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Is the Mind More Powerful Than the Heart? A Randomized Controlled Trial of Two Loving-Kindness Interventions

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Abstract

Context. Many spiritual traditions, like Buddhism, emphasize the importance of developing feelings of love and altruism toward others regardless of circumstances, but doing so can be challenging, especially in situations where individuals have little self-control resources, for example due to cognitive fatigue. These traditions offer techniques to train the mind, such as loving-kindness meditation, in order to facilitate the achievement of this altruistic goal. However, the evidence in favour of the prosocial benefits of loving-kindness meditation is scant and at times conflicting. **Methods.** To investigate the long-term effects of loving-kindness training on prosociality and aggression, we investigated changes in a range of prosocial variables at pre, post, and follow-up in a preregistered randomized controlled trial comparing three groups: loving-kindness meditation (N = 58), loving-kindness reflection (N = 53), and waitlist control (N = 92). Furthermore, to establish the robustness of the loving-kindness training relative to challenging circumstances such as cognitive fatigue, we introduced an ego depletion task at post-testing. **Results.** Planned contrasts analyses suggested effects on positive affect only for the meditation group, on self-reported compassion for both the meditation and reflection groups, and explicit social attitudes, aggressive behaviour, cognitive accessibility of a recent altruistic behaviour, willingness to help, only for the reflection group. There were no other group differences in attitudes toward aggression, dehumanization, implicit aggression, or charity donations. Moderation analyses revealed three-way interactions between group, implicit attitudes, and ego depletion, as hypothesized, but only for compassion and cognitive accessibility of a recent altruistic behaviour. **Discussion.** In conclusion, results suggest that the effects of reflecting on loving-kindness concepts provide robust and reliable prosocial effects, perhaps more than meditating on those concepts. Even those benefits are limited to certain behaviours, cognitions, or affects, and they do not generalize to many common behaviours such as charity donations. However, ego depletion seems to only moderate implicit attitudes when loving-kindness is embodied through meditation and not when it is confined to the intellectual mind.

Keywords: loving-kindness, meditation, prosociality, altruism, compassion, aggression

Significance Statement

Many people believe loving-kindness meditation has clear prosocial benefits, but few studies have investigated the question rigorously. We investigated changes in a range of prosocial variables at pre, post, and follow-up in a preregistered randomized controlled trial comparing three groups: loving-kindness meditation, loving-kindness reflection, and waitlist control. Our results suggest effects on positive affect only for the meditation group, on self-reported compassion for both the meditation and reflection groups, and on explicit attitudes, aggressive behaviour, cognitive accessibility of a recent altruistic behaviour, and willingness to help only for the reflection group. It seems that the effects of reading and thinking about loving-kindness concepts provide robust and reliable prosocial effects, perhaps more than meditating on those concepts.

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While good intentions are essential to a moral life, they are insufficient on their own because their behavioural execution depends on limited cognitive resources. Patience and tolerance, for example, require cognitive resources that are not always available to us. Isn't it harder to be patient and tolerant when we are tired, hungry, or physically exhausted? Indeed, we often lack these cognitive resources, in which case we tend to rely on more automated modes of thought and action. In other words, when we are tired and exhausted, we are likely to run on "autopilot" mode and revert to our default reactions and behaviours. Under normal circumstances, and when stemming from honest and "good" intentions, these automatic reactions and behaviours are quite helpful; however, what of automatic reactions stemming from non-virtuous intentions? When these automatic behaviours are non-virtuous (for example, characterized by aggression or impatience), individuals may end up acting in ways that they will later regret. Is it possible to modify the underlying intentions of these automatic processes? This study aims to use loving-kindness meditation as a way to reinforce *virtuous* automatisms and encourage people to act with more compassion. Specifically, we hope to transform the automatisms of aggression and selfishness into benevolence and altruism.

Most of us have good intentions or, at the very least, have an idea of how to act in accordance with moral standards. Sometimes, however, selfish temptations take over, leading to behaviours such as lying, cheating, stealing, discrimination, and aggression. What strategies do individuals use to overcome these selfish temptations? Can we train *positive* or *virtuous* strategies? More importantly, could this training lead to more prosocial behaviours? This research aims to better understand the dynamic interaction between unconscious values and

deliberate behaviours, focusing on how automatisms (virtuous and non-virtuous) influence our prosocial and aggressive behaviours after having completed directed contemplative training (e.g., mindfulness/loving-kindness meditation training). This study aims to advance contemplative research relative to loving-kindness and help promote and nurture compassionate living and human flourishing.

People often spontaneously and unconsciously put their selfish interests ahead of those of their group despite social norms or personal values (Moore & Loewenstein, 2004). Thus, sacrificing oneself for others or for the "common good" is not an intuitive or easy undertaking; it often requires conscious, intentional, and deliberate effort. In addition, a given behavioural outcome generally depends on the interaction between (a) deliberate systemic psychological influences on behaviour (such as values and moral norms) and (b) automatic cognitive influences on behaviour (like unconscious values; Hofmann et al., 2009). When the deliberate system is exhausted (e.g., cognitive fatigue) or naturally weak (e.g., due to an impulsive personality), the automatic system usually "takes" control of the behaviour. For example, people who have a *low capacity for self-control or working memory* have a weaker deliberate system, making them much more likely to be influenced by the automatic system and follow their impulses than those with higher capacities for self-control and working memory (Friese & Hofmann, 2009; Hofmann, Gschwendner, Friese, et al., 2008). In short, research shows that two systems compete to determine behaviour and that the exhaustion of the deliberate system gives way to the automatic system.

The deliberate system can be depleted in multiple ways: cognitive fatigue or cognitive load (lack of mental energy), time pressure (lack of structural resources) and alcohol (biological and cognitive hindrances; Hofmann et al., 2009). All of these factors may interfere with one's

ability for self-control and cause people to react more impulsively, even if these reactions are undesirable. For example, while conscious eating goals (e.g., a low-calorie diet) only predict candy consumption when people have all their mental energy, the automatic system becomes the primary predictor once participants are cognitively tired (Hofmann et al., 2007). Similarly, the automatic system determines the brand of food to buy, but only when time is of the essence, while participants choose their conscious preference when they have time to choose (Friese et al., 2006). From a prosocial perspective, implicit attitudes better predict interracial interaction behaviour when participants undergo a cognitively taxing memory task, whereas the opposite pattern emerges for explicit attitudes (Hofmann, Gschwendner, Castelli, et al., 2008). Finally, people's unconscious aggression predicted their hostile behaviour, but only in cases of cognitive fatigue (Schmidt et al., 2015), which illustrates how automatic processes influence behaviour. Therefore, relying solely on our deliberate system (e.g., self-control, goals, values, or norms) for moral and altruistic behaviour may be problematic because, once exhausted, our deliberate system loses its influence over our prosocial behaviours.

What would happen if we turned the negative, selfish automatic system into a positive, altruistic system—would it lead to prosocial behaviours? We believe that transforming one's automatic system in this way would positively influence our behaviours, most obviously in the context of an exhausted deliberate system (i.e., when cognitively depleted). We propose that loving-kindness meditation is a prime candidate to make our automatic system more moral and virtuous. Loving-kindness meditation aims to develop pleasant and loving feelings towards oneself and all others (Hutcherson et al., 2008). In such a practice, individuals consider the positive feelings they have toward their loved ones, then extend those feelings to others (e.g., themselves, an acquaintance, a difficult being) and, finally, to all beings. The exercises

associated with this practice involve repeating short sentences (e.g., “May you be healthy,” and “May all beings be protected and safe”) and visualizing the recipient of these sentences (Galante et al., 2014). Loving-kindness meditation aims to establish a positive sense of interconnectedness with others (Kang et al., 2014). Indeed, research shows that such interventions can reduce unconscious bias (Dasgupta, 2013; Kang et al., 2014; Rudman et al., 2001; Stell & Farsides, 2015) and increase altruistic behaviours (Galante et al., 2016; Leiberg et al., 2011; Weng et al., 2013).

However, the effects of this loving-kindness meditation practice on aggression remain mixed. A meta-analysis of 16 randomized controlled trials of loving-kindness meditation and mindfulness states that these interventions did not decrease aggression (Kreplin et al., 2018). It is difficult to draw strong conclusions given the quality of many of these meditation studies. For example, five out of the seven loving-kindness or compassion studies from this meta-analysis (Kreplin et al., 2018, supplementary materials) explicitly advertised their study as a meditation study, increasing the odds of self-selection bias. Furthermore, the authors of the meta-analysis gave only one of those studies a medium quality score (2 = moderate); all other studies received the lowest possible score (3 = weak).

Another meta-analysis of 22 studies that compared the effects of compassion-based meditation training (including loving-kindness) and a control group indicates that this practice increases positive emotions but highlights the lack of evidence regarding its effect on negative emotions (Galante et al., 2014). In light of this last finding, we do not know whether loving-kindness meditation can decrease aggression, a behaviour associated with negative emotions such as anger. Moreover, the duration of these effects is subject to debate (e.g., Devine et al., 2012; Lai et al., 2016), and no research has investigated whether the gains achieved by loving-

kindness meditation are maintained when the deliberate system is exhausted (e.g., under conditions of poor self-control, cognitive fatigue, or poor working memory). Our current research attempts to rectify these limitations by investigating whether loving-kindness meditation has the potential to durably shape the automatic system in a moral and altruistic way and lead to prosocial behaviours, regardless of the integrity of the deliberate system.

Research Questions and Hypotheses

Research question 1. Will the prosocial benefits of a six-week online loving-kindness meditation or loving-kindness reflection program be greater than those of a waitlist control group immediately after the program?

Hypothesis 1. Meditation and reflection groups on loving-kindness should show a greater increase in prosocial outcomes compared to the control group. Specifically, they should show a greater increase in charitable giving, compassion, intentions to help others, and cognitive accessibility of altruistic memories, and a greater reduction in aggressive behaviour, conscious and unconscious aggression, negative social attitudes, dehumanization, and cognitive accessibility of aggressive memories.

Research question 2. How will meditation or reflection programs differ across implicit or explicit measures?

Hypothesis 2. The loving-kindness meditation group should show greater improvements in implicit measures (unconscious aggressiveness, cognitive accessibility of altruistic or aggressive memories) compared to the reflection group. However, these two groups will show similar changes in explicit/controlled measures (conscious aggression, aggressive behaviour, social attitudes, dehumanization, charity giving, compassion, and intentions to help others).

Research question 3. Will the prosocial benefits persist six weeks post-intervention (Time 3)?

Hypothesis 3. The differences between the three groups described in the first two hypotheses should persist six weeks after the end of the program.

Research question 4. Will cognitive fatigue moderate the relationship between attitudes and behaviour differently in different experimental groups?

Hypothesis 4. Cognitive fatigue ("ego depletion") will moderate the link between attitudes and prosociality (i.e., it will act as a "boundary condition"), where cognitive fatigue will increase the predictive power of implicit attitudes and reduce that of explicit attitudes. However, this will only be true for the reflection group and waiting list group. Specifically, the reflection and waiting list groups should be *less* prosocial when they are cognitively exhausted (in terms of aggressive behaviour, charitable giving, compassion, and intentions to help others) compared to when they are not cognitively depleted. We hypothesize that the loving-kindness meditation group, having undergone six-week meditative training in developing their implicit and explicit prosocial automatisms cognitive depletion, will not influence their prosocial behaviours—they will be more prosocial whether they are explicitly depleted or not.

Materials and Methods

Open Practices

We preregistered this study on the Open Science Framework (OSF) platform: research design, inclusion and exclusion criteria, sample size, variables, hypotheses, and analyses (https://osf.io/gkd8s). The data, analysis script, analysis report, and materials for this study are also available on OSF (https://osf.io/5khjm). An open, interactive report of all analyses is available here: https://remi-theriault.com/scripts/varela.

Design & Procedure

This study used a mixed design (between subjects with repeated measurements). Participants completed a series of tasks and questionnaires at three-time points to assess the pre-post (T1, T2) effectiveness of a loving-kindness intervention and its long-term (T3) effects.

Power Analysis, Participants, and Exclusions

We determined our sample size based on a power analysis. Using the means and standard deviations of the smallest IAT effect size of Kang et al. (2014; for homelessness, not race), we estimated that their effect size *d* of Cohen = 0.57 (for later measurements only and for the crucial comparison of interest, i.e., meditation versus waiting list). For planned contrasts, detecting this average effect size, with a power of 80% and an alpha level of 0.05, requires 50 participants per group (150 in total). For moderations, detecting a medium effect size ($f^2 = 0.15$), with a power of 80%, an alpha level of 0.05, and six predictors (excluding ordinate at origin) requires 98 participants in total. Therefore, we planned to have 50 participants, after exclusions, per group (waitlist, loving-kindness meditation and loving-kindness reflection), for a total of 150 participants.

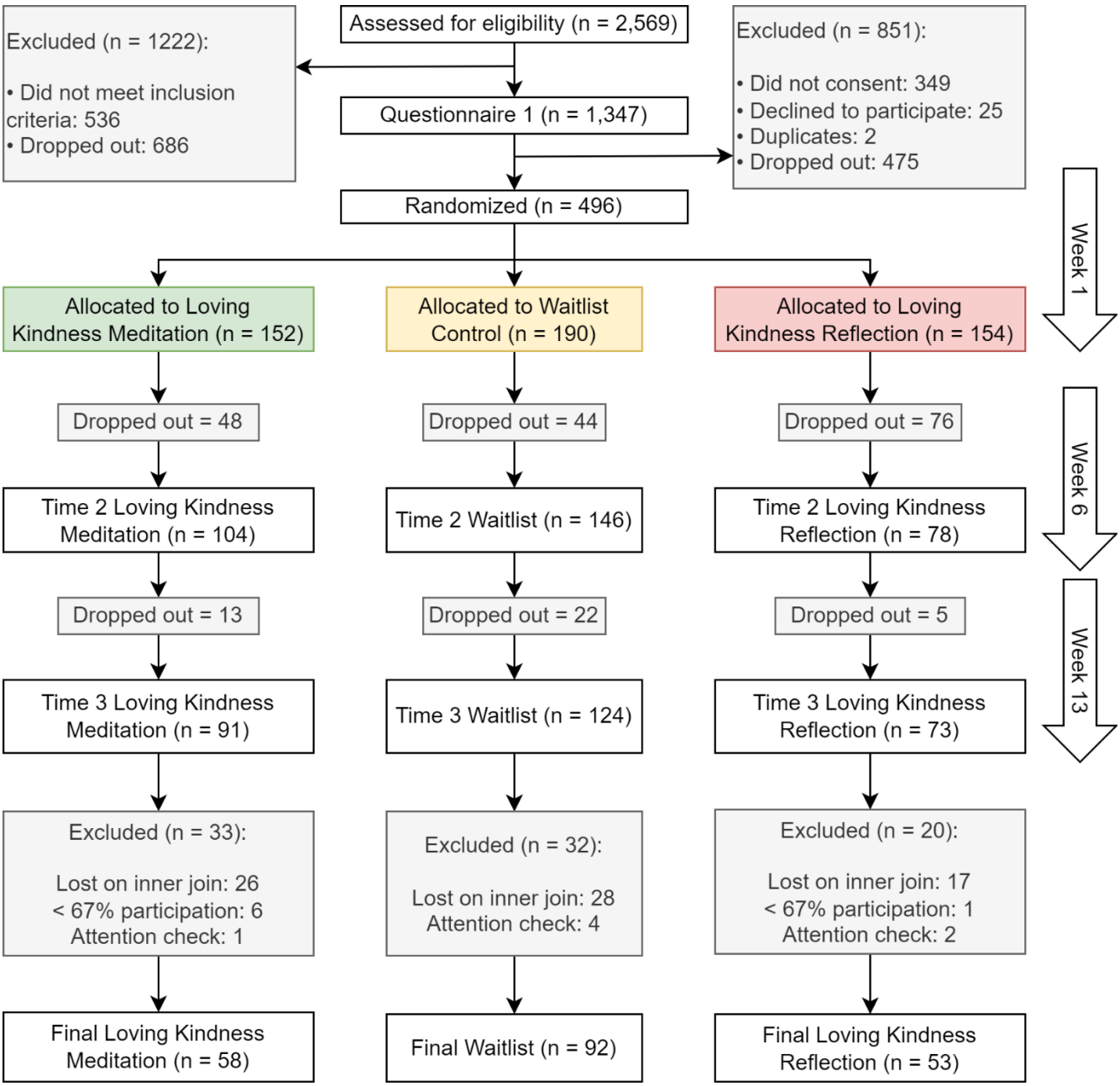
We recruited participants through online Facebook groups, as well as online and physical ads, in majority (but not exclusively) aimed at students in the greater Montreal area. Data collection started in October 2020 and ended in December 2022[1]. Our inclusion criteria required that participants (1) be between the ages of 18 and 35, (2) have normal or corrected vision, (3) have no history of psychiatric or neurological disorders, and (4) have no more than 10 hours of

[1] The Qualtrics platform experienced difficulties with the automated emailing system sending the daily exercises between February 24 and March 2, 2022, and emails scheduled to be sent were not sent automatically for those participants. Therefore, we provided participants with direct links to the exercises and encouraged them to fill those that were missed. Furthermore, due to an experimenter error in the randomization setting on the Qualtrics platform mid-study, some participants were only assigned to the control group over a period of approximately 3 months (February 2021 to May 2021), which explains why the control group has slightly more participants than the two other groups.

meditation experience, as significant experience could confound the interpretation of the results and reduce the potential for improvement following the intervention. The study was conducted in French, and participants were compensated up to $100 for full participation. From the 2,569 participants initially assessed for eligibility, a total of 203 participants were included in the final analyses (see Figure 1 for the participant flow diagram). Participant demographics for the final sample are available in Table 1. The ANOVAs suggest that the three groups were equal at baseline on age, whether they had followed a psychology course or not, and meditation experience. The reflection group, however, showed a lower proportion of women than the other two groups.

Figure 1

Participant Flow Diagram



Note: "Lost on inner join" refers to participants that were dropped when joining data sets from Time 1, 2, and 3 when participants were completely absent from one of these three time-measurement data sets.

Table 1

Sample Demographics Split by Intervention Group

Characteristic	Meditation (*N* = 58)	Reflection (*N* = 53)	Waitlist (*N* = 92)	F / χ^2	*df*	*p*
Age	25 (± 4.5)	25 (± 3.7)	24 (± 3.7)	0.89	2, 113.74	.413
Gender *				6.58	2	.0372
Female	48 (83 %)	37 (70 %)	80 (87 %)			
Male	10 (17 %)	16 (30 %)	12 (13 %)			
Already completed a psychology course				5.1	2	.0781
No	29 (50 %)	21 (40 %)	29 (32 %)			
Yes	29 (50 %)	32 (60 %)	63 (68 %)			
Meditation experience				0.75	2	.688
< 5 hours	46 (79 %)	40 (75 %)	75 (82 %)			
Between 5 and 10 hours	12 (21 %)	13 (25 %)	17 (18 %)			

Note. Differences are determined by one-way ANOVA or Pearson's χ^2-test. Groups differed on Gender at baseline as well as relative to how much they practiced meditation during the 13 weeks of the study.

Description of Groups

Loving-Kindness Meditation Group

Inspired by a recent study on loving-kindness meditation (Kang et al., 2014), participants in the meditation group listened to guided meditation recordings lasting an average of 30 minutes each week for six weeks. A certified loving-kindness meditation teacher guided the teachings in each recorded session according to the teaching tradition. The themes, in order of presentation, were: (1) loving-kindness toward oneself, (2) loving kindness toward a benefactor, (3) loving kindness toward a friend, (4) loving kindness toward a neutral person, (5) loving kindness toward a difficult person, and (6) loving-kindness toward all beings. The script of the guided meditations is available on OSF.

For each week/theme, participants were also asked to listen to a 10-minute abridged version of the week's 30-minute training. Participants listened to the same 10-minute abridged audio recording seven days in a row each for six weeks (thus, for a total of 42 days). A

stopwatch on the online platform made it possible to approximate treatment adherence by comparing the time spent on the platform to the total time of registration. An introductory video led by the teacher was also presented to participants at the beginning of the study.

Loving-Kindness Reflection Group

To mimic a reflection and discussion on loving-kindness topics, we converted the traditional teachings into audio podcast-like episode discussions on the six topics. Participants in the *reflection* group listened to audio podcasts of an average duration of 30 minutes once a week each week for six weeks. The episodes were hosted by the same teacher in the meditation group and a podcast facilitator (author S.D.). Each episode covered the week's central theme and followed the same themes of the meditation group, with some minor differences to accommodate for the '"discussion" nature of the format: (1) self-kindness, (2) types of love, (3) interdependence, (4) loving-kindness toward our enemies, (5) altruism, and (6) loving-kindness toward all beings. Podcast scripts are available on OSF.

In addition to listening to the week's audio episode, participants were asked to read texts on the week's loving-kindness topic *each day of the week.* Each reading exercise took about 10 minutes to complete. This was done to match the "10-minute/day" meditation required in the *meditation* group. Participants there read a total of 42 excerpts (7 days x 6 themes) of texts related to loving-kindness. The reading exercises also included a reflection question (included in the 10 minutes) that promoted cognitive engagement with the material. They were asked after each reading, "Briefly, what did you think of this excerpt? Do you agree? Do you disagree? What did you like, and what did you dislike?" The names of the nine books from which the extracts were taken and the link to the full extracts can be found on OSF. An introductory video

was also presented to participants at the beginning of the study, this time co-hosted by the teacher and podcast host.

Waitlist Control Group

This group did not receive any intervention. Participants in this group were informed that it is normal for them not to be able to complete the planned exercises and activities immediately but that they are still expected to complete them eventually.

Procedure

We randomly assigned participants to one of three groups, who were tested three times: time 1 (week 1) to provide a baseline, time 2 (week 7) to validate the pre-post effectiveness of the loving-kindness intervention, and time 3 (week 13) to assess its long-term effects. They were unaware of their experimental condition. Experimenters described the experiment as a general study of social preferences, thoughts, feelings, and economic decisions (to prevent social desirability or experimental demand characteristics).

At Time 1, after reading and accepting the consent form, participants completed the tasks in the following order: (a) trait self-control, trait aggression, normative beliefs toward aggression, explicit attitudes, and dehumanization, in random order; (b) implicit aggression; (c) behavioural aggression; and (d) intentions to help others and compassionate love, in random order. At Time 2, participants completed the tasks in the following order: (a) normative beliefs toward aggression, explicit attitudes, and dehumanization, in random order; (b) implicit aggression; (c) the cognitive fatigue manipulation (i.e., the Stroop-based ego depletion task); (d) subjective fatigue; (e) affect; (f) behavioural aggression; (g) cognitive accessibility (memory task); (h) intentions to help others; (i) compassionate love; (j) giving behaviour (charity task). Completing the same task repeatedly can lead to certain response biases called “practice effects”

(McCabe et al., 2011). We believe that these biases would be particularly important for charity, cognitive accessibility, and cognitive fatigue tasks; these tasks were, therefore, only used at Time 2. At Time 3, participants completed the tasks in the following order: (a) normative beliefs toward aggression, explicit attitudes, and dehumanization, in random order; (b) implicit aggression; (c) behavioural aggression; (d) intentions to help others; and (e) compassionate love.

Measures

The trait aggression, normative beliefs towards aggression and compassionate-love scales were translated into French.

Trait aggression. We used the *Brief Aggression Questionnaire* (ω = .79; 12 items; Buss & Perry, 1992). Item example: "Given enough provocation, I may hit another person" (1 – *extremely uncharacteristic of me* to 7 – *extremely characteristic of me*).

Implicit aggression. We used the *Implicit Association Test*, the aggression version, available on the Millisecond website[2] (e.g., Banse et al., 2015). This task is considered valid and reliable (Banse et al., 2015). The IAT is a categorization task in which participants are asked to sort stimuli by quickly pressing a key into two response categories: an object dimension (e.g., Self-Others) and an attribute dimension (e.g., Peaceful-Aggressive). The reasoning of the IAT is that the classification of two related objects results in faster reactions and a lower number of errors than the classification of unrelated objects (Banse et al., 2015). By combining the Self-Other dimension with an attribute dimension, the IAT can be used to determine whether the attribute dimension is associated with the participant's self-concept (Greenwald et al., 1998). The experimental blocks (3, 4, 6, 7) contain 20, 40, 20 and 40 trials, respectively, and the practice

[2] "Aggression IAT", available at: https://www.millisecond.com/download/library/iat/aggressioniat/

blocks (1, 2, 5) contain 20 tests each. A higher score on the Aggressiveness-IAT indicates a stronger association between the participant's sense of self and words related to aggression.

Aggressive behaviour (dependent variable). To measure (reactive) aggression, we used a modified version of Taylor's aggression paradigm (similar to Denson et al., 2010), available on the Millisecond website.[3] In this modified version, participants face off against a so-called virtual (computer-controlled) player in a reaction time task in which they must press a button as fast as possible when a target appears on the screen. On each try, the fastest player (the participant or their opponent) chooses the intensity and duration of an unpleasant sound to administer to the slower player. This task is considered valid and reliable (Chester & Lasko, 2019) and contains four blocks with 1, 8, 8 and 8 trials, respectively. A higher average volume and duration of the sound administered indicates more aggressive behaviour. For consistency with our previous research (Thériault & Dandeneau, 2023), we used the "normalized product of the averages" quantification strategy (i.e., the average *volume* of all 25 trials by the average *duration* of all 25 trials).

Normative beliefs about aggression. We measured normative beliefs toward aggression via the *Normative Beliefs about Aggression scale* (ωT1 = .85, ωT2 = .88, ωT3 = .89; 20 items; Huesmann & Guerra, 1997), adult version. Example item: "It is wrong to insult other people" (1 – *It's perfectly ok* to 4 – *It's really wrong*).

Explicit attitudes. We measured explicit attitudes toward various social groups via "warmth thermometers," sliding bars that range from 0 to 100 (e.g., like in Kang et al., 2014; ωT1 = .94, ωT2 = .96, ωT3 = .96; 9 items). Groups represented include Blacks, homeless,

[3] "Competitive Reaction Time Task (Complete) - English", available at: https://www.millisecond.com/download/library/competitivereactiontime/

Indigenous, Muslims, women, animals, seniors, and refugees (the scale also includes Whites for comparison purposes, but this group was not included in the scale average).

Dehumanization. We measured dehumanization through the *Dehumanization Scale* (ωT1 = .96, ωT2 = .97, ωT3 = .96; 9 items; Kteily et al., 2015). The groups are the same as for the scale of explicit attitudes.

Intentions to help others. We measured intentions to help others via a scale developed by DeWall et al. (2008; ωT1 =.67, ωT2 =.68, ωT3 =.69; 6 items). Example item: "Giving money to a homeless person" (1 – *Not at all likely to help* to 9 – *Very likely to help*).

Compassionate love. We measured compassionate love through *Compassionate Love Scale* (ωT1 = .94, ωT2 = .95, ωT3 = .95; 21 items; Sprecher & Fehr, 2005). Item example: "I feel tremendous compassionate love for people everywhere" (1 – *Not at all true of me* to 7 – *Very true of me*). We opted to use a measure of compassionate love, rather than other more established scales of compassion, as our focus was more on the "love" component than the "compassion" component; indeed, in French the practice is called and focuses on feelings of "benevolent love" ("amour-bienveillant") and not compassion per se, as loving-kindness meditation and compassion meditation are considered quite distinct practices in traditional Buddhist contexts (Zhou et al., 2023).

Manipulation of cognitive fatigue. We used a modified version of the colour-naming Stroop task (available on the Millisecond website[4] and (using the parameters specified by Schmidt et al., 2015) to manipulate cognitive fatigue (i.e., the "ego depletion" task). Different colour words (e.g., RED) appeared on the screen, and participants indicated the "ink" colour of the word (e.g., blue letters) by pressing the appropriate response key as quickly as possible. In

[4] "Color Word Stroop with Keyboard Responding - English", available at: https://www.millisecond.com/download/library/stroop/

the control group (low ego-depletion), all words were congruent; that is, the colour of the letters always corresponded to the written word (e.g., RED in red letters). In the experimental group (high ego-depletion), all words were incongruent; that is, the colour of the letters did not match the written word (e.g., RED in blue letters). This task has been validated in previous studies (e.g., Imhoff et al., 2014; Schmidt et al., 2015) and contains a practice block (12 trials) and an experimental block (180 trials). All participants were randomly assigned to either the experimental group or the control group. This task was completed only at Time 2, after four other measures that could act as predictors (normative beliefs toward aggression, explicit attitudes, and dehumanization, and implicit aggression) and before seven others conceptualized as direct outcomes (subjective fatigue, affect, behavioural aggression, cognitive accessibility, intentions to help others, compassionate love, giving behaviour).

Subjective fatigue. We measured subjective fatigue via the *5-item Brief State Self-Control Capacity Scale* (ω = .67; 5 items; Lindner et al., 2019). Example item: "I feel drained" (1 – *Not at all* to 5 – *A lot*).

Affect. We measured positive and negative affects via the *Positive and Negative Affect Schedule*, French version (ω positive affect = .86, ω negative affect = .82; 10 items; Gaudreau et al., 2006). Item example: "Angry" (1 – *Very slightly or not at all* to 5 – *Extremely or a lot*). This scale is exploratory and could mediate some of our effects.

Cognitive accessibility of memories. We measured the cognitive accessibility of aggressive and altruistic memories via a timed response task programmed in the Inquisit software. Participants were asked to remember the last time they engaged in aggressive behaviour and press the spacebar as soon as their memory was clear in their minds. They were also asked to remember an altruistic behaviour. The aggressive and altruistic trials were

counterbalanced across participants. Faster reaction times were used to indicate greater cognitive accessibility of that memory valence. Thus, greater cognitive accessibility will, in turn, be interpreted as an indicator of the chronic influence of the automatic system.

Charitable behaviour. We measured prosocial behaviour through a charity task (adapted from Böckler et al., 2016; Hare et al., 2010; Tusche et al., 2016; $\omega = .98$; 24 items). This task served as an indirect measure of altruism by taking the average (in percentage) of donations to each of the 24 charities/foundations presented. For each organization, a first screen presented a brief description of the organization: name, main purpose of the organization, target population, and logo. Participants were then asked to indicate with a sliding bar how much they would be willing to donate to this organization, up to a maximum of $10 from their total compensation. They were informed that only one of these choices would be selected and made at random. In this way, each potential donation to an organization was considered independent of the other organizations, and therefore, the only decision that would count would be the selected one. Participants also noted how familiar they were with each organization in a sub-question (from 1 – Not very familiar to 5 – Much familiar). We selected 24 organizations for their relative popularity in Québec, six for each level of governance (municipal, provincial, federal, and international)[5]. The charities were presented in random order.

[5] Those were: ***(a) municipal*** (Montréal: 1 – Moisson Montréal, 2 – Accueil Bonneau, 3 – Association de Montréal pour la déficience intellectuelle (AMDI), 4 – Centre d'amitié autochtone de Montréal (CAAM), 5 – Fondation du Centre des femmes de Montréal, 6 – Centre communautaire LGBTQ+ de Montréal), ***(b) provincial*** (Québec: 7 – Équiterre, 8 – Club des petits déjeuners du Québec, 9 – Fondation québécoise du cancer, 10 – Société Parkinson du Québec, 11 – Oxfam-Québec, 12 – Fondation Papillon), ***(c) federal*** (Canada: 13 – Croix-Rouge canadienne, 14 – Centraide, 15 – Société canadienne de l'autisme, 16 – Fondation David Suzuki, 17 – Conservation de la nature Canada, 18 – Fondation des maladies du cœur et de l'AVC du Canada), and ***(d) international*** (19 – UNICEF, 20 – Amnistie international, 21 – Greenpeace, 22 – Fonds mondial pour la nature (WWF), 23 – Médecins sans frontières (MSF), 24 – Armée du Salut).

Deviations from Preregistration

We deviated from the original preregistered protocol to align with best practices. Specifically, the preregistration mentioned we would only center variables and apply no other transformation. However, in the current paper, to align with best practices, we have additionally imputed missing data, transformed variables to normality, winsorized univariate outliers based on the median absolute deviation, and standardized variables (in this order). We report differences with the original protocol where appropriate.

Results

Data Analysis

We used a critical value of $p < 0.05$ with bilateral tests. We did not use corrections for multiple comparisons and instead left the interpretation of the results to the discretion of the reader (Althouse, 2016; Feise, 2002; Rothman, 1990).

Hypotheses 1 and 2. To compare the differences between the groups at Time 2, as described in the preregistration, we performed multiple regression analyses with planned contrasts, using R software, to test each hypothesis. The planned contrasts are:

(a) Loving-kindness meditation versus waitlist control group

(b) Loving-kindness reflection versus waitlist control group

(c) Loving-kindness meditation versus loving-kindness reflection

We used post (Time 2) scores as the dependent variable, while controlling for pre baseline scores (Time 1) and group interaction term as covariates in the regression model. (Yzerbyt et al., 2004). In the case of the charity task and the two cognitive accessibility tasks, the model does not include covariates for Time 1, as these measures were only administered once (at

Time 2) to reduce practice effects. Considering each combination of group comparison and dependent variable, there were 3 × 10 = 30 analyses in total.

Preliminary Analyses

We assessed the presence of differential attrition by group using Pearson's Chi-squared tests of independence. Results comparing participant counts between Time 1 (initial assessment) and Time 2 (post-intervention) suggest that the effect is statistically significant, and medium (χ^2 = 26.57, p = < .001; Adjusted Cramer's v = 0.22, 95% CI [0.14, 1.00]). Results comparing participant counts between Time 2 and Time 3 (follow-up) suggest that the effect is statistically not significant and very small (χ^2 = 3.57, p = .168; Adjusted Cramer's v = 0.07, 95% CI [0.00, 1.00]). These results suggest that the dropout rates of participants over time were not similar across the three groups from Time to Time 2, indicating differential attrition.

It is possible that, because of differential attrition, participants from each group had different characteristics. To answer this question, we used a logistic regression model to test whether group, age, or gender can predict dropout. The analysis suggests that the group and gender variables do not significantly predict dropout. However, age appears to be a significant predictor, with younger participants slightly more likely to drop out (b = -0.05, 95% CI [-0.10, -0.00], p = .034; b* = -0.20, 95% CI [-0.38, -0.02]). This suggests that the differential attrition noted initially may be partly driven by age-related factors rather than the intervention characteristics per se. Consequently, the observed group differences in dropout rates at the post-intervention phase might reflect underlying age distributions within these groups rather than the direct impact of the group interventions.

Finally, to provide a brief qualitative assessment of the reflection group's written reflections on the readings, as a sort of manipulation check, we produced a word cloud of the

positive and negative words used by participants. We first translated the list of words from French to English through Google Translate. The resulting Figure 2 suggests that participants from the loving-kindness reflection group properly engaged in the exercise, as the most commonly used positive (love, compassion, empathy, kindness) and negative (suffering, difficult, anger, enemies) words match the themes of the readings and intervention.

Figure 2

Word Cloud of Positive and Negative Words for the Reflection Group Readings Diaries

Group Differences at Time 1

Despite the differential attrition identified, ANOVA analyses presented in Table 2 suggest groups did not differ on variables measured at baseline (Time 1).

Table 2

Baseline Differences by Intervention Group

Characteristic	Meditation (N = 58)	Reflection (N = 53)	Waitlist (N = 92)	F / χ^2	*df*	*p*
BSCS	0.24 (± 0.97)	-0.12 (± 0.83)	-0.084 (± 1.1)	2.41	2, 200	.0923
BAQ	-0.23 (± 0.96)	0.20 (± 1.1)	0.030 (± 0.97)	2.57	2, 200	.0794
Attitude	0.18 (± 1.1)	-0.20 (± 0.94)	-0.0016 (± 0.98)	2.06	2, 200	.130
Dehumanization	0.11 (± 1.1)	-0.10 (± 1.0)	-0.0082 (± 0.96)	0.62	2, 200	.541
NOBAGS	-0.12 (± 1.0)	0.084 (± 1.1)	0.029 (± 0.92)	0.66	2, 200	.519
WHS	0.063 (± 1.1)	-0.18 (± 0.96)	0.067 (± 0.95)	1.23	2, 200	.295
CLS	0.13 (± 1.2)	-0.036 (± 0.98)	-0.061 (± 0.91)	0.69	2, 200	.504
Blast Intensity * duration	0.011 (± 0.95)	0.21 (± 1.1)	-0.13 (± 0.97)	1.89	2, 200	.154

Note. Differences are determined by one-way ANOVA or Pearson's χ^2-test.

Group Differences at Time 2

Multiple regression with planned contrasts analyses at Time 2 revealed that both the meditation and reflection groups showed moderately more compassionate love than the waitlist group, but that only the meditation group showed moderately more positive affect (exploratory

variable) than the waitlist group. However, the reflection group showed a little more positive explicit attitudes toward various social groups, as well as moderately shorter reaction times to remember an altruistic event than the waitlist group (suggesting that altruism was more cognitively accessible to them). Furthermore, the reflection group showed a little lower behavioural aggression than both the waitlist group and the meditation group. There were no other group differences in attitudes toward aggression, dehumanization, implicit aggression, or charity donations (Table 3).

Figure 3 displays group means over time (Time 1, Time 2, and Time 3), with 95% confidence intervals adjusted for within-subject variance as by the method of Morey (2008). For variables with no reference value at baseline (Time 1), we provide violin plots instead (Figure 4).

Table 3

Planned Comparisons at Time 2

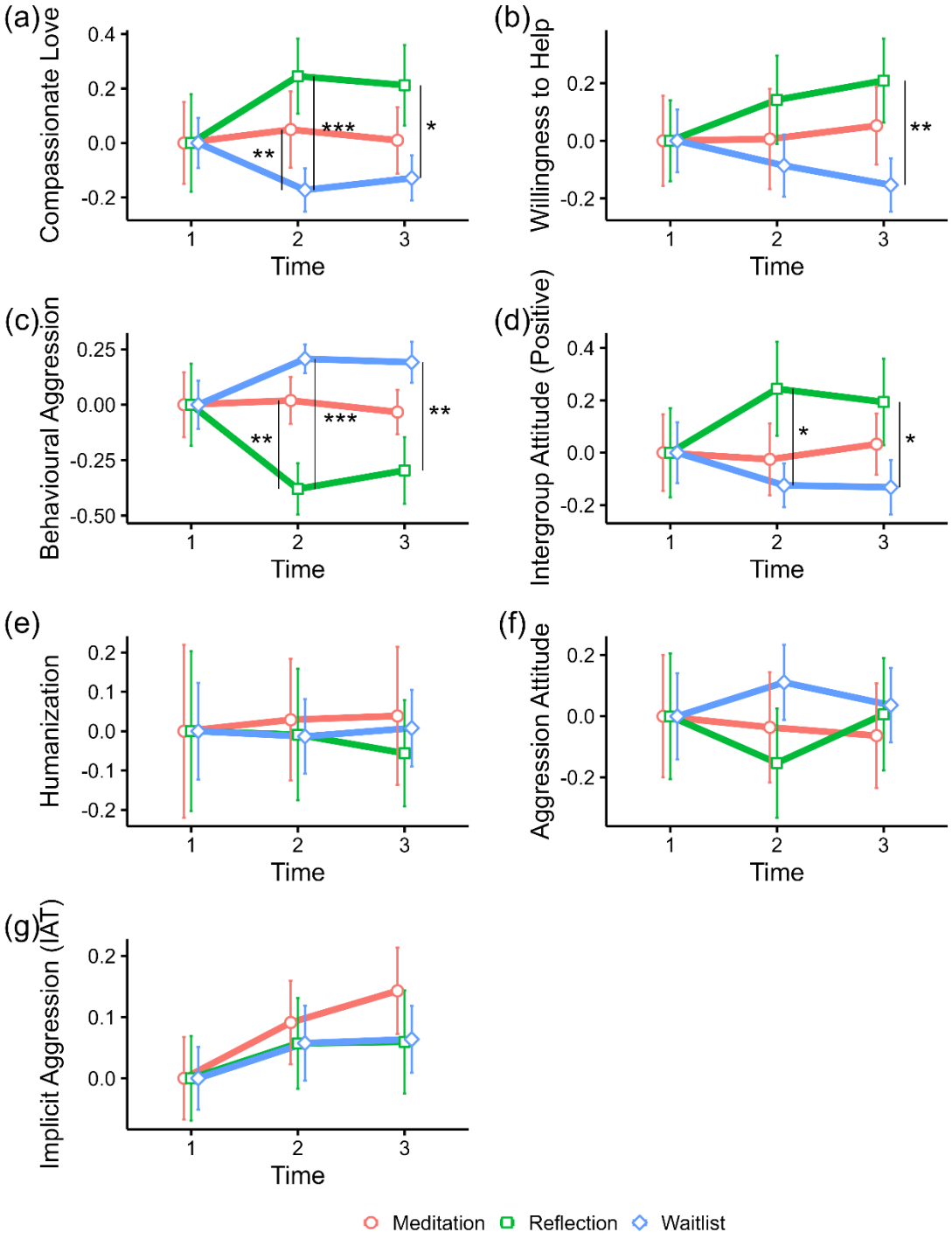
Dependent Variable	Comparison	*df*	*t*	*p*	*d*
NOBAGS	Meditation - Reflection	197	0.09	.930	-0.09
	Meditation - Waitlist	197	-1.57	.117	-0.30
	Reflection - Waitlist	197	-1.63	.105	-0.21
Attitude	Meditation - Reflection	197	-0.98	.327	0.11
	Meditation - Waitlist	197	1.29	.197	0.29
	Reflection - Waitlist	**197**	**2.34**	**.020***	**0.17**
Dehumanization	Meditation - Reflection	197	0.84	.400	0.25
	Meditation - Waitlist	197	0.80	.427	0.16
	Reflection - Waitlist	197	-0.16	.875	-0.09
IAT	Meditation - Reflection	197	-0.59	.554	-0.31
	Meditation - Waitlist	197	0.51	.611	0.08
	Reflection - Waitlist	197	1.14	.256	0.38
CRTT	**Meditation - Reflection**	**197**	**2.78**	**.006****	**0.20**
	Meditation - Waitlist	197	-1.44	.153	-0.05
	Reflection - Waitlist	**197**	**-4.43**	**< .001*****	**-0.25**
Memory (Altruistic)	Meditation - Reflection	200	1.79	.075	0.34
	Meditation - Waitlist	200	-0.52	.602	-0.09
	Reflection - Waitlist	**200**	**-2.48**	**.014***	**-0.43**
Memory (Aggressive)	Meditation - Reflection	200	0.59	.559	0.11
	Meditation - Waitlist	200	0.05	.962	0.01
	Reflection - Waitlist	200	-0.60	.550	-0.10
WHS	Meditation - Reflection	197	-0.45	.653	0.08
	Meditation - Waitlist	197	0.70	.484	0.07
	Reflection - Waitlist	197	1.17	.243	-0.01
CLS	Meditation - Reflection	197	-1.10	.274	-0.03
	Meditation - Waitlist	**197**	**2.46**	**.015***	**0.42**
	Reflection - Waitlist	**197**	**3.61**	**< .001*****	**0.45**
Charity	Meditation - Reflection	197	-0.38	.704	-0.09
	Meditation - Waitlist	197	-1.37	.174	-0.19
	Reflection - Waitlist	197	-0.90	.368	-0.11
PANAS (Positive)	Meditation - Reflection	200	1.39	.166	0.26
	Meditation - Waitlist	**200**	**2.76**	**.006****	**0.46**
	Reflection - Waitlist	200	1.15	.252	0.20
PANAS (Negative)	Meditation - Reflection	200	1.07	.285	0.20
	Meditation - Waitlist	200	1.62	.107	0.27
	Reflection - Waitlist	200	0.39	.696	0.07

Note. d = adjusted Cohen's d (adjusted for contrasts, but not covariates); NOBAGS = attitude toward aggression; IAT = implicit aggression; CRTT = Competitive Reaction Time Task (blast intensity × duration); WHS = Willingness to Help Scale; CLS = Compassionate Love Scale; PANAS = Positive and Negative Affect Schedule (this measure was exploratory).

* $p < .05$, ** $p < .01$, *** $p < .001$. Rows with grey shading indicate statistical significance.

Figure 3

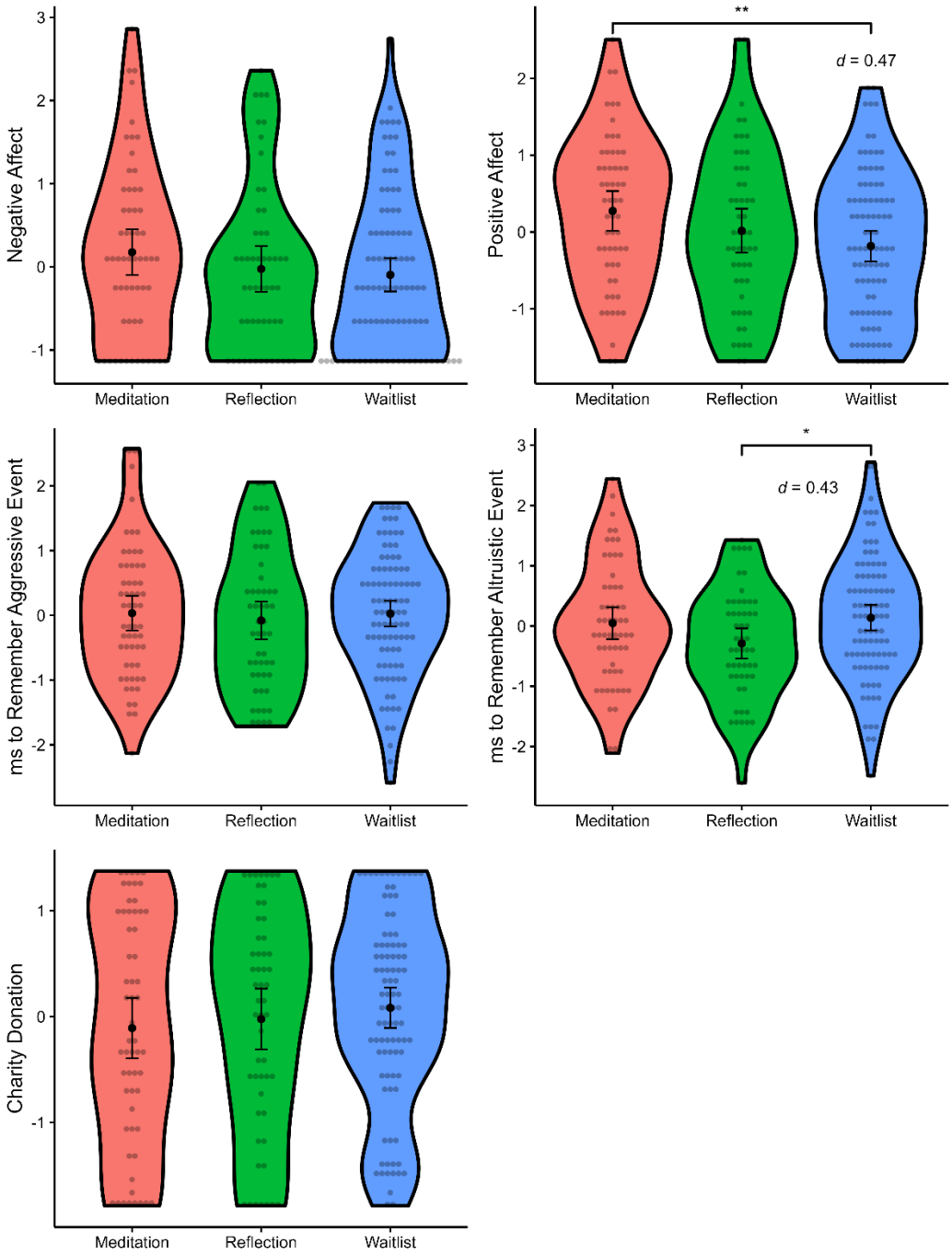
Plot of Group Means Over Time



Note: Group means over time (Time 1, Time 2, and Time 3), with 95% confidence intervals adjusted for within-subject variance as by the method of Morey (2008). Scores at Time 1 were subtracted from scores at Times 1, 2, and 3 for visualization purposes (change scores). *NOBAGS*: Normative Beliefs about Aggression scale; *IAT*: Implicit Association Test – Aggression.

Figure 4

Violin Plots of Variables Measured at Time 2 Only



Note: Dots = means; error bars = bootstrapped 95% confidence intervals; width = distribution density (frequency). * = $p < .05$; ** = $p < .01$. The meditation group shows higher positive affect than the waitlist group, and the reflection group shows faster reaction time to remember an altruistic event (suggesting higher cognitive accessibility) than the waitlist group.

Group Differences at Time 3

Hypothesis 3. We used the same approach to compare the differences between groups at Time 3. Considering each combination of group comparison and dependent variable, there were $3 \times 7 = 21$ analyses in total (because the charity and cognitive accessibility tasks were only administered at Time 2 to reduce practice effects).

Our contrasts analyses also revealed group differences at Time 3. However, only the reflection group showed lasting positive effects on attitudes (still a small effect), behavioural aggression (still a small effect), and compassion (still a moderate effect), suggesting these effects are durable in time (Table 4). Furthermore, the reflection group showed a delayed onset effect on willingness to help, whereas they were a little more willing to report intentions to help in various hypothetical scenarios than the waitlist group.

Table 4

Planned Comparisons at Time 3

Dependent Variable	Comparison	*df*	*t*	*p*	*d*
NOBAGS	Meditation - Reflection	197	-1.03	.306	-0.28
	Meditation - Waitlist	197	-1.22	.223	-0.25
	Reflection - Waitlist	197	-0.06	.955	0.03
Attitude	Meditation - Reflection	197	-0.32	.752	0.22
	Meditation - Waitlist	197	1.85	.066	0.35
	Reflection - Waitlist	**197**	**2.14**	**.034***	**0.13**
Dehumanization	Meditation - Reflection	197	1.29	.197	0.31
	Meditation - Waitlist	197	0.84	.401	0.15
	Reflection - Waitlist	197	-0.61	.541	-0.16
IAT	Meditation - Reflection	197	0.08	.938	-0.16
	Meditation - Waitlist	197	1.30	.196	0.20
	Reflection - Waitlist	197	1.15	.250	0.36
CRTT	Meditation - Reflection	197	1.35	.178	0.07
	Meditation - Waitlist	197	-1.49	.138	-0.09
	Reflection - Waitlist	**197**	**-2.91**	**.004****	**-0.15**
WHS	Meditation - Reflection	197	-0.95	.342	0.06
	Meditation - Waitlist	197	1.81	.071	0.18
	Reflection - Waitlist	**197**	**2.79**	**.006****	**0.12**
CLS	Meditation - Reflection	197	-1.15	.253	-0.04

	Meditation - Waitlist	197	1.71	.088	0.33
	Reflection - Waitlist	**197**	**2.94**	**.004****	**0.37**

Note. d = adjusted Cohen's d (adjusted for the contrasts, but not covariates); NOBAGS = attitude toward aggression; IAT = implicit aggression; CRTT = Competitive Reaction Time Task (blast intensity × duration); WHS = Willingness to Help Scale; CLS = Compassionate Love Scale.
* $p < .05$, ** $p < .01$. Rows with grey shading indicate statistical significance.

Deviations from Preregistration for Group Differences

There were some outcome differences when strictly following the preregistration plan of only centering variables (i.e., no imputation of missing data, no transformation, no winsorization, no exclusion of multivariate outliers, and no standardization). First, for attitudes toward various social groups, the pairwise contrast between the Reflection and Waitlist groups was not significant anymore at both Time 2 and Time 3. For compassionate love, the pairwise contrast between the Meditation and Waitlist groups at Time 3, which was only marginally significant (p = .07), now gained significance. Furthermore, in the preregistration, we specified the PANAS variable as exploratory; the corresponding pairwise contrast should thus be considered exploratory as well.

Moderation Analyses

Hypothesis 4. To assess the moderating role of ego-depletion between attitudes and prosociality (at Time 2), acknowledging group, we used moderated regressions (Aiken & West, 1991). Given each combination of type of attitude and dependent variable, there were 2 x 4 = 8 analyses in total.

We conducted moderation analyses looking at the interaction between the experimental group, level of ego depletion, and either *implicit* attitudes or *explicit* attitudes toward aggression. The 3-condition experimental group variable was transformed into two dummy-coded terms in R by treating the group variable as a categorical variable, with the Waitlist group as the reference

level. Accordingly, the first dummy variable (Reflection Group) tested for the effect of the Reflection condition vs. Waitlist, and the second dummy-coded variable (Meditation Group) tested for the effect of being in the Meditation condition vs. Waitlist. The ego-depletion variable was also automatically dummy-coded in R by treating the variable as a factor variable. Accordingly, the low-ego depletion group acted as the reference level.

The moderation analyses revealed a three-way experimental group × level of ego depletion × *implicit* attitudes interaction for the meditation group (vs. waitlist) and not for the reflection group, as hypothesized, but only on outcome measures of compassion and cognitive accessibility of a recent altruistic behaviour (The same moderation analysis with explicit attitudes toward aggression (NOBAGS) revealed non-significant experimental group × ego depletion × explicit aggression interactions on all of the outcome measures. Table 5 and Table 6, Figure 5), and not on behavioural aggression, charity, helping, or cognitive accessibility of a recent aggressive behaviour.

As shown in Figure 5, panel A, for the waitlist group, the effect of implicit aggression clearly depends on depletion: implicit aggression relates to lower compassion in the waitlist group (expected) but to higher compassion in the depletion group (unexpected). However, for the meditation group, the effect was absent or partly reversed. Finally, As shown in Figure 5, panel B, for the meditation group, higher implicit aggression relates to shorter reaction time (unexpected) unless they are depleted. However, for the waitlist group, the effect was absent or partly reversed. The same moderation analysis with *explicit* attitudes toward aggression (NOBAGS) revealed non-significant experimental group × ego depletion × explicit aggression interactions on all of the outcome measures.

Table 5

Moderation Analyses for Three-Way Interaction on Compassionate Love

Dependent Variable	Predictor	*df*	*b*	*t*	*p*	sr^2	95% CI
Compassionate Love	**GroupReflection**	**191**	**1.46**	**3.80**	**< .001*** **	**.06**	**[0.00, 0.13]**
	GroupMeditation	**191**	**1.45**	**2.99**	**.003****	**.04**	**[0.00, 0.09]**
	IAT	**191**	**-1.50**	**-3.57**	**< .001*** **	**.06**	**[0.00, 0.11]**
	ConditionDepleted	**191**	**1.17**	**2.80**	**.006****	**.03**	**[0.00, 0.08]**
	GroupReflection × IAT	**191**	**1.72**	**2.88**	**.004****	**.04**	**[0.00, 0.08]**
	GroupMeditation × IAT	**191**	**1.81**	**2.39**	**.018***	**.03**	**[0.00, 0.07]**
	GroupReflection × ConditionDepleted	191	0.09	0.13	.899	.00	[0.00, 0.00]
	GroupMeditation × ConditionDepleted	**191**	**-1.91**	**-2.62**	**.010****	**.03**	**[0.00, 0.07]**
	IAT × ConditionDepleted	**191**	**1.98**	**3.49**	**.001*** **	**.05**	**[0.00, 0.11]**
	GroupReflection × IAT × ConditionDepleted	191	-0.00	-0.00	.998	.00	[0.00, 0.00]
	GroupMeditation × IAT × ConditionDepleted	**191**	**-3.11**	**-3.00**	**.003****	**.04**	**[0.00, 0.09]**

Note. IAT = implicit aggression (Implicit Association Test). The three-way interaction between group, implicit aggression, and depletion is significant, but only for the meditation group relative to the waitlist, not for the reflection group.

We report the squared semi-partial correlation (sr^2), also known as the delta R squared (ΔR^2), as an index of effect size. The sr^2 allows us to quantify the unique contribution (proportion of variance explained) of an independent variable on the dependent variable beyond the other variables in the model. The sr^2 is often considered a better indicator of the practical relevance of a variable.
* p < .05, ** p < .01, *** p < .001. Rows with grey shading indicate statistical significance.

Table 6

Moderation Analyses for Three-Way Interaction on the Accessibility of Altruistic Memory

Dependent Variable	Predictor	*df*	*b*	*t*	*p*	sr^2	95% CI
Accessibility of Altruistic Memory	**GroupReflection**	**191**	**-0.94**	**-2.38**	**.018***	**.03**	**[0.00, 0.07]**
	GroupMeditation	**191**	**-1.07**	**-2.14**	**.034***	**.02**	**[0.00, 0.06]**
	IAT	191	0.25	0.57	.568	.00	[0.00, 0.01]
	ConditionDepleted	**191**	**-1.15**	**-2.68**	**.008****	**.03**	**[0.00, 0.08]**
	GroupReflection × IAT	191	-0.86	-1.39	.166	.01	[0.00, 0.03]
	GroupMeditation × IAT	191	-1.51	-1.94	.054	.02	[0.00, 0.05]
	GroupReflection × ConditionDepleted	191	1.33	1.73	.086	.01	[0.00, 0.04]

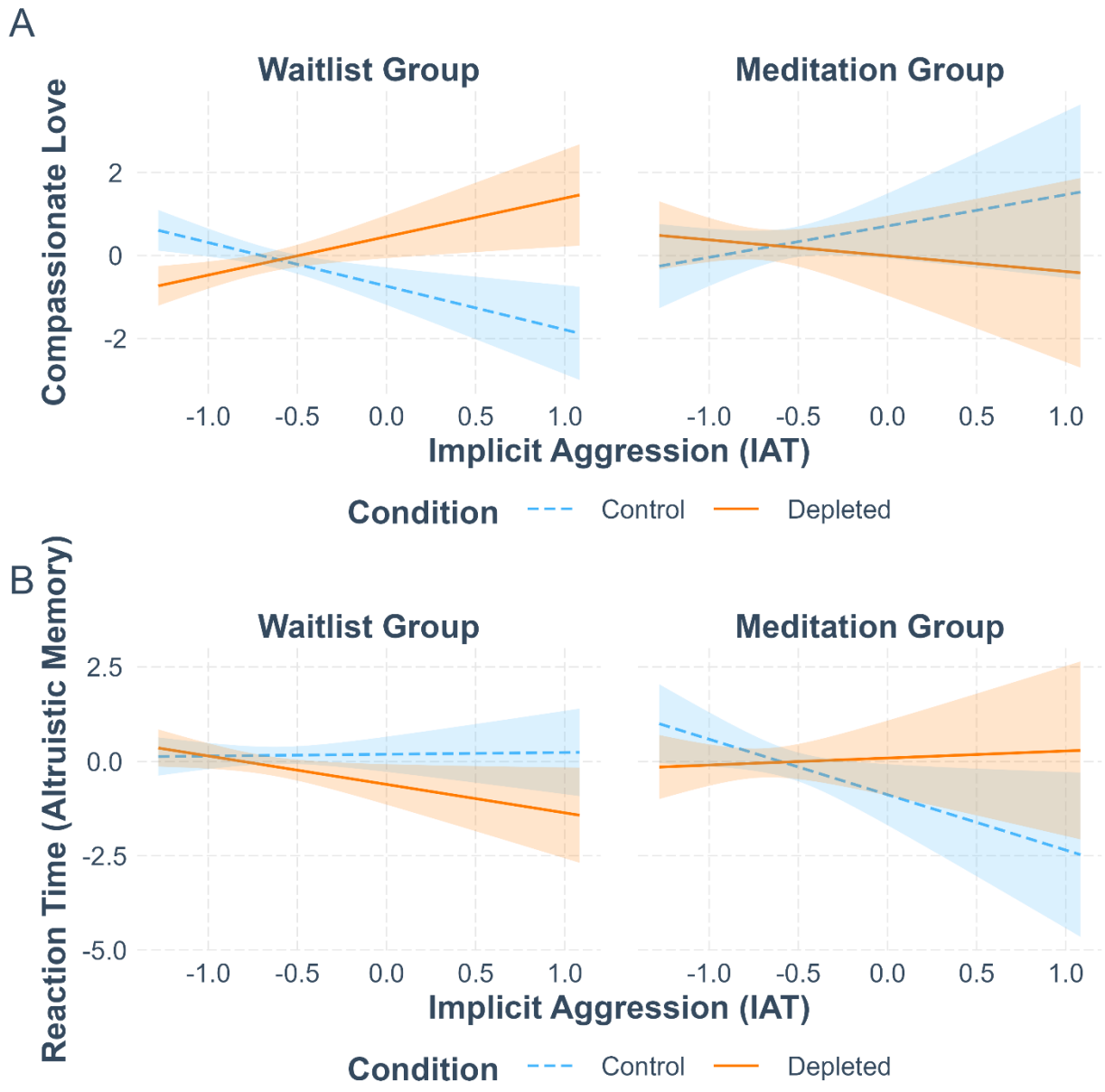
	GroupMeditation × ConditionDepleted	**191**	**1.80**	**2.40**	**.017***	**.03**	**[0.00, 0.07]**
	IAT × ConditionDepleted	**191**	**-1.31**	**-2.24**	**.026***	**.02**	**[0.00, 0.06]**
	GroupReflection × IAT × ConditionDepleted	191	1.93	1.80	.074	.02	[0.00, 0.05]
	GroupMeditation × IAT × ConditionDepleted	**191**	**2.50**	**2.34**	**.020***	**.03**	**[0.00, 0.07]**

Note. IAT = implicit aggression (Implicit Association Test). The three-way interaction between group, implicit aggression, and depletion is significant, but only for the meditation group relative to the waitlist, not for the reflection group.

We report the squared semi-partial correlation (sr^2), also known as the delta R squared (ΔR^2), as an index of effect size. The sr^2 allows us to quantify the unique contribution (proportion of variance explained) of an independent variable on the dependent variable beyond the other variables in the model. The sr^2 is often considered a better indicator of the practical relevance of a variable.
* $p < .05$, ** $p < .01$. Rows with grey shading indicate statistical significance.

Figure 5

Moderation Plots for Three-Way Interactions of Compassionate Love and Accessibility of Altruistic Memory



Note. Moderation Plots show two three-way interactions.

Discussion

In this research, we aimed to compare two types of loving-kindness interventions, a meditation-based practice and a more cognitive/reflection-based practice, to a waitlist control group. We compared those groups on several behavioural, explicit, and implicit prosocial variables. Groups were measured three times: Week 1 (T1), Week 7 (T2), and Week 13 (T3) to control for baseline but also to investigate their longer-term effects. We also explored whether these effects were moderated by relevant contextual moderator variables (i.e., cognitive fatigue). We hypothesized that the loving-kindness meditation and loving-kindness reflection groups would show greater prosociality relative to the waitlist control group. Specifically, higher charitable giving, compassionate love, intention to help others, and cognitive accessibility of altruistic memories, as well as lower aggressive behaviour, conscious and unconscious aggression, negative social attitudes, dehumanization, and cognitive accessibility of aggressive memories. We predicted that these differences would emerge immediately after the six-week intervention at Time 2 and that they would persist six weeks later.

Our results only partly confirmed our hypotheses. First, the meditation group showed both higher positive affect and compassionate love at Time 2 than the waitlist group, but the effect on compassionate love did not persist at Time 3. Second, the reflection group showed more positive explicit social attitudes, faster reaction times to remember a previous altruistic behaviour, higher compassionate love, and lower aggression at Time 2 than the waitlist group. The reflection group also showed lower aggression at Time 2 than the meditation group. At Time 3, the effects on positive explicit social attitudes, compassionate love, and aggression persisted for the reflection group, relative to the waitlist group, but not to the meditation group (for aggression). Furthermore, at Time 3, the reflection group showed a higher willingness to help,

relative to the waitlist group, a difference that was not present at Time 2, indicating a delayed onset effect.

In summary, these results suggest that whereas meditating on loving-kindness seems to increase compassionate love and positive affect, reflecting on loving-kindness appears to yield additional prosocial benefits, including behaviour (aggression), cognitive accessibility (of altruism), attitudes (toward social groups), and intentions to help. We summarize these findings in Table 7.

Table 7

Summary of Results and Hypotheses 1-3

Variable	Meditation	Reflection	Type	Effect Size	Hypotheses
Compassion	Yes*	Yes	Explicit	Moderate	H1+, H2+, H3-
Cognitive accessibility of altruistic memories	No	Yes	Implicit	Moderate	H1-, H2-, H3+
Aggressive behaviour	No	Yes	Explicit	Small	H1-, H2-, H3+
Explicit social attitudes	No	Yes	Explicit	Small	H1-, H2-, H3+
Willingness to help	No	Yes*	Explicit	Small	H1-, H2-, H3-
Cognitive accessibility of aggressive memories	No	No	Implicit	--	H1-, H2-, H3+
Conscious aggressiveness	No	No	Explicit	--	H1-, H2+, H3+
Unconscious aggressiveness	No	No	Implicit	--	H1-, H2-, H3+
Dehumanization	No	No	Explicit	--	H1-, H2+, H3+
Charity donations	No	No	Explicit	--	H1-, H2+

Note. * Effect lost or emerged at Time 3. The plus sign symbolizes that the hypothesis was confirmed; the minus sign that it was disconfirmed. H1 = hypothesis that the variable improved. H2 = hypothesis that meditation will improve more than reflection for implicit but not explicit variables. H3 = Changes will persist at follow-up (Time 3).

We had also hypothesized that an ego depletion task would influence the relationship between attitudes and prosociality, such that the reflection and waitlist groups would be less prosocial when depleted but that the loving-kindness group would not. We found that although

the ego depletion task did influence the link between implicit aggression and the two outcomes with a moderate effect size as a result of the intervention (compassion and cognitive accessibility of altruistic memories), the pattern of results was not as expected.

Overall Summary of Findings and Contributions to Literature

Overall, this study aligns with previous literature by showing that meditating on loving-kindness increases compassion and positive affect, but it does not reduce aggression. It also adds to the literature by showing that loving-kindness meditation does not reduce implicit aggression, nor does it make altruistic or aggressive memories more cognitively accessible. The evidence that loving-kindness generally improves *explicit* attitudes is weak; this study provides additional evidence that loving-kindness meditation does not improve explicit attitudes, specifically in terms of positive-negative evaluation and dehumanization of various social groups or regarding the social acceptability of aggressive behaviour. The evidence that loving-kindness generally improves *implicit* attitudes is also rather weak; this study provides additional evidence that loving-kindness meditation does not improve all implicit attitudes, particularly regarding implicit aggression. Finally, the evidence that loving-kindness generally increases altruism is mixed; this study provides additional evidence that loving-kindness meditation does not improve altruism, particularly in the form of charity donations or willingness to help.

This study further adds to the literature by showing the unique effects of a novel intervention, namely that reading texts and listening to podcasts about loving-kindness can help reduce behavioural aggression and explicit social attitudes and increase compassion, cognitive accessibility of altruistic memories, and willingness to help. Results show that for novice non-meditators, it may be more effective to develop a cognitive/reflective practice relative to attempting to install a more formal meditation-based practice. It could be because meditating is

hard, whereas reading texts and listening to podcasts are more common and popular activities but they are also considered by many as being more pleasant and easier. Indeed, many individuals must learn to read and listen at a young age, but they do not necessarily learn to meditate. This is especially relevant in the current study since participants could not, in fact, have any meaningful meditation experience to be eligible, but certainly, they had reading and listening experience (which was required to participate in the study). This is especially true given that the sample seemed to be particularly well-educated, with a majority of them having already taken psychology classes. In this sense, developing a formal meditation practice may not be strictly necessary to benefit from the prosocial benefits and teachings of the loving-kindness philosophy. With the advent of apps and training regimens trying to instil a sustained meditation practice, our results open the door to an alternative route to developing loving-kindness, namely a more cognitive/reflective approach that may be at least as beneficial.

The study also contributes to the literature on ego depletion by showing that specific cognitive training interventions, like loving-kindness meditation, can change the relationship between implicit attitudes (but not explicit attitudes) and explicit (self-reported) and implicit (reaction time) outcomes. This is the first study to show that this relationship can be changed through cognitive training. It also adds to the literature by showing that in control participants, the effects of implicit aggression are not always predictable or intuitive for self-reported (compassionate love) and reaction-time-based tasks (accessibility of altruistic memories). It adds to both the ego depletion and contemplative literatures by experimentally showing that loving-kindness meditators might overcompensate for their implicit aggression when in full control of their self-regulatory resources, but that this overcompensation dissipates once cognitively depleted, suggesting that this change results from a cognitive effort and that it is not automatized.

Perhaps this pattern of vanishing overcompensation upon depletion would disappear, for example, in long-term meditators (> 6 weeks), where it could have had time to become automatized and, as such, would not have to rely on cognitive control anymore.

Next, we unfold those results by diving deeper into the direct effects of the meditation and reflection interventions, as well as the moderating effects of ego depletion.

Effects of Loving-Kindness *Meditation*

At Time 2, the meditation group showed moderately more compassionate love and positive affect (an exploratory variable) than the waitlist group (hypothesis-consistent). We did not measure positive affect at Time 3, so it is not possible to know whether this effect persisted in time. However, we know that the effect on compassionate love was lost at follow-up (Time 3) for the meditation group, though we hypothesized the effects would persist. Furthermore, for both Time 2 and Time 3, contrary to our hypotheses, there were no effects of the meditation group on explicit attitudes toward aggression or various social groups, dehumanization, implicit attitudes toward aggression, aggressive behaviour, cognitive accessibility of altruistic or aggressive memories, willingness to help, charity donations, or negative affect.

Affect and Compassion

The effect of loving-kindness meditation on positive affect, as well as its lack of effect on negative affect, is well-known, as suggested by meta-analyses and later studies (Fredrickson et al., 2017; Galante et al., 2014; He et al., 2015; Sirotina & Shchebetenko, 2020; Zeng et al., 2015), though some studies find the opposite pattern (e.g., Liu et al., 2022) or no effect at all (Galante et al., 2016; Rizzato, 2014). The effect on compassionate love is also consistent with previous literature on compassion, a related concept and a more specific type of positive affect

(Carrero et al., 2023; Galante et al., 2014; Hildebrandt et al., 2017; Jazaieri et al., 2013; but see Fredrickson et al., 2008; Kreplin et al., 2018).

Implicit Attitudes

The lack of effect on *implicit* attitudes at first appears to contrast with previous research showing an improvement of implicit attitudes following a loving-kindness meditation intervention (Hutcherson et al., 2008; Kang & Falk, 2020; Kang et al., 2014; Stell & Farsides, 2015). However, past studies investigated implicit bias toward specific social groups (i.e., Black individuals, homeless individuals, substance users, or neutral strangers) instead of implicit *aggression*, in our case. It is possible that loving-kindness meditation changes our implicit attitudes relative to social groups but not one's implicit aggression in general. Indeed, at least one loving-kindness study failed to find effects on implicit positive or negative bias toward others (Law, 2012); another found implicit effects only for the target of the visualization but not for non-targets (Hutcherson et al., 2008); and yet another failed to find any implicit effects toward politicians (Schroter & Jansen, 2022). Furthermore, one study that found a reduction of implicit bias against stigmatized individuals in the loving-kindness condition only found a marginally significant effect (Kang & Falk, 2020).

Explicit Attitudes

The absence of effects on *explicit* attitudes (of aggression, dehumanization, and various social groups) also seems to be in contrast with the results of a recent meta-analysis investigating the effect of loving-kindness meditation on negative interpersonal attitudes (Zhou et al., 2023) as well as other studies showing its effects on explicit positive attitudes toward neutral strangers (Hutcherson et al., 2008), homeless people (Parks et al., 2014), or others in general (Kang et al., 2015). It is possible that explicit attitudes toward aggression and specific social groups (in terms

of positive feelings and dehumanization) behave differently than attitudes toward neutral strangers. This idea is supported by studies that have failed to find the effects of loving-kindness meditation on explicit attitudes toward Black individuals, homeless people (Kang et al., 2014), or politicians (Schroter & Jansen, 2022).

Furthermore, the only study that found reductions in explicit prejudice toward a specific group (homeless people) actually provide conflicting results: only the orthogonal planned contrast was significant, but not the *t* tests (Parks et al., 2014). Although the authors report both tests, this study was actually coded as having no effect on attitudes in another meta-analysis (Kreplin et al., 2018). Finally, another study that found an increase in positivity toward others in the loving-kindness group only found a marginally significant effect and found no effect at all regarding negativity toward others, suggesting that the effect may be specific to positive and negative affective associations (Kang et al., 2015). However, we note that for our measures of explicit attitudes and dehumanization, we cannot categorize them as clearly positive or negative since those scales used a slider (thermometer) that could range from one end to the other.

Altruism

The absence of effect on altruism-related variables (charity donations, willingness to help, time to remember a previous altruistic behaviour) at first also appears to contrast with previous literature investigating loving-kindness or compassion meditation interventions (Ashar et al., 2016; Böckler et al., 2018; Condon et al., 2013; Galante et al., 2016; Leiberg et al., 2011; Liu et al., 2022; Weng et al., 2015; Weng et al., 2013). Of particular relevance is a similar online loving-kindness meditation randomized controlled trial that found that participants from the meditation group were marginally more likely to donate to a charity than participants following a light physical exercise course (Galante et al., 2016). However, in this case, the effect on donation

did not actually reach conventional significance (only “marginal significance”), just like in one other such study looking at helping behaviour (Condon et al., 2013). Other studies found no effect at all on prosocial behaviour, such as on offering one’s seat to a rude experimenter (Condon, 2014, Study 2), the number of experiments volunteered (Law, 2012), or the amount donated to a car accident victim (Mascaro, 2012)—though this last study suffers from low statistical power. In another (underpowered) study, children in the loving-kindness group actually gave less than the control group (Frazier-Meyers, 2017).

Aggression

We also find that contrary to our hypotheses, loving-kindness meditation did not decrease aggressive behaviour. This finding generally supports the results of a meta-analysis that demonstrated that loving-kindness meditation and mindfulness interventions generally increase compassion and empathy but do not decrease aggression (Kreplin et al., 2018). Interestingly, although many studies have looked at the effects of *mindfulness* on aggression (Gillions et al., 2019; Tao et al., 2021), few have looked at the effects of *loving-kindness* on aggression. We could only identify one such study that compared mindfulness, loving-kindness, and a control group without intervention on the same behavioural aggression task used in the current study (i.e., using sound blasts). Consistent with the results of the current study, the researchers found no differences between groups (Keng & Tan, 2018). Besides, one underpowered study from an unpublished thesis found that loving-kindness meditation, relative to an active comparison group, did not lead to a reduction in teacher-reported problem behaviours—which included aggression—in second and third-grade students (Collins-McHugh, 2016).

Of relevance, one meta-analysis found that loving-kindness tends to reduce negative interpersonal attitudes (Zhou et al., 2023). Of the eight randomized control multiweek loving-

kindness or compassion-based meditation intervention studies included, six of them looked at anger or hostility as outcome variables—concepts related to aggression. Looking at these individual studies more closely, only one appears to find some indirect association between loving-kindness meditation practice and reduced anger on the next day (Carson et al., 2005). However, manually performing paired *t* tests from the reported means and standard deviations, we see that both the intervention and control groups showed no significant changes from pre to post. The five other studies from this list also do not find a significant direct effect of loving-kindness meditation on anger (Condon, 2014, Study 2; Frazier-Meyers, 2017; Herriman, 2019; Lang et al., 2019; Poehlmann-Tynan et al., 2020).

Effects of Loving-Kindness *Reflection*

At Time 2, the reflection group showed moderately more compassionate love than the waitlist group, moderately shorter reaction times to remember an altruistic event (suggesting that altruism was more cognitively accessible to them), and a little more positive explicit attitudes toward various social groups (hypothesis-consistent). The reflection group also showed a little lower behavioural aggression (blast intensity × duration) not only compared to the waitlist group (hypothesis-consistent) but also relative to the meditation group (hypothesis-inconsistent). However, we had hypothesized that the reflection (and meditation) group would show changes in measures of controlled behaviour, such as behavioural aggression. We had also hypothesized that the reflection group would show smaller improvements in implicit measures than the meditation group, such as the reaction-time-based altruistic memory retrieval task, but the effect surprisingly came up only for the reflection group. We instead find that the reflection group has influenced both implicit and explicit measures (whereas the meditation group only influenced explicit measures).

We had hypothesized that these changes would persist at Time 3, and they did: the reflection group showed lasting positive effects on attitudes (still a small effect), behavioural aggression (still a small effect), and compassion (still a moderate effect), suggesting these effects are somewhat durable in time. Furthermore, the reflection group showed a delayed onset effect on willingness to help. Specifically, they were a little more willing to report intentions to help in various hypothetical scenarios than the waitlist group, but this effect only emerged at Time 3. It is possible that the continued effects of the reflection intervention are durable and continue to influence people's thought processes for some time after the intervention.

The unexpected differences between the meditation and reflection groups raise many questions. One large-scale 9-month intervention study (i.e., the *ReSource Project*) found that an intervention targeting a so-called warm affective system (i.e., the Affect Module) increased altruistically motivated behaviour, whereas an intervention targeting a so-called cold cognitive system (i.e., Perspective Module) did not (Böckler et al., 2018). The authors note that their results are surprising given "traditional neoclassical economic views advocating that cognitive understanding and rational thinking is the most promising route to increased cooperation" (Böckler et al., 2018, p. 8). With our findings, we show that the debate about affective and cognitive systems for personality and behavioural change is not over.

The fact that participants in the reflection group showed more compassionate love (at Time 2) and more willingness to help (at Time 3) but no higher charity donations is also interesting. On the one hand, this could be seen as support for the relative independence of attitudes and behaviours. On the other hand, it could also be an artifact of the task itself. Indeed, perhaps monetary donations constitute a very specific form of altruism that is not generalizable to other types of behaviour. One of our participants, for example, noted that "The generosity of

which we are capable is a matter of the heart; there are other ways to be generous than with money." Therefore, perhaps a charity task does not properly capture the way in which the intervention made them feel more altruistic.

Comparisons with Reflection Groups in Other studies

Few meditation studies have used discussion or reflection interventions as an active control group. The current study was itself inspired by a series of studies that compared loving-kindness interventions to loving-kindness discussion groups (Kang et al., 2014; Kang et al., 2015). The only measures shared (and therefore that can be compared) between the Kang studies and the current study is that of explicit and implicit attitudes, although even those measures differ somewhat. The fact that our reflection group affected both explicit and implicit measures contrasts with the Kang studies, which demonstrated that a discussion intervention on loving-kindness (from which our reflection intervention is inspired) did not diminish explicit and implicit prejudices, or explicit positive attitudes toward others, relative to a control group.

We note that the reflection group in the current study differs in several respects to these previous discussion groups, which might in part explain the differences in results. First, the readings from the Kang studies consisted of very short summary notes in preparation for the first group discussion (week 1), a short Buddhist poem or story (weeks 2, 4, and 6), with each text taking perhaps 30 seconds to two minutes to read, or short video presentations by Buddhist figures (weeks 3 and 5). In their discussion, Kang et al. (2015, p. 1068) note,

> First, methodologically, it is hard to equate the discussion and meditation interventions for time, effort, and activity, because the discussion course did not include comparable home practice activities as in the meditation course. Future studies may include homework assignments for the discussion course, such as reading and/or contemplating activities on lovingkindness meditation.

Therefore, in the current study, we have attempted to equate the discussion and meditation interventions for time, effort, and activity by including homework assignments consisting of readings and podcasts on the theme of loving-kindness. In contrast, participants in the current study had to read new and much longer (equivalent of approximately 10 min daily) book excerpts on the topic of loving-kindness every *day*, instead of once a week. The amount of commitment and cognitive effort required from participants must thus have been far greater in the current study.

Second, the discussion intervention in the Kang studies included weekly group discussion classes, which our reflection group did not have. Instead, our reflection group passively listened to podcasts about the week's readings, additionally allowing a better conceptual integration of the book excerpts' lessons. Finally, the Kang studies were all conducted in person, whereas our study was fully online.

The Moderating Role of Ego Depletion

We had originally hypothesized that an ego depletion procedure would influence the link between attitudes and prosociality (i.e., it would act as a "boundary condition"), such that being ego-depleted would increase the predictive power of implicit attitudes and reduce that of explicit attitudes (Friese et al., 2008; Hofmann et al., 2009). We expected that this would only be true of the reflection and waitlist groups. Specifically, we speculated that the reflection and waitlist groups should be less prosocial when depleted (regarding aggressive behaviour, charity donation, compassion, and willingness to help). Because the loving-kindness meditation group would have more prosocial implicit inclinations (aligned with their explicit inclinations), this group should have remained equally prosocial whether participants were depleted or not—that is, there would be no moderation for this group. Our results suggest that implicit aggression influenced implicit

attitudes for two variables of interest: compassion and cognitive accessibility of altruistic memories.

Implicit Attitudes and Compassion

Implicit aggression influenced compassionate love in a three-way interaction in the meditation vs waitlist contrast (IAT × Meditation Group × Depletion). There was no three-way interaction in the reflection vs waitlist contrast (IAT × Reflection Group × Depletion), as expected. We had hypothesized an interaction in the waitlist group but not in the meditation group, and this pattern correctly emerged. However, the direction of the results is unexpected. Results suggest that for the waitlist group, the effect of implicit aggression clearly depends on depletion: implicit aggression relates to lower compassion in the control condition but to higher compassion in the depletion group. These results are unexpected because, based on previous literature, ego depletion should *increase* the steepness of the slope of implicit attitudes and not really change the direction of the slope completely (Friese et al., 2008; Hofmann et al., 2009). Furthermore, implicit attitudes should only show a significant slope when depleted for controlled responses such as self-report questionnaires, whereas it is not the case here. Moreover, for the meditation group, this interaction effect was absent or even partly reversed.

These results suggest that implicit aggression actually predicts compassionate love *negatively* and that cognitive fatigue actually makes implicit aggression predict compassionate love *positively*. This result is difficult to reconcile with previous literature, given that we would expect higher implicit aggression to relate to lower, not higher, compassionate love. Given that this was a self-reported questionnaire, it is possible that the cognitive fatigue state made participants less accurate at introspection and, therefore, made them see themselves as more compassionate than they really are, strengthening so-called positive illusions about the self like

the self-enhancement bias (Brown, 1986; Paulhus et al., 1989). However, current literature suggests that the opposite happens: people show weaker positive illusions after ego depletion (Fischer et al., 2007).

This interaction between implicit aggression and compassionate love was mostly suppressed in the meditation group. It is possible that participants who received a loving-kindness intervention (meditation or reflection) may be less vulnerable to ego depletion than those who did not. The results of a loving-kindness meditation intervention provide support for this idea (Hunsinger et al., 2013). In this study, individuals who participated in three sessions of loving-kindness meditation demonstrated greater cognitive control than those who received no intervention. Indeed, those who meditated were faster at a Stroop task, in which the participant must select the colour of the text (rather than its meaning). This finding suggests that, although the main goal of loving-kindness meditation is the development of positive feelings, this practice may have beneficial effects on the regulation of cognitive resources. Thus, individuals practicing loving-kindness meditation may have a greater capacity for cognitive control and, consequently, be less susceptible to the effects of ego depletion.

Nonetheless, something can also be said of the pattern of partial reversal in the meditation group. When they are not depleted, for participants in the meditation group, implicit aggression positively predicted compassionate love, indicating that due to their loving-kindness meditation practice, they may overcompensate for their implicit aggression. However, this effect is lost after depletion, where implicit aggression loses its predictive power and participants stop overcompensating.

Implicit Attitudes and Cognitive Accessibility of an Altruistic Memory

Implicit aggression also slightly moderated accessibility of a personally relevant altruistic event, again in a three-way interaction (IAT × Meditation Group × Depletion). There was no three-way interaction in the reflection vs waitlist contrast (IAT × Reflection Group × Depletion), as expected. We had hypothesized an interaction for those in the waitlist group but not for those in the meditation group, but this time, the opposite pattern emerged, and the specific results were still unexpected. They suggest that for the waitlist group, the depletion procedure seems to have little effect on the accessibility of a recent altruistic event. These results are unexpected because, again, based on previous literature, ego depletion should increase the steepness of the slope of implicit attitudes, not really change the direction of the slope completely (Friese et al., 2008; Hofmann et al., 2009). Moreover, implicit attitudes should show a significant slope when depleted for automatic responses such as implicit measures based on reaction times, whereas it is not the case here. The direction of the depletion effect is also unexpected: whereas the slope of implicit aggression on implicit altruism is flat for the control condition (expected), it is negative for the depletion condition. This suggests that implicit aggression, once depleted, makes altruistic memories more cognitively accessible, a somewhat counterintuitive result.

This trend is reversed for the meditation group: in this case, higher implicit aggression relates to shorter reaction times unless they are depleted. This result is again somewhat unexpected, given that we would have expected the two slopes to be similar. Furthermore, were the two slopes not to be similar, it would be more intuitive that they would be swapped so that implicit attitudes gained predictive power when depleted rather than lose it. These results suggest that, for most people, implicit aggression does not predict the cognitive accessibility of altruistic memories and that cognitive fatigue does not change that. However, they also suggest that things

are different for people who practice loving-kindness meditation: in this case, implicit aggression does predict the cognitive accessibility of altruistic memories unless they are depleted.

As in the case of the compassionate love variable, it seems that when they are not depleted, for participants in the meditation group, implicit aggression positively predicted cognitive accessibility of altruistic memories, indicating that due to their loving-kindness meditation practice, they may overcompensate for their implicit aggression. However, this effect is lost after depletion, where implicit aggression loses its predictive power and participants stop overcompensating.

Other Variables

No effects were found for implicit attitudes and the other variables, and explicit attitudes did not act as moderator at all. That said, given that we only found moderate effect sizes for the variables of compassion and cognitive accessibility of altruistic memories (see Table 7), it is in some sense not surprising that we found moderations solely for those two variables. At the same time, it is also possible that the lack of effect on these variables emerges from the lack of reliability of the ego depletion effect, as the concept has been vividly contested over the last decade (Dang et al., 2020; Friese et al., 2018; Hagger et al., 2016; Lurquin et al., 2016; Vohs et al., 2021).

Dual Model of Impulse and Self-Control

This paper contributes to the literature on ego depletion and cognitive fatigue (Hofmann et al., 2009), by showing that specific mental training interventions, such as loving-kindness meditation, can change the relationship between implicit attitudes (but not explicit attitudes) and both explicit (self-reported) and implicit (reaction time) outcomes. This is the first study to show that this relationship can be modified through mental training. It also adds to the literature by

showing that among control group participants, the effects of implicit aggression are not always predictable or intuitive for both self-reported (loving-kindness) and reaction time-based (accessibility of altruistic memories) tasks.

The paper adds to both the ego depletion and loving-kindness literatures by experimentally showing that loving-kindness meditators can overcompensate for implicit aggression when they are in full control of their self-regulatory resources, but that this overcompensation dissipates once they are cognitively depleted, suggesting that this shift is the result of cognitive effort and is not automatized (Hofmann et al., 2009). Perhaps this pattern of disappearance of overcompensation during exhaustion would disappear, for example, in long-term meditators (> 6 weeks) where chronic exposure to loving-kindness would have had more time to fully automate this process, so that individuals would no longer have to rely on their self-control and cognitive resources.

Limitations

This study has many limitations. Like many studies, the sample comprised a large majority (81%) of females. Many participants had also already completed a psychology course (61%). This suggests that the participants are mostly students with a fairly high level of education. It could also suggest that participants already had some interest in the human experience, introspection, and reflection, which perhaps made them more open and supportive of the intervention's approach. This education level might in particular have made them more familiar with reading and reporting their thoughts and reflections, which could perhaps in part explain the current results.

Several participants declared having practiced meditation over the 13 weeks of the study, even though they were not instructed to do so: 43% in the reflection group and 30% in the

waitlist control group. Investigating by meditation type, we find that these numbers go decrease substantially if we filter for the specific type of meditation used in this study (i.e., loving-kindness, instead of, e.g., mindfulness): 15% in the reflection group, and 5% in the waitlist control group. Even though these numbers are a lot lower, the fact that the reflection and waitlist groups still had some participants practice the intervention intended for the other group probably adds additional noise to the findings.

There were high attrition rates from Time 1 to Time 2, especially in the reflection group (49%), but even in the meditation (32%) and waitlist groups (23%), and a Chi-squared test confirmed the presence of differential attrition, even though dropouts were lower from Time 2 to Time 3 (reflection group: 6%, meditation: 15% and waitlist groups 13%, no differential attrition). It is possible that participants in the reflection group had higher dropouts at first because of the highly demanding nature of the intervention, which required higher-order cognitive processing and effort (such as through using critical thinking skills when assessing the readings). That said, our dropout rates were relatively low compared to a very similar online loving-kindness meditation intervention, which had an 82% dropout rate (Galante et al., 2016). Nonetheless, it is possible that this differential attrition reflects that participants who left the study may have different characteristics than those who did not (e.g., lower motivation or self-control), reducing the potency of random assignment and, thus, affecting the internal validity of the research.

Future online interventions could benefit from certain strategies to limit attrition, such as early warning about tasks requested, personalization (obtaining individual information), and an appeal to awareness (specifying to participants that attrition affects data quality; Zhou & Fishbach, 2016). In order to reduce self-selection bias, the current study recruitment strategy took special care to avoid any references to meditation, loving-kindness, and related terms, and

only generically described the study as a "longitudinal study of personality, emotions, decision making and interpersonal behaviors" (translated from French). However, it is possible that this in turn led to more dropout once participants had a better understanding of their respective intervention.

Although we aimed to measure compassionate love rather than compassion per se, it might have been informative to use better compassion and self-compassion scales rather than the *Compassionate Love Scale*. Furthermore, it might also have been informative to include measures of trait mindfulness and trait interpersonal mindfulness. As well, the real-life implications of the current study may be somewhat limited given that most valid loving-kindness or compassion training programs include both practice and reflection around loving-kindness.

Special limitations apply to the issue of context and more specifically to the experimental manipulation of cognitive fatigue ("ego depletion"), since the concept is contested (Dang et al., 2020; Friese et al., 2018; Hagger et al., 2016; Lurquin et al., 2016; Vohs et al., 2021) and that unpublished pilot studies have revealed difficulties in developing a task that creates real and reliable cognitive fatigue, even using previously validated tasks. It is thus possible that the limited effects of our cognitive fatigue task are due to the fact that it was not particularly effective in our participants. Future studies addressing the question of the effect of cognitive fatigue should agree on the best task to use, ideally determining the gold standard in the field.

Future research would benefit from directly testing the question of the role of dosage, for example by experimentally manipulating the level of exposure to the treatment (meditation or reading). Alternatively, it might also be possible to use advanced statistical methods such as the Complier Average Causal Effect (CACE), also known as the Local Average Treatment Effect (LATE), to test the influence of treatment adherence, since presumably, participants who

meditate or reflect more on loving-kindness should see larger effects (Sagarin et al., 2014). Survival analyses would also be relevant to assess how differential attrition evolves over time and across experimental group.

While we partly explain the differences between the meditation and reflection groups by the level of difficulty of the practices and their popularity among a highly educated population, it would be relevant in future studies to test this hypothesis by including questions directly addressing the perceived level of difficulty of the activities, immediately after each activity. On the one hand, this would allow us to calculate an average level of difficulty per activity/text and to adapt the activities or texts accordingly. On the other hand, this would also allow us to establish an average level of difficulty per intervention, to see if the two groups are comparable at this level, and if the differences in results can indeed be explained by the level of difficulty.

Another limitation to mention concerns in particular the reflection group. Indeed, one assumption here is that the effects observed in the reflection group are due to the *reflection* process associated with the readings. In particular, this assumption is justified by the reflection text that the participants in this group had to write after each reading. However, it is possible that these participants listened to the podcasts and completed the readings and writing exercises rather passively, without any specific or subsequent reflection after the exercises themselves. It is thus possible that the intervention works even when the participants do not engage in specific or ongoing reflection after the activities. Future studies could measure the level of reflection reported by the participants on a daily basis to verify that this is indeed the mechanism of action of the intervention, or at least test the percentage of variance that this factor could explain.

To more directly assess the mechanisms of change over time resulting from interventions, it would be relevant for future studies to include daily measures of ecological momentary

assessment (sampling experience), which allow testing the moderating role of change in the regression slope of specific mechanisms of action, such as levels of acceptance, equanimity, presence, or positive affect (Linz et al., 2022; Shiffman et al., 2008).

The study was conducted online rather than in a laboratory setting, with all the limitations that this entails, such as lower levels of attentional control during tasks and questionnaires as well as during experimental interventions or manipulations. Finally, the intervention itself for all groups was also done online. Online meditation does not allow for any social interaction with the rest of the group, as in many studies, or with the meditation instructor, thus limiting some of the interpersonal benefits documented in the literature on loving-kindness (Hutcherson et al., 2008; Kang et al., 2014; Stell & Farsides, 2015) and mindfulness (Canby et al., 2021). Participants also experienced the training at different times since recruitment was ongoing. For example, some participants did the study in the middle of social isolation, due to the pandemic, and other participants did the study without health restrictions. The health context may thus have mitigated some of the potential benefits of the meditation or reflection intervention.

Conclusion

In conclusion, it seems that the effects of reading and thinking about loving-kindness concepts provide robust and reliable prosocial effects, perhaps more than engaging in a more formal meditation practice related to these concepts. However, these benefits are limited to certain behaviours, cognitions, or affects, and they do not generalize to many common behaviours such as charity donations. In addition, ego depletion seems to only moderate implicit attitudes when loving-kindness is experienced through a meditative practice rather than a more cognitive and reflection-related practice. Overall, this study contributes to the contemplative and

ego depletion literatures by delimitating the direct effects of *reflecting* and *meditating* on loving-kindness.

Compliance with Ethical Standards

The study reported in this manuscript has been approved by the university ethics committee and has, therefore, been performed in accordance with the ethical standards laid down in the 1964 Declaration of Helsinki and its later amendments. All persons gave their informed consent prior to their inclusion in this study. The authors report no conflict of interest.

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