group (path B). These analyses, which used a time-lagged mediation rather than a cross-sectional mediation as in previous studies of acts of kindness, provide some of the first evidence of a mechanism of change for acts of kindness.

Collectively, our results provide converging evidence with previous research that acts of kindness may be a promising intervention for individuals with clinical presentations. Although acts of kindness are often conceptualized as an exercise that is primarily relevant for improving well-being in asymptomatic samples, the results of the current study challenge that notion. As noted earlier, participants had a medium symptom severity on average, with 24–37% of the sample even falling into the severe range for symptoms of depression, anxiety, and stress.

### *Limitations*

Results of this study should be considered in light of its limitations. First, we cannot rule out all effects of the COVID-19 pandemic on results. The pandemic suspended recruitment of new participants late into data collection, consequently resulting in a smaller sample size than we might otherwise have obtained. A smaller sample size ultimately results in lower power to detect significant effects. Furthermore, the COVID-19 social restrictions may have interacted with our study conditions in unknown and unmeasured ways. For example, social restrictions may have worked against the acts of kindness and social activities conditions – which required purposeful and planned social interaction – and in favor of the cognitive reappraisal condition, which required no such social interaction to complete the study activities. Conversely, it is also possible that

social restrictions may have enhanced the potency of the acts of kindness and social activities conditions, as these conditions required individuals to maintain social connections during a time when society was socially isolated, whereas cognitive reappraisal did not require individuals to challenge their social isolation. Given that the social restrictions associated with the pandemic were unexpected at the outset of the study, we do not have any measures that might help us determine the exact nature of the impact on our results. However, we attempted to limit the potential influence of the pandemic on our results by including COVID restrictions as a covariate in all analyses, as well as keeping the face-to-face requirement in place for social activities to maintain consistency across participants.

Second, this study was largely autonomous, as participants only interacted with the researchers at baseline and during a brief two-week phone check-in. However, thought records are typically used in the context of weekly CBT with a professional therapist. Therefore, our cognitive reappraisal condition is not equivalent to honing one's use of a thought record with feedback from a therapist each week. This lack of coaching may explain why the cognitive reappraisal group had a greater amount of missing data than the acts of kindness group. This disparity was expected, as completing thought records is presumably a more complex skill than performing acts of kindness. Although we attempted to account for the missing data with multiple imputation – which produced results consistent with the original dataset (see Online Supplement 2) – missing data can never be truly retrieved. Therefore, the greater retention of participants within the acts of kindness condition may have resulted in advantages for acts of kindness over cognitive reappraisal. That said, our results raise the question of whether thought records should be a preferred well-being self-help tool when the current results suggest acts of kindness produce better results and result in less dropout.

### *Future directions*

The current study provides a foundation for several areas of future inquiry. First, if acts of kindness are to be adopted as a clinical technique, it will be important to demonstrate their benefits in a bona fide clinical sample, i.e., one in which all participants meet criteria for a DSM-5 diagnosis (American Psychiatric Association, 2013). Although the current study provides a step in that direction, the self-reported depression and anxiety symptoms used in our study are not equivalent to a professionally-administered diagnostic assessment for a depressive or anxiety disorder. Therefore, it will be important to demonstrate the benefits of acts of kindness in a more clinically representative sample, perhaps one that utilizes a diagnostic assessment instrument such as the Structured Clinical Interview for DSM-5 (SCID-5, First et al., 2016).

Second, the COVID-19 pandemic has raised questions about the necessity of in-person interaction for social connection. Across the globe, people have been searching for ways to maintain social connection while quarantining. Even after the pandemic passes, there may be some individuals who are more willing to connect with others via virtual platforms than in-person activities. Therefore, it would be worthwhile to replicate this study using virtual platforms (e.g., phone, facetime, or Zoom) to determine if comparable benefits are achieved.

Finally, we only examined the effects of performing acts of kindness on the participants. It would be important to also examine the impact on the recipients of these kind actions, as there may be bidirectional benefits for both the giver and receiver of kindness.

## Conclusion

In this study, we demonstrated that performing acts of kindness promotes social connection, a construct that is a key predictor of both well-being and recovery from anxiety and depressive disorders. We further demonstrated that performing acts of kindness results in greater well-being benefits than established CBT techniques (thought records and social activity planning). Finally, this study provides some of the first evidence of a mechanism of change for acts of kindness: public self-absorption. Collectively, the results of this study highlight the clinical potential of acts of kindness, and future research will confirm whether acts of kindness should be incorporated in the canon of evidence-based clinical techniques.

## Notes

1. We included the stress scale as an additional measure of anxiety given its correspondence with generalized anxiety disorder symptoms (Lovibond & Lovibond, 1995).
2. We repeated all of our analyses reported in Tables 2–4 with current mental health treatment included as a level-two covariate. Without exception, results were identical to those reported in these tables (i.e., all significant or marginally significant results remained significant or marginally significant, and all non-significant results remained non-significant.
3. Note that we pre-specified MSPSS as a covariate prior to collecting or analyzing any data.
4. For all parameters tested, the AIC and BIC values agreed (i.e., the parameters with the lowest AIC values also had the lowest BIC values).

5. We used the untransformed raw values of linear time in weeks for both linear segments of the piecewise regression model, rather than the square-root-transformed values, as a square root transformation of two linear segments entered simultaneously may introduce unknown properties into the model.
6. Though we used SCS-R to draw conclusions about group differences in missing data, we confirmed the same pattern held across other outcomes.

## Acknowledgments

We would like to express our sincere thanks to our research assistants, without whom this project would not have been possible: Myshellia Carter, Ashton Cole, Benjamin Holbrook, Marley Musarra, and Brianna Schonhiutt.

## Disclosure statement

No potential conflict of interest was reported by the author(s).

## Funding

This work was supported by the Graduate School, Ohio State University

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## Data availability statement

Please contact the first author if you are interested in working with the data reported in this manuscript. The materials described in this article are openly available in the open science Framework at https://osf.io/v4nxc/?view_only=2c45de2cd3a8467694beb9a218d2312d

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This article has earned the Center for Open Science badge for Open Materials. The materials are openly accessible at https://osf.io/v4nxc/?view_only=2c45de2cd3a8467694beb9a218d2312d

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