

UNIVERSITÉ DU QUÉBEC À MONTRÉAL

TRANSFORMER LES HABITUDES DU CŒUR ET DE L'ESPRIT :
DES BONNES INTENTIONS À UNE RÉPONSE PROSOCIALE FIABLE

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RÉMI THÉRIAULT

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RÉSUMÉ

Les pratiques contemplatives, telles que la présence attentive ou la méditation d'amour-bienveillant, sont de plus en plus populaires dans la société occidentale. De nombreux bienfaits, notamment interpersonnels et sociaux, sont attribués à ces pratiques dans des traditions millénaires, notamment bouddhistes. L'objectif de cette thèse est de clarifier les circonstances dans lesquelles des interventions de méditation d'amour-bienveillant et d'activation cognitive de concepts associés à la présence attentive génèrent les effets les plus importants sur l'agression et la prosocialité. La thèse pose donc essentiellement la question de savoir comment l'effet de ces interventions varie en fonction de la *durée d'intervention*, de *l'individu*, du *contexte* et de la *méthode de pratique*. Nous testons ces idées dans le cadre de quatre études, dont trois études en ligne (article 1) et un essai randomisé contrôlé de six semaines (article 2). Les trois premières études testent une brève exposition contextuelle (amorçage) de la présence attentive via une tâche de phrases brouillées, et, considérés conjointement, les résultats suggèrent que cette tâche n'affecte pas l'agression ou l'hostilité et qu'il n'y a donc pas de modulation par trait de personnalité. La quatrième étude compare deux interventions d'entraînement à l'amour-bienveillant sur six semaines à un groupe témoin par liste d'attente. Le premier groupe méditait sur l'amour-bienveillant, tandis que le deuxième groupe réfléchissait à l'amour-bienveillant (via des extraits de livres populaires et des balados). Nos résultats confirment d'abord que la durée de ces interventions compte, puisque l'on trouve des effets via une longue durée (six semaines de pratique quotidienne, étude 4), mais pas lors d'une brève durée contextuelle (amorçage, études 1-3). Les résultats de la quatrième étude suggèrent également que les attitudes implicites prédisent différemment la prosocialité pour certaines variables en fonction du niveau de fatigue cognitive, manipulé expérimentalement, ce qui supporte l'importance du contexte dans l'influence de ces interventions sur la prosocialité. Enfin, les résultats suggèrent également que la méthode de pratique est importante, puisque réfléchir à l'amour-bienveillant semble plus prometteur que méditer sur la chose. Ultimement, cette thèse contribue de manière significative à la littérature sur la présence attentive et l'amour-bienveillant en apportant des réponses à de nombreuses questions et en ouvrant la porte à de nouvelles questions avec cette prometteuse nouvelle intervention basée sur la réflexion sur l'amour-bienveillant.

Mots clés : présence attentive, pleine conscience, amour-bienveillant, méditation, réflexion, fatigue cognitive, déplétion de l'égo, altruisme, agression, attitudes implicites

ABSTRACT

Contemplative practices, such as mindfulness or loving-kindness meditation, are increasingly popular in Western society. Numerous benefits, particularly interpersonal and social, are attributed to these practices in ancient traditions, particularly Buddhist ones. The aim of this thesis is to clarify the circumstances in which interventions of loving-kindness meditation and of the cognitive activation of concepts associated with mindfulness generate the most significant effects on aggression and prosociality. The thesis therefore essentially asks the question of how the effect of these interventions varies depending on the *duration of intervention*, the *individual*, the *context*, and the *method of practice*. We are testing these ideas in four studies, including three online studies (Article 1) and a six-week randomized controlled trial. The first three studies tested brief contextual exposure (priming) of mindfulness via a scrambled sentence task, and taken together, the results suggest that this task does not affect aggression or hostility and that there is therefore no moderation by personality trait. The fourth study compared two loving-kindness training interventions over six weeks to a waitlist control group. The first group meditated on loving-kindness, while the second group reflected on loving-kindness (via excerpts from popular books and podcasts). Our results first confirm that the duration of these interventions is important, since we find effects via long exposure (six weeks of daily practice, study 4), but not during brief contextual exposure (priming, studies 1-3). The results of the fourth study also suggest that implicit attitudes predict prosociality differently for certain variables depending on the level of cognitive fatigue, manipulated experimentally, which supports the importance of context in the influence of these interventions on prosociality. Finally, the results also suggest that the method of practice matters, since thinking about loving-kindness seems more promising than meditating on it. Ultimately, this thesis contributes significantly to the literature on mindfulness and loving-kindness by providing answers to numerous questions and opening the door to new ones with this promising new intervention based on reflection on loving-kindness.

Keywords: attentive presence, mindfulness, loving-kindness, meditation, reflection, cognitive fatigue, ego depletion, altruism, aggression, implicit attitudes

1. INTRODUCTION GÉNÉRALE

Cette thèse s'intéresse globalement à la relation des interventions de présence attentive et d'amour-bienveillant à l'agression et à la prosocialité, d'un point de vue de cognition sociale. Brièvement, le premier article investigate l'effet d'une activation cognitive de concepts reliés à la présence attentive sur le comportement d'agression. Le deuxième article s'intéresse spécifiquement au *changement* des habitudes du cœur et de l'esprit qui émerge de la pratique de la méditation d'amour-bienveillant (*loving-kindness meditation*). La thèse, dans son ensemble, adopte la perspective de « l'interaction entre la personne et la situation » dans la détermination du comportement, tel que dans la tradition du père de la psychologie sociale, Kurt Lewin (Ross et al., 2011). La thèse pose essentiellement la question : pour quelles personnes et dans quelles situations ces pratiques sont-elles les plus prometteuses?

Selon certains chercheurs, le concept de soi peut être relativement complexe, mais une seule « version » du concept de soi fait surface à la fois, appelé le concept de soi « de travail » (Markus & Wurf, 1987). Le soi est ainsi considéré comme un réseau associatif de concepts ou une collection de représentations, de schémas et de croyances à propos du soi, mais lequel de ces aspects fait surface pour définir le concept de soi de travail dépend en réalité des circonstances présentes. On parle donc d'*accessibilité cognitive* : certaines circonstances rendent ainsi certains concepts/schémas plus ou moins accessibles cognitivement (Higgins, 1996). Une méthode pour mesurer l'accessibilité cognitive de certains concepts consiste à chronométrer le temps de réaction de personnes participantes de concepts associés : des concepts plus facilement accessibles susciteront des temps de réaction plus courts (Markus, 1977).

On parle également d'*accessibilité situationnelle* (due par exemple à une activation/un amorçage temporaire de concepts, le « priming ») ou d'*accessibilité chronique* (due à une plus grande fréquence d'activation conjointe de concepts/schémas; Higgins, 1996). Les concepts qui sont accessibles chroniquement sont en d'autres termes chroniquement à l'avant-plan du concept de soi de travail, alors que ceux qui sont accessibles situationnellement ne restent que temporairement à l'avant-plan du concept de soi de travail. En conséquence, des concepts qui sont fréquemment activés ensemble devraient créer des réseaux hautement interconnectés et donc plus fréquemment (chroniquement) accessibles. Également, des situations différentes

devraient activer des schémas différents et ainsi produire différentes versions du soi, et conséquemment, différents comportements. Cependant, des schémas chroniquement accessibles devraient produire des comportements plus consistants malgré des situations différentes.

En plus de situations différentes, différents *états psychologiques* peuvent aussi favoriser différents schémas comportementaux. Selon le modèle duel du contrôle de soi et de l'impulsivité par exemple (Hofmann et al., 2009), l'accessibilité cognitive de ces concepts dépend également des ressources cognitives disponibles. En effet, lorsque nous sommes en plein contrôle de nos moyens avec toute notre énergie mentale, ce sont nos attitudes et nos valeurs conscientes ou explicites qui ont plus tendance à déterminer notre comportement. Au contraire, lorsque nous n'avons pas accès à toute notre force mentale (p. ex., parce que nous sommes cognitivement épuisés ou que nous avons une faible mémoire de travail, un faible contrôle de soi), ce sont nos attitudes et nos valeurs inconscientes ou implicites qui ont plus tendance à prédire notre comportement. En d'autres termes, être privé de ses pleines facultés cognitives devrait nous rendre plus impulsifs.

Les pratiques contemplatives sont des méthodes introspectives à la première personne qui visent notamment à produire des altérations de certains processus cognitifs (p. ex., en renforçant l'autorégulation de l'attention) et affectifs (p. ex., en générant certaines émotions intentionnellement); celles-ci incluent par exemple la méditation, le Qi Gong ou la prière (p. ex., Bruce et al., 2018; Dorjee, 2016; Mind and Life Education Research Network (MLERN) et al., 2012). Dans le cas de la présence attentive, l'objectif est notamment une meilleure régulation attentionnelle, alors que dans le cas de l'amour-bienveillant, l'objectif est plutôt de développer une bienveillance intérieure globale et durable. En ce sens, ces pratiques représentent d'importants outils potentiels pour créer des changements au niveau de « la personne » (c.-à-d., de la personnalité) et donc de nos interactions sociales—agressives ou prosociales.

Alors que les études investiguant les effets directs de pratiques contemplatives sont de plus en plus populaires, les recherches investiguant les modérateurs de ces effets sont beaucoup plus rares. En particulier, peu de recherches ont systématiquement tenté de clarifier quels individus bénéficient le plus de ces pratiques, à quels moments, en fonction de la méthode d'enseignement, de la durée de l'entraînement, ou de d'autres facteurs (tel que la fatigue cognitive). L'objectif de

cette thèse (et des articles 1 et 2) est donc de clarifier les circonstances dans lesquelles les interventions de présence attentive et d'amour-bienveillant génèrent les effets les plus importants sur l'agression et la prosocialité. La thèse pose donc en essence la question à savoir comment l'effet de ces interventions varie en fonction de *l'individu*, du *contexte*, de la *méthode de pratique* et de la *durée d'intervention*.

Le premier article vise donc à évaluer pour *qui* l'activation *situationnelle* de la présence attentive produit le plus faible niveau d'agression (objectif 1). Le deuxième article quant à lui vise à évaluer dans *quelles circonstances* (quel niveau de fatigue cognitive) une activation *soutenue* de la bienveillance (induite expérimentalement) produit le plus faible niveau d'agression et le plus haut niveau de prosocialité (objectif 2). Le deuxième article vise également à évaluer si la méthode de pratique de la bienveillance (via la méditation ou la réflexion; objectif 3) y joue pour quelque chose. Considérées conjointement, ces deux études permettent également de considérer le rôle de la durée de ces interventions (objectif 4) avec d'un côté une intervention très brève et minimale (via un amorçage cognitif) et de l'autre une intervention soutenue (via une pratique quotidienne).

Nous nous basons sur le modèle duel du contrôle de soi et de l'impulsivité (Hofmann et al., 2009) pour émettre l'hypothèse générale qu'un faible niveau de ressource cognitive devrait mener à une augmentation de l'agression et une réduction de la prosocialité, que ce soit *dispositionnellement* (faibles traits de personnalité comme le contrôle de soi ou la mémoire de travail; article 1, objectif 1) ou *situationnellement* (fatigue cognitive induite expérimentalement; article 2, objectif). Puisque les pratiques contemplatives incarnées comme la méditation qui impliquent le corps et les émotions affecteraient probablement plus fortement les attitudes implicites qu'une pratique cognitive de lecture de textes, nous nous attendions également à ce que cet effet soit influencé par le groupe (objectif 3). Plus spécifiquement, nous nous attendions à ce que le groupe de méditation reste moins agressif et plus prosocial même en contexte de fatigue cognitive. Enfin, alors que le changement occasionné par l'activation de la présence attentive devrait être temporaire et circonscrit (accessible situationnellement uniquement), le changement apporté par une pratique de méditation soutenue devrait être continu et durable

(chroniquement accessible), et les effets généraux devraient ainsi être beaucoup plus importants suivant l'entraînement soutenu (objectif 4).

L'article 1 a été publié dans la revue *Mindfulness*. L'article 2 quant à lui, bien qu'en voie d'être soumis, sera réorganisé en plus d'un article afin d'optimiser le rendement académique et la contribution à la littérature dans des revues à haut facteur d'impact. Dans l'intérêt de la cohérence interne de cette thèse et de l'argumentaire général, la présente structure de l'article 2 est néanmoins conservée.

2. ARTICLE 1 : ACTIVATION IMPLICITE DE LA PRÉSENCE ATTENTIVE : LE TRAIT DE CONTRÔLE DE SOI MODÈRE-T-IL SON EFFET SUR LE COMPORTEMENT AGRESSIF ?

Cette section introduit l'article 1. Des recherches antérieures montrent qu'un nouveau paradigme expérimental consistant à activer implicitement (« amorcer ») des concepts associés à la présence attentive par le biais d'une tâche de phrases brouillées produit des effets sociaux positifs sur la cognition et l'affect. Cependant, les effets de ce paradigme sur le comportement social méritent d'être étudiés plus en détail. Comme plusieurs études lient la présence attentive à une moindre agressivité, l'agressivité représente un candidat prometteur à étudier dans le cadre du paradigme actuel. De plus, des recherches ont démontré que les traits de personnalité, tels que le trait de présence attentive, modèrent l'effet de l'amorce de présence attentive, soulignant l'importance d'identifier les modérateurs potentiels.

Dans une étude exploratoire 1, nous avons examiné laquelle de nombreuses variables de personnalité était la plus significativement liée à la procédure d'amorce de la présence attentive. Dans des études de suivi confirmatoires, nous avons tenté de reproduire ces résultats en utilisant la même méthodologie mais en utilisant des échantillons plus grands et seulement quelques mesures d'intérêt (étude 2) ou des mesures supplémentaires (étude 3). Le contrôle de soi est apparue comme le seul modérateur significatif de l'effet de l'amorce de présence attentive sur le comportement. En conséquence, nous avons spécifiquement testé l'interaction entre la maîtrise de soi et la procédure d'amorçage de la présence attentive dans les deux études de suivi. Les conclusions concernant le rôle du contrôle de soi de la première étude n'ont pas été reproduites

dans les études suivantes. Malgré des résultats initiaux prometteurs, nos conclusions de suivi confirmatoires suggèrent que le contrôle de soi ne modère pas l'effet de l'activation implicite de la présence attentive sur le comportement agressif.

Implicitly Activating Mindfulness: Does Trait Self-Control Moderate Its Effect on Aggressive Behaviour?

Rémi Thériault*  & Stéphane Dandeneau 

Department of Psychology, Université du Québec à Montréal, Québec, Canada

Correspondence: Correspondence concerning this article should be addressed to Rémi Thériault, Department of Psychology, Université du Québec à Montréal, C.P. 8888, Succursale Centre-Ville, Montréal, Québec, Canada, H3C 3P8. E-mail: theriault.remi@courrier.uqam.ca.

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Abstract

Objectives. Previous research shows that a novel experimental paradigm consisting of implicitly activating (“priming”) concepts associated with mindfulness through a scrambled sentence task yields positive social effects on cognition and affect. Yet, the effects of this paradigm on *social behaviour* warrant further investigation. As several studies link mindfulness to lower aggression, aggression represents a promising candidate to investigate within the current paradigm. Furthermore, research has demonstrated that personality traits—such as trait mindfulness—moderate the effect of the mindfulness prime, highlighting the importance of identifying potential moderators. **Methods.** In an exploratory Study 1, we investigated which of several personality variables most meaningfully related to the priming mindfulness procedure. In confirmatory follow-up studies, we attempted to replicate those results using the same methodology but using larger samples and only a few measures of interest (Study 2) or additional measures (Study 3). **Results.** Self-control emerged as the only meaningful moderator of the effect of the mindfulness prime on behaviour. Accordingly, we specifically tested the interaction between self-control and the mindfulness priming procedure in the two follow-up studies. The findings regarding the role of self-control from the first study did not replicate in the subsequent studies. **Conclusions.** Despite promising initial results, our confirmatory follow-up findings suggest that trait self-control does not moderate the effect of implicitly activating mindfulness on aggressive behaviour. **Preregistration.** Study 1 was not preregistered. Studies 2 and 3 were preregistered on OSF: <https://osf.io/582wx/> and <https://osf.io/w46r9/>.

Keywords: implicit mindfulness, aggression, self-control, priming, open science

2.1 Introduction

Recent meta-analyses suggest that mindfulness offers many benefits for health and psychological well-being (Carsley et al., 2018; McClintock et al., 2019; Querstret et al., 2020; Vonderlin et al., 2020), even when taught online (Spijkerman et al., 2016). Other meta-analyses suggest that mindfulness promotes prosocial behaviours (Berry et al., 2020; Donald et al., 2019). In particular, mindfulness appears to improve attitudes toward outgroups (members of other social groups than their own; Berry, Wall, Cairo, et al., 2023; Berry, Wall, Tubbs, et al., 2023; Hunsinger et al., 2014; Kang et al., 2014; Lueke & Gibson, 2015; Parks et al., 2014b), and reduce discriminatory behaviours (Lueke & Gibson, 2016) and aggression (Fix & Fix, 2013; Gillions et al., 2019a; Heppner et al., 2008). When mindfulness is viewed from a dispositional perspective (i.e., as a personality trait or disposition), it is also associated with lower levels of aggression (Eisenlohr-Moul et al., 2016; Heppner et al., 2008; Shorey et al., 2014). However, it is important to note that not all researchers agree on the prosocial effects of mindfulness; for example, another meta-analysis suggests that the benefits of mindfulness meditation are limited (Kreplin et al., 2018). Furthermore, practicing mindfulness is generally effortful, and not everyone has the motivation or energy to do it consistently.

What if some of the benefits of mindfulness could require little to no effort? Researchers have proposed that although all individuals have an innate capacity for mindfulness, implicit (automatic) cognitive processes (e.g., through priming) can momentarily leverage this innate potential to activate a temporary state of mindfulness (Bergeron et al., 2016; Bergeron & Dandeneau, 2016). In one paradigm, researchers unobtrusively present participants with mindfulness-related words (e.g., present moment, non-judgement) using a scrambled sentence task. Accordingly, even this type of indirect priming of mindfulness-related concepts seems to yield benefits, such as more positive cognitive and affective responses (Bergeron et al., 2016; Bergeron & Dandeneau, 2016). After experiencing a social (Bergeron et al. 2016) or personal stressor (Bergeron & Dandeneau, 2016), participants in the implicit mindfulness condition (vs. a control condition) showed higher levels of positive affect and situational self-esteem, lower levels of negative affect, perceived stress, physiological arousal, cortisol (a stress-related hormone) and attentional bias toward negative information.

In general, semantic priming tasks are thought to work by subtly activating constructs or mental representations and making them more temporarily or situationally accessible to memory, which then affects cognition, affect, and possibly, behaviour (Smeesters et al., 2010). In the case of the mindfulness priming task, then, one explanation is that the prime temporarily activates mental constructs related to mindfulness, which, by their nature, then *induces* a state of mindfulness. Some authors suggest that this process in a way activates individuals' innate capacity for mindfulness, thereby making them more open and willing to face unpleasant situations and emotions in a non-defensive and non-judgmental way (Bergeron et al., 2016; Bergeron & Dandeneau, 2016).

To date, the effects of implicitly activating mindfulness were focused on *self*-directed outcomes—one's positive and negative mood, self-esteem, perceived stress, and the like. However, what of behaviours directed toward *others*, such as interpersonal aggression? First, reviews of priming research highlights the importance of the difference between an attentional focus on self versus on other in the context of priming (Smeesters et al., 2010). Second, looking at behavioural components in addition to cognitive and affective outcomes is also important because whereas the effects of priming on cognition is well established, its effects on *behaviour* have been the subject of heated debate (Bargh, 2014; Meyer, 2014). Several classical behavioural priming studies, for instance, have failed to replicate (Klein et al., 2014; McCarthy et al., 2021; McCarthy et al., 2018). Although one meta-analysis found a robust overall effect of priming on behaviour, this effect is small and can vary based on what exactly is primed and other factors (Weingarten et al., 2016a). Replication studies looking at *behavioural* priming are thus still relevant today as they were a decade ago.

Beyond the importance of investigating the direct effect of behaviour priming, it is also of interest to investigate the boundary conditions of those effects by investigating moderators such as individual differences (Maier et al., 2007; Smeesters et al., 2009; Weingarten et al., 2016b). In the case of priming mindfulness, for example, Bergeron et al. (2016) showed that participants with low trait mindfulness benefited the most from the implicit mindfulness activation after having experienced a relatively intense social stressor (public speaking part of the Trier Stress Task), indicating that subtly activating the universal concepts of “focus,” “non-judgement,” and

“letting go” temporarily help those who might not routinely have these habits of mind. The study authors suggest that this is because the implicit activation temporarily helped individuals with low trait mindfulness to experience or perceive the social stressor in a more “mindful-like” manner. Following the same logic, one could ask which personality traits or disposition may moderate the effect of the implicit mindfulness activation on *others*-directed outcomes, such as aggression.

Indeed, some people may be more likely to benefit from a temporarily induced state of mindfulness. Individuals low in trait/implicit aggression for example would likely not improve since they would generally rarely act aggressively (i.e., there would be a floor effect), whereas highly aggressive people may stand to benefit the most. Similarly, people with good self-control or high working memory should excel at self-regulating to avoid acting aggressively (Hofmann et al., 2009), so they would show similar effects as for people low in aggression. More naturally impulsive people though—those low in self-control and/or working memory—might again benefit the most from the mindfulness prime.

While previous studies using the mindfulness priming procedure have focused on the effects of the implicit activation *on the self* in a context of an ego threat, the present set of replication studies emphasize outcomes directed toward *others*, namely aggression. Furthermore, previous studies have only looked at the effects of the mindfulness prime after an ego threat or stressor, not alone, which should also be tested. Therefore, in addition to testing a direct effect of the procedure without an ego threat or stressor, the current research also seeks to better understand which personality factors moderate the effect—in other words, by demonstrating for whom the effect appears to be the most beneficial. We attempt to answer these questions in a set of three studies.

2.2 Study 1

In the first of three studies, we hypothesized that participants in the implicit mindfulness priming condition would show lower levels of aggression towards others than those in the control priming condition (because of the emphasis on the concepts of letting go, non-judgment, and acceptance). We also hypothesized that the mindfulness priming procedure would relate to lower

aggression the most for individuals: low in self-control or working memory (because these people are normally more impulsive), low in trait mindfulness (because these people are less likely to be in a state of mindfulness), or high in trait or implicit aggression (because for individuals low in aggression, there might be a floor effect, meaning it would be difficult to reduce aggression any further).

2.2.1 Methods

2.2.1.1 Participants

Our selection criteria were that participants come from either the United States or Canada. They were compensated \$1, and the study was conducted in English. We planned to use *t* tests for comparing groups and multiple regressions for testing the moderations. Using the means and standard deviations from Bergeron et al. (2016; for self-esteem) we estimated their Cohen's *d* effect size = 0.46 (for posterior measures only). Based on the *pwr* package (Champely, 2020), we estimated that for *t* tests, detecting this average effect size, with 80% power and an alpha level of 0.05, requires 76 participants per group (228 in total). We also estimated that, for multiple regressions, to detect a small effect size of $f^2 = 0.04$ (based on the squared semi-partial correlation reported by Bergeron et al., 2016), with a power of 80% and an alpha level of 0.05 would require a sample size of at least 266 participants. To err on the side of caution, assuming a portion of the data collected on the online platform would be unusable (e.g., due to low-quality answers, mid-study dropouts, or other exclusions), we set the target sample size to 300 on the CloudResearch recruitment platform (formerly TurkPrime; Litman et al., 2017).

Six datasets were merged (joined) through an inner join—3 Qualtrics surveys and 3 Inquisit tasks. Duplicates were addressed with the *rempsyc::best_duplicate* function, which keeps the duplicate with the least amount of missing values, and in case of ties, takes the first occurrence. The resulting pool of participants consisted of 284 participants with unique worker IDs. We excluded participants with duplicate IP addresses (1), that declined to keep their participation after debriefing (1), or with more than 80% of incorrect responses on the crucial mindfulness priming task (18), for a total of 20 exclusions. We thus analyzed the data of 264 participants (Gender: 55.70% women, 29.50% men, 0.00% non-binary, 14.77% missing; Country: 95.08%

USA, 1.52% missing, 1.14% Canada, 2.27% other). Of the 264 participants, 129 were in the mindfulness priming condition, and 135 in the control condition.

2.2.1.2 Procedure

This study uses a between-subject design, whereby participants were randomly assigned to either the experimental group (mindfulness priming) or the control group (neutral words). In a first block, all participants completed three scales (trait self-control, trait aggression, and trait mindfulness) in a randomized order. They then completed the implicit aggression and working memory tasks, before being randomly assigned to one of the two priming conditions (mindfulness vs. control priming). Finally, participants completed an outcome measure of behavioural aggression.

Priming mindfulness. To implicitly activate mindfulness, we used the same word scrambling task as Bergeron et al. (2016) and Bergeron and Dandeneau (2016). The task consists of presenting participants with series of words from which they must select specific words to construct a meaningful sentence. The *unchosen* word is used as a prime to activate the desired construct. For example, “play present outside we moment” becomes “we play outside” where *present moment* is the prime word. By focusing participants’ attention on the words most meaningful to the sentence, the task unobtrusively presents prime words to which limited attention is brought. The experimental (mindfulness) condition consisted of eight sentences containing mindfulness primes (e.g., letting go, nonjudgmental, awareness), and four sentences containing neutral primes. The control (neutral) condition consisted of the same 12 sentences, but with neutral primes instead (e.g., table, rope, sky).

2.2.1.3 Measures

Trait Self-Control. We used the *Brief Self-Control Scale – Alternative Version* (α and ω in the present study = 0.84; 7 items; Tangney et al., 2004). Example item: “I am good at resisting temptation” (1 – *Not at all* to 5 – *Very much*).

Trait Aggression. We used the *Brief Aggression Questionnaire* ($\alpha = 0.81$; $\omega = 0.82$; 12 items; Buss & Perry, 1992). Example item: “Given enough provocation, I may hit another person” (1 – *extremely uncharacteristic of me* to 7 – *extremely characteristic of me*).

Trait Mindfulness. We used the *Kentucky Inventory of Mindfulness Skills* ($\alpha = 0.88$; $\omega = 0.89$, 39 items; Baer et al., 2004). Example item: “I notice when my moods begin to change” (1 – *Never or very rarely true* to 5 – *Very often or always true*).

Implicit Aggression. We used the *Implicit Association Test*, aggression version, available on the [Millisecond website](#) (e.g., Banse et al., 2015). This task is considered valid and reliable (Banse et al., 2015). The experimental blocks (3, 4, 6, 7) contain 20, 40, 20 and 40 trials respectively, and the practice blocks (1, 2, 5), 20 trials each.

Working Memory. We used the *Self-Ordered Pointing Task* available on the [Millisecond website](#) (e.g., Gillett, 2007). This task is considered valid and reliable (Ross et al., 2007) and contains 12 blocks (number of trials not applicable).

Aggressive Behaviour (Dependent Variable). To measure (reactive) aggression, we used a modified version of the Competitive Reaction Time Task (CRTT), also known as Taylor’s Aggression Paradigm, available on the [Millisecond website](#) (similar to Denson et al., 2010). This task is considered valid and reliable (Chester & Lasko, 2019) and contains 4 blocks with 1, 8, 8 and 8 trials, respectively.

2.2.1.4 Data Analyses

To ensure optimal normal distribution of the data, we identified and applied optimal normalizing transformations (excluding the Ordered Quantile Normalization transformation) via the *bestNormalize* package (Peterson, 2021; Peterson & Cavanaugh, 2020). We also specifically used Welch *t* tests, as per recommendations (Delacre et al., 2017), using a critical value of $p < 0.05$ with two-tailed tests.

There were no missing data. *bestNormalize* transformed the following variables: aggressive behaviour (square root), trait mindfulness (Box Cox), trait aggression (asinh), working memory (Yeo-Johnson), and implicit aggression (square root). Trait self-control required no transformation. After the transformations, the variables were reasonably normally distributed and homoscedastic in each group. We identified 10 univariate outliers in the control group, and 10 in the experimental group, with group-based median absolute deviations greater than three. These observations were winsorized using the group's three median absolute deviation value (Leys et al., 2013; Thériault et al., 2024). We also standardized all continuous variables.

For the linear models, the group variable was dummy coded as 0 (control/reference group) and 1 (mindfulness priming group). The model included all interaction terms between the condition variable and the other potential moderators (condition \times trait mindfulness, condition \times trait self-control, condition \times trait aggression, condition \times working memory, and condition \times implicit aggression), as well as the simple effects of those moderators.

We performed all statistical analyses in R version 4.2.2 (R Core Team, 2022) using the following additional packages: *effectsize* (Ben-Shachar et al., 2020; 2022), *psych* (internal reliability analyses; Revelle, 2018), *dplyr* (data manipulation; Wickham et al., 2021), *interaction* (moderation figure; Long, 2019), as well as report (Makowski et al., 2022) and *rempsyc* (Thériault, 2023) for convenience functions (checking univariate assumptions, missing items, *t* tests, moderations, tables, etc.).

CRTT Quantification Strategy. The CRTT suffers from a plethora of quantification strategies, which makes it difficult to compare and replicate past findings. Elson and colleagues (Elson, 2016; Elson et al., 2014) reports 157 different quantification strategies used by researchers to date. Unfortunately, there is currently no consensus as to the right analytical technique to employ. Current recommendations are to justify and preregister the strategy used.

We used the following quantification method in Study 1 (but also Studies 2 and 3): we multiplied (1) the average *volume* of all 25 trials by (2) the average *duration* of all 25 trials, and then normalized the product. We refer to this strategy as “the normalized product of the averages”. The optimal normalization transformation was then identified and applied automatically through

the “bestNormalize” package in R (Peterson, 2021; excluding the option to consider the Ordered Quantile Normalization transformation).

To us, using the product of the average volume and average duration makes the most intuitive sense as a representation of the combined effects of one’s “aggressive” behaviour. For example, investigating the subcomponent of the CRTT separately can be misleading—setting the volume to 10 for a duration of 1 second is qualitatively different than setting the volume to 5 for 5 seconds. Methods using the sum of volume intensity and duration, or the average of all 50 trials (intensity and duration) would yield, in our opinion, misleading aggression scores. Using the above numbers as an example, our normalized product of averages score would compare 10 (10 x 1) vs. 25 (5 x 5), whereas the sum method would compare 11 (10 +1) vs 10 (5 +5), and the average of all trials 5.5 vs. 5. Using the product of intensity and duration, as was done by Bartholow et al. (2005) and Arriaga et al. (2011), takes into consideration the *combined* effect of both volume and duration components of the aggressive behaviour.

2.2.2 Results

The Welch Two Sample *t* test testing the difference of aggressive behaviour by condition ($M_{\text{Control}} = -0.06$, $M_{\text{Mindfulness}} = 0.06$) suggests that the effect is statistically not significant, and very small (difference = -0.12, 95% CI [-0.36, 0.13], $t(258.24) = -0.95$, $p = 0.342$; Cohen’s $d = -0.12$, 95% CI [-0.36, 0.12]).

For the linear model testing the interactions (moderations), using the *performance* and *see* packages (Lüdtke, Ben-Shachar, et al., 2021; Lüdtke, Patil, et al., 2021), we assessed that the model residuals were reasonably linear, homoscedastic, and normally distributed, and there were no high collinearity or model-based outliers flagged.

Results of the moderation analyses showed that trait self-control significantly moderated the effect of priming mindfulness on aggression (Table 1.1 and Figure 1.1). Simple slope analyses (with -1 and $+1$ SD; Aiken & West, 1991; Hayes, 2018) revealed that the mindfulness priming condition predicted higher aggression, but only for people high in self-control and not for those at mean or low self-control (Table 1.2).

2.2.3 Discussion

Results from this study show that: (a) the mindfulness priming condition (compared to the control condition) did not significantly influence participants' level of aggression; and (b), of the five personality variables tested, self-control was the only significant moderator that interacted with the experimental condition to predict aggression. Furthermore, we initially hypothesized that priming mindfulness would lower aggression, and more significantly so for those with *low* self-control. However, the results differ from our predictions: the simple slopes of the interaction showed that priming mindfulness *increased* aggression for those with *high* self-control. Indeed, a closer visual inspection of the interaction (Figure 1.1) suggests that participants with low self-control in the mindfulness priming condition reported lower aggression than their counterparts in the control condition; however, the corresponding simple slope was not significant. Rather, unexpectedly, the mindfulness priming condition (compared to the control condition) seems to have caused higher aggression for individuals *high* in self-control.

That a mindfulness intervention may lead to higher aggression is both counterintuitive and contrary to existing literature (Fix & Fix, 2013; Gillions et al., 2019a), although in a series of studies, researchers showed that mindfulness induction can reduce guilt and prosocial reparation (Hafenbrack et al., 2022). That this effect arises in people high in self-control is perhaps even more surprising, given that mindfulness typically has beneficial effects even for high self-control individuals (Bowlin & Baer, 2012). Nonetheless, the literature also suggests that trait self-control can sometimes have ironic effects. That is, whereas trait self-control typically relates to positive, desirable effects, it can sometimes lead to counterintuitive (“ironic”) effects, such as negative, undesirable effects when interacting with certain situational circumstances, such as ego depletion (Imhoff et al., 2014a; Lindner et al., 2017, however the concept of ego depletion is contested, see, e.g., Friese et al., 2019).

Perhaps a mindfulness prime, which is different from mindfulness practice, is another such condition with ironic effects for high self-control individuals. For example, it could be that participants with high self-control normally inhibit their aggressive impulses, but that the mindfulness priming condition, by emphasizing words like “letting go”, encouraged them to also let go of their usual internal control. In contrast, for people low in self-control, who normally act

more impulsively, priming mindfulness might have made them act indeed more mindfully, which in turn could translate to lower aggression.

All in all, these unexpected findings do not align well with previous theoretical predictions of the self-control literature and warrant further investigation. Thus, we aimed to validate these results in a second, confirmatory, preregistered study relying on a larger sample size.

2.3 Study 2

Based on the exploratory results from Study 1, we adapted and narrowed our research question and hypotheses to be as parsimonious as possible and only include the three variables of interest: self-control, the priming conditions, and aggression as a dependent variable. For this second, confirmatory study, we speculated that the mindfulness priming procedure might temporarily encourage individuals high in self-control to “let go” and act more naturally, and thus possibly more aggressively. Consequently, we hypothesized that trait self-control will moderate the mindfulness priming procedure, such that individuals high in self-control in the mindfulness priming condition would show *higher* aggression (compared to those in the control condition or those with low self-control).

2.3.1 Methods

2.3.1.1 Participants

We planned to recruit and analyze a minimum of 342 participants in total after exclusions. We determined this sample size based on a power analysis conducted with the *pwr* package in R (Champely, 2020), assuming an sr^2 (converted to f^2) of 0.03 (based on the sr^2 value of the self-control interaction effect obtained in study 1), 1 numerator degree of freedom, 90% power, and a significance level of 0.05. To err on the side of caution, assuming a bit over one-third of the data collected on the online platform would be unusable (e.g., due to low-quality answers, mid-study dropouts, failed attention checks, or other exclusions), we set the target sample size to 513 on CloudResearch.

Three datasets were merged (joined) through an inner join—2 Qualtrics surveys and 1 Inquisit task. Duplicates were addressed with the *rempsyc::best_duplicate* function, which keeps the duplicate with the least amount of missing values, and in case of ties, takes the first occurrence. The resulting pool of participants consisted of 377 participants with unique worker IDs. We excluded participants with more than 80% of incorrect responses on the crucial mindfulness priming task (10) or who failed the attention checks (5), for a total of 15 exclusions. We thus analyzed the data of 362 participants (Mean age = 43.3, *SD* = 12.70, range: [21, 81]; Gender: 59.10% women, 39.20% men, 1.66% non-binary; Country: 100.00% United States of America; Race: 76.52% White, 9.67% Black or African American, 7.18% Asian, 4.42% Mixed, 2.21% other). Analyses were conducted with $n = 178$ participants in the mindfulness priming condition and $n = 184$ in the control condition.

2.3.1.2 Procedure

We used the same experimental design (between-subject design) and procedure as in the first study: all participants completed scales of trait self-control, trait aggression, and trait mindfulness (in a randomized order), the implicit aggression task, followed by the priming mindfulness task and behavioural aggression task.

2.3.1.3 Measures

We used the same scales as in Study 1: the *Brief Self-Control Scale – Alternative Version* (α and $\omega = 0.84$; Tangney et al., 2004), the *Brief Aggression Questionnaire* ($\alpha = 0.83$; $\omega = 0.84$; Buss & Perry, 1992), and the *Kentucky Inventory of Mindfulness Skills* (α and $\omega = 0.90$; Baer et al., 2004). We also used the same Competitive Reaction Time Task (CRTT) and quantification method as in Study 1 for the measure of aggressive behaviour.

2.3.1.4 Data Analyses

To ensure optimal normal distribution of the data, we again identified and applied optimal normalizing transformations (excluding the Ordered Quantile Normalization transformation) via the *bestNormalize* package (Peterson, 2021; Peterson & Cavanaugh, 2020). We used a critical value of $p < 0.05$ with two-tailed tests. We report, as per recommendations, using the *rempsyc*

package (Thériault, 2023), item-level missing values by scale, as well as participants' maximum number of missing items by scale (Parent, 2013). Trait mindfulness had 0.01% missing data points (with no participant with more than 2 missing items out of 39). No other data were missing. Visual inspection of the missing data using the *visdat* package (Tierney, 2017) revealed no specific patterns.

As per best practice (van Ginkel et al., 2020), we imputed the two item-level missing values (before calculating the scales means) using the *missForest* R package (Stekhoven, 2022; Stekhoven & Bühlmann, 2012). *bestNormalize* (Peterson, 2021) transformed the following variables: aggressive behaviour (square root), trait mindfulness (log), and trait aggression (asinh). After the transformations, the variables were reasonably normally distributed and homoscedastic in each group. We identified one univariate outlier in the control group, and four in the experimental group, with group-based median absolute deviations greater than three, so we winsorized these observations by group to three median absolute deviations (Leys et al., 2013; Thériault et al., 2024).

2.3.2 Results

For the linear model, using the *performance* and *see* packages (Lüdtke, Ben-Shachar, et al., 2021; Lüdtke, Patil, et al., 2021), we assessed that the model residuals were reasonably linear, homoscedastic, and normally distributed, and that there were no high collinearity or model-based outliers flagged. We tested a linear regression model with an interaction term between self-control and condition (mindfulness priming versus control), which was the only significant interaction in Study 1. Unlike in Study 1, the interaction was not significant (Table 1.3), meaning that self-control did not moderate the effect of the mindfulness priming task on aggression.

Furthermore, as described in the preregistration, we also tested an alternative exploratory model that contained trait aggression and trait mindfulness, along with two three-way interactions (the condition \times trait aggression \times trait self-control interaction, and the condition \times trait mindfulness \times trait self-control interaction). All assumptions were similarly reasonably respected, but none of the three-way or two-way interactions were significant (Table 1.4). This means that self-control, trait aggression, and trait mindfulness did not moderate the effects of the mindfulness priming

task on aggression, even when controlling for other terms. It also means that self-control did not moderate the interaction between the mindfulness priming task and trait aggression, or the interaction between the mindfulness priming task and trait mindfulness.

2.3.3 Discussion

As in Study 1, regardless of level of trait self-control, the overall direct effect of the mindfulness condition on aggressive behaviour was not significant. Furthermore, in contrast to Study 1, trait self-control did not moderate the effect of condition on aggressive behaviour, and the effect sizes were practically 0, meaning there was not even a *hint* of an effect.

The first conclusion indicates that the mindfulness priming condition clearly does not have a direct effect on people's levels of aggressive behaviour. As in Study 1, unobtrusively priming participants with "mindfulness-related" words did *not* influence their outward aggressive behaviours. This outcome somewhat contrasts with previous literature, but it is important to note that previous studies (Bergeron et al., 2016; Bergeron & Dandeneau, 2016) showed effects on *self*-directed outcomes (e.g., positive and negative mood, self-esteem, perceived stress), and not on behaviours focused on *others*. In this sense, the current results suggest the mindfulness priming procedure might not be strong enough to counter strong other-directed outcomes such as aggression. Perhaps self-reported measures that emphasize internal self-focused dynamics, as opposed to behavioural measures, would be better suited to capture the effects.

Another reason for the non-replication of the moderator effect could be due in part to slight differences in their design, for example through the potential influence of task order and cognitive depletion. Indeed, to narrow our focus in Study 2, we eliminated extraneous variables such as the implicit measure of aggression (aggression IAT) and working memory (self-ordered pointing task), both of which are cognitively demanding. It is possible that the previous addition of these tasks acted as a sort of cognitive depletion *before* completing the mindfulness priming task, thereby influencing the behavioural outcome of aggression.

Whereas mindfulness priming may lead high self-control individuals to let go, they might not let go so easily, precisely because they are good at controlling themselves. However, if they are first

cognitively depleted, they may be more willing to let go when primed to that effect. Consistent with this idea, the literature suggests that brief mindfulness inductions interact with ego depletion to influence aggression (Yusainy & Lawrence, 2015), and furthermore that ego depletion has a stronger effect in high self-control individuals (Imhoff et al., 2014a; Lindner et al., 2017). Thus, perhaps the priming effect emerges on more outward-focused behaviours when high self-control participants are cognitively depleted. We test this idea again in the next study.

2.4 Study 3

The goal of this third study was to help us better understand the diverging results from studies 1 and 2. Study 1 suggested that self-control moderates the effect of the mindfulness priming task on aggression, but Study 2 suggested that this was not the case. One way to reconcile these findings is the possibility that the interaction between the mindfulness priming task and self-control only emerges when people are first cognitively depleted (e.g., through the implicit aggression and working memory tasks). To cover the possibility that the results from Study 1 were due to such methodological differences between Study 1 and Study 2, in this third study, we opted for a more exact replication of Study 1 and re-added the implicit aggression and working memory measures to have as close a replication as possible.

We also added two additional measures to see if the moderation effects between self-control and the mindfulness priming task would also extend to self-reported outcomes. First, we added a measure of mood, because it has been shown to be influenced by the mindfulness priming task in earlier studies (Bergeron et al., 2016; Bergeron & Dandeneau, 2016). Second, we also added a state measure of hostility to investigate the possible effects of the priming condition on more self-reported measure of aggression (as opposed to the other-focused measure, i.e., the CRTT). Hostility has also been shown to be associated with mindfulness (Heppner et al., 2008). We added these additional outcome measures at the very end of the procedure to make sure that this would not influence our testing of the original interaction of interest. This way, it would be possible to test both the original hypothesis on behavioural aggression, mood, and state hostility while maintaining a full replication design of Study 1. Based on the Study 1 results, we hypothesized that trait self-control will moderate the mindfulness priming procedure, such that participants with *high* self-control in the mindfulness priming condition would show *higher*

levels of aggression, negative affect, and hostility than their counterparts in the control condition. For hostility, we also expected to find effects only for the global score and the two subscales of theoretical interest and most sensitive to experimental manipulations (i.e., “feeling mean”, and “aggravation”).

2.4.1 Methods

2.4.1.1 Participants

We used the same parameters as in Study 2. Six datasets were merged (joined) through an inner join—3 Qualtrics surveys and 3 Inquisit tasks. Duplicates were addressed with the *rempsyc::best_duplicate* function, which keeps the duplicate with the least amount of missing values, and in case of ties, takes the first occurrence. The resulting pool of participants consisted of 475 participants with unique worker IDs. We excluded participants with duplicate IP addresses (3), with more than 80% of incorrect responses on the crucial mindfulness activation task (33), who failed the attention checks (5), or who missed entire sections of the study (2), for a total of 43 exclusions. We thus analyzed the data of 432 participants (Mean age = 43.50, *SD* = 12.80, range: [19, 85]; Gender: 58.10% women, 40.50% men, 1.39% non-binary; Country: 99.54% United States of America, 0.46% other; Race: 77.78% White, 11.11% Black or African American, 4.17% Asian, 3.47% Mixed, 1.39% American Indian or Alaska Native, 2.08% other). Participants were randomly assigned to the mindfulness activation group ($n = 214$) or to the control group ($n = 218$).

2.4.1.2 Procedure

We used the same experimental design (between-subject design) and procedure as in the first study: all participants completed scales of trait self-control, trait aggression, and trait mindfulness (in a randomized order), followed by the implicit aggression, working memory, priming mindfulness, and behavioural aggression tasks, in this order. Additionally in this study, participants also completed measures of mood and state hostility, in this order, after the behavioural aggression task, as additional dependent variables.

2.4.1.3 Measures

We used the same scales as in Study 1: the *Brief Self-Control Scale – Alternative Version* ($\alpha = 0.83$; $\omega = 0.84$; Tangney et al., 2004), the *Brief Aggression Questionnaire* (α and $\omega = 0.85$; Buss & Perry, 1992), and the *Kentucky Inventory of Mindfulness Skills* ($\alpha = 0.89$; $\omega = 0.90$; Baer et al., 2004). However, we also added the *Positive and Negative Affect Schedule – Short Version* (positive affect $\alpha = 0.82$, $\omega = 0.83$, negative affect α and $\omega = 0.92$; 10 items; Kercher, 1992) and the *State Hostility Scale* (α and $\omega = 0.98$; Anderson et al., 1995), which participants completed after the CRTT and before demographic questions. Originally, we only planned to include the *State Hostility Scale*'s two subscales of theoretical interest and most sensitive to experimental manipulations (i.e., “feeling mean” and “aggravation”, for a total of 21 items; Anderson & Carnagey, 2009) and prepared the online questionnaire accordingly. However, after discussion, we agreed to use all four subscales (35 items) of the *State Hostility Scale* and wrote as such in the preregistration. However, due to experimenter error, we forgot to update the online questionnaire, so the short version with only the “feeling mean” and “aggravation” subscales were used. Example item: “I feel like yelling at somebody” (1 – *Strongly disagree* to 5 – *Strongly agree*).

2.4.1.4 Data Analyses

To ensure optimal normal distribution of the data, we again identified and applied optimal normalizing transformations (excluding the Ordered Quantile Normalization transformation) via the *bestNormalize* package (Peterson, 2021; Peterson & Cavanaugh, 2020). We used a critical value of $p < 0.05$ with two-tailed tests. State hostility had 0.02% missing data points (with no participant with more than 1 missing items out of 21). No other data were missing. Visual inspection of the missing data using the *visdat* package (Tierney, 2017) revealed no specific patterns.

As per best practice (van Ginkel et al., 2020), we imputed the two state hostility item-level missing values (before calculating the scales means) using the *missForest* package (Stekhoven, 2022; Stekhoven & Bühlmann, 2012). *bestNormalize* (Peterson, 2021) transformed the following variables: aggressive behaviour (square root), trait mindfulness (Yeo-Johnson), trait aggression

(square root), state hostility (Box Cox), positive affect (Box Cox), negative affect (square root), implicit aggression (Yeo-Johnson), and working memory (Yeo-Johnson). After the transformations, the variables were reasonably normally distributed and homoscedastic in each group, except for negative affect and state hostility, which were still right skewed. We identified 45 univariate outliers in the control group, and 41 in the mindfulness group, with group-based median absolute deviations greater than three, so we winsorized these observations by group to three median absolute deviations (Leys et al., 2013; Thériault et al., 2024).

2.4.2 Results

For the linear model, using the *performance* and *see* packages (Lüdtke, Ben-Shachar, et al., 2021; Lüdtke, Patil, et al., 2021), we assessed that the model residuals were reasonably linear, homoscedastic, and normally distributed, and that there were no high collinearity or model-based outliers flagged. However, for the models using negative affect and hostility as dependent variables, the quantile-quantile plots suggested that the model residuals were not completely normally distributed (one of the assumptions of such linear models), even after optimal transformation through the *bestNormalize* package. Critically, none of the interactions of interest, between self-control and the condition term on aggressive behaviour, affect, or state hostility were significant (Table 1.5).

Furthermore, as described in the preregistration, we also tested alternative exploratory models that examined the influence of the two-way interactions between condition and personality traits (trait mindfulness, trait self-control, trait aggression, working memory, and implicit aggression) on all outcome measures (aggression, positive affect, negative affect, and state hostility). All assumptions were similarly reasonably respected, but none of the two-way interactions were significant (Table 1.6).

2.4.3 Discussion

The results of Study 3 show that, as in Study 1 and Study 2, the mindfulness priming condition does not have direct effects on either behavioural aggression or self-reported mood and hostility toward others. The results also show, as in Study 2, that self-control, trait aggression, implicit

aggression, trait mindfulness, or working memory do not moderate the effect of the mindfulness priming condition on aggression, mood, or hostility. Despite sufficient statistical power (as established by our power analyses based on the sr^2 effect size from Study 1), we did not replicate the counterintuitive results shown in Study 1. Taken together, these results suggest that the mindfulness priming condition may be too subtle to influence aggressive behaviour, positive and negative affect, and hostility toward others.

2.5 General Discussion

In the current set of studies, we wanted to test whether subtly priming the concept of mindfulness would influence behavioural aggression, negative affect, and hostility, either directly or in interaction with key personality variables shown to be associated with aggression. Results of Study 1 showed that priming mindfulness alone was not sufficient to affect behavioural aggression. However, when interacting with self-control, priming mindfulness did affect aggression: for people with low self-control, priming mindfulness related to lower aggression, whereas for people with high self-control, it related to higher aggression.

However, these results could not be replicated in two follow-up studies. Specifically, we attempted to make Study 2 more parsimonious by only including variables of interest: trait self-control and aggression. This reduced design could not replicate the findings from Study 1. Therefore, we hypothesized that something from the design of Study 1 was necessary to generate the effects observed initially. In Study 3, we thus made a close replication of Study 1, while also adding two additional, more subtle dependent variables that should be more easily influenced than behaviour: feelings of hostility as well as positive and negative affect. Even with this close replication, the findings from Study 1 did not emerge. Together, these results (including effect sizes approximating zero in Studies 2 and 3) suggest that the findings from Study 1 might represent a Type I error, or in other words, a false positive due for example to chance.

Based on our results, along with existing literature (Bergeron et al., 2016; Bergeron & Dandeneau, 2016), we suggest that the effectiveness of priming mindfulness procedures may be limited to self-directed outcomes that help one deal with an ego threat, and that it does not influence negative or antisocial other-directed outcomes such as aggression. The distinction

between self-focus and other-focus in the context of priming is consistent with previous literature (Smeesters et al., 2010), and these results help delineate the extent and limits of brief interventions relying on scrambled word tasks priming mindfulness.

Our results suggest that unobtrusively priming mindfulness does not influence aggression, hostility, or affect. This is somewhat surprising given that previous research demonstrated an effect on several variables, including affect (Bergeron et al., 2016; Bergeron & Dandeneau, 2016). One important difference, however, is that previous studies testing the effects of this priming mindfulness task did so in the context of an ego threat (such as failing an anagrams task, recalling a very negative personal event, or a public speaking task), either before or after having mindfulness implicitly activated. Other research has also shown the protective nature of mindfulness on self-thoughts and self-affect (Britton et al., 2012; Fogarty et al., 2015; Heppner et al., 2008; Huffziger & Kuehner, 2009; Kuehner et al., 2008). In our current set of studies, instead of testing the effects of mindfulness during or after psychological stress, we investigated the boundary conditions of implicit mindfulness in the context of externally directed aggression, hostility, and negative affect.

One could argue that the CRTT, by administering and receiving loud sound blasts, could have acted as a sort of psychological threat. However, this task also acted as our dependent variable, making it impossible to assess its effect on itself. Still, in the third study, participants additionally completed the PANAS, a popular schedule of positive and negative affect, followed by a hostility questionnaire. If the CRTT had acted as a psychological threat, we would have seen its effects on the two subsequent dependent variables, affect and hostility, though we have not. Whereas previous studies (Bergeron et al., 2016; Bergeron & Dandeneau, 2016) showed that participants reported higher positive affect and lower negative affect following psychological stress, our current studies suggest that the same mindfulness priming task does not make people *behave* or *feel* less aggressively towards others. One interpretation is that the temporarily prime-induced state of mindfulness is too subtle to influence variables like behaviour, and in particular, aggression, further highlighting the differences between traditional mindfulness practice and this particular scrambled words task.

Finally, there were also minor demographic differences between the studies. In the original studies (Bergeron et al., 2016; Bergeron & Dandeneau, 2016), participants were recruited from the university campus and participants completed the experiment in French, whereas in the current studies, participants were recruited online, from CloudResearch, and completed the experiment in English. Furthermore, the samples differ on other characteristics, such as age (the university sample being about 20 years younger).

These findings also highlight the importance of open science and preregistered replication studies. In the current era of the replication crisis (e.g., Camerer et al., 2018; Ioannidis, 2005; Open Science Collaboration, 2015), preregistered replication studies seem more necessary than ever (Cesario, 2014; Nosek et al., 2018; Nosek & Lakens, 2014). Without them, it is difficult to establish whether original findings—no matter how convincing—are due to chance alone (or worse, to questionable research practices) or whether they are true effects that can be trusted and built upon. Priming effects, in particular, have been difficult to replicate, and some scientists have called for researchers to begin with replicating their own priming effects (Cesario, 2014; Doyen et al., 2012; Ramscar, 2016; Ramscar et al., 2015).

In this sense, the current set of replication studies from our own priming effects constitute a nice case study. Although we conducted the first study transparently and honestly, we were not able to replicate them in follow-up replication studies. There is always a 5% chance of finding a significant finding even when the effect does not in fact exist. Considering researchers' degrees of freedom further inflate this number. Yet, many researchers may still underestimate the likelihood of false positives and accordingly develop a misplaced confidence in exploratory findings from single studies. Thus, we believe that this set of studies demonstrates the importance of preregistered replication studies, and particularly so in the context of mindfulness and priming research.

2.5.1 Limitations and Future Directions

Study 1 was more exploratory in nature, and as such, was not preregistered, even though multiple tests were conducted, thus increasing the risks of Type I errors (false positives). In Study 2, the instructions for the mindfulness priming procedure were accidentally randomized with the order

of the questions, so although the task objectives were obvious, some participants have seen the explicit instructions at different points within that specific task. However, the high success rate for the task suggests this was not a meaningful limitation. In Study 3, the fact that participants completed the CRTT and Positive and Negative Affect Schedule before the State Hostility Scale might have affected their answers on the hostility measure. However, we believe it unlikely to have affected the effect of the experimental priming condition. All three studies were conducted online, as opposed to in the laboratory making it difficult to assess participant's attention and concentration. Even though some measures were cognitive or behavioural (such as the tasks to measure implicit aggression, working memory, and aggression), all three studies also relied on self-report measures (trait self-control, trait aggression, trait mindfulness, affect, state hostility), which have known limitations, such as being vulnerable to demand characteristics. Finally, readers should avoid generalizing the conclusions from this study to mindfulness as a whole or to intentional mindfulness practice because these are different from the *implicit* (non-intentional) mindfulness priming used in this study.

Beyond rigorously replicating past research on priming mindfulness using preregistered open science protocols, future research should continue exploring the boundary conditions of this paradigm. We have established that priming mindfulness does not have strong, direct effects on aggression, and that factors such as trait self-control, trait aggression, trait mindfulness, implicit aggression, and working memory, do not moderate its effect on aggression, hostility, or affect. Future research could test whether other dependent variables and moderators are more sensitive to this paradigm, such as positive or prosocial other-focused behaviours, rather than anti-social behaviour like aggression. In particular, future research should test the hypothesis that the effectiveness of the mindfulness prime specifically depends on self-directed outcomes that help one recover from an ego threat. One experimental design for example could compare two conditions: one with an ego threat and one without, before the mindfulness prime, while having both other-focused outcomes (for which it should not work) and self-focused outcomes (for which significant interactions with e.g., mindfulness should come up).

In conclusion, we report mixed findings regarding the effectiveness of a priming mindfulness procedure in relation to different personality traits relative to aggressive behaviour and hostility.

A first study showed that self-control moderates the effect of a mindfulness priming task on behavioural aggression, yet two null-results follow-up studies suggest our initial findings may have been a false positive. These findings suggest that scrambled-word-based priming mindfulness tasks do not influence aggression, either alone or in interaction with other personality variables. Instead, the effectiveness of priming mindfulness procedures may be specific to *self*-directed outcomes that help one recover from ego threat and does not influence negative or antisocial *other*-focused outcomes such as aggression. It is still unclear, however, whether priming mindfulness would influence positive or prosocial other-focused behaviours. These results also highlight the importance of open science and preregistered replication studies in mindfulness research.

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2.5.3 Author Contributions

Rémi Thériault: Conceptualization, Methodology, Investigation, Project administration, Formal analysis, Data Curation, Visualization, Software, Writing - Original Draft, Writing - Review & Editing. **Stéphane Dandeneau:** Conceptualization, Methodology, Writing - Review & Editing, Resources, Supervision, Project administration, Funding acquisition.

2.5.4 Funding

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2.5.5 Open Practices and Data Availability Statement

The data, analysis scripts, and supplemental materials for this study are available on the Open Science Framework (Study 1: <https://osf.io/cqbjy/>; Study 2: <https://osf.io/cqbjy/>; Study 3: <https://osf.io/nzv6y/>), where Studies 2 and 3 were also preregistered (Study 2: <https://osf.io/582wx/>; Study 3: <https://osf.io/w46r9/>). This manuscript first appeared as a preprint on *Research Square* at <https://doi.org/10.21203/rs.3.rs-3161372/v2>.

2.5.6 Compliance with Ethical Standards

All three human studies reported in this manuscript have been approved by the ethics committee of the Université du Québec à Montréal and have therefore been performed in accordance with the ethical standards laid down in the 1964 Declaration of Helsinki and its later amendments.

2.5.7 Informed Consent

All persons gave their informed consent prior to their inclusion in each of those studies.

2.5.8 Conflict of Interest

The authors declare no competing interests.

2.5.9 Use of Artificial Intelligence

AI was not used.

Table 1.1

Exploring Other Personality Moderators of Priming Mindfulness (Study 1)

Dependent Variable	Predictor	<i>df</i>	<i>b</i>	<i>t</i>	<i>p</i>	<i>sr</i> ²	95% CI
Aggression	condition	252	0.16	1.39	0.17	0.01	[0.00, 0.02]
	KIMS	252	0.15	1.49	0.14	0.01	[0.00, 0.03]
	BSCS	252	-0.16	-1.64	0.10	0.01	[0.00, 0.03]
	BAQ	252	0.05	0.47	0.64	0.00	[0.00, 0.01]
	SOPT	252	0.27	2.93	< 0.01**	0.03	[0.00, 0.07]
	IAT	252	0.20	2.40	0.02*	0.02	[0.00, 0.05]
	condition × KIMS	252	-0.25	-1.80	0.07	0.01	[0.00, 0.03]
	condition × BSCS	252	0.49	3.32	< 0.01**	0.04	[0.00, 0.08]
	condition × BAQ	252	0.14	1.03	0.31	0.00	[0.00, 0.02]
	condition × SOPT	252	-0.03	-0.27	0.79	0.00	[0.00, 0.00]
condition × IAT	252	-0.17	-1.36	0.18	0.01	[0.00, 0.02]	

Note. Aggression refers to the product of blast intensity and blast duration in the Competitive Reaction Time Task (CRTT). KIMS: trait mindfulness; BSCS: trait self-control; BAQ: trait aggression; SOPT: working memory; IAT: implicit aggression.

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.

* $p < 0.05$, ** $p < 0.01$. Rows with grey shading indicate statistical significance.

Table 1.2

Simple Slopes of Mindfulness Priming Condition on Aggression at -1, mean, and +1 SD of Self-Control (Study 1)

Dependent Variable	Predictor (+/-1 SD)	<i>df</i>	<i>b</i>	<i>t</i>	<i>p</i>	<i>sr</i> ²	95% CI
Aggression	condition (LOW-BSCS)	252	-0.33	-1.73	0.09	0.01	[0.00, 0.03]
	condition (MEAN-BSCS)	252	0.16	1.39	0.17	0.01	[0.00, 0.02]
	condition (HIGH-BSCS)	252	0.65	3.47	< 0.01***	0.04	[0.00, 0.09]

Note. Aggression refers to the product of blast intensity and blast duration in the Competitive Reaction Time Task (CRTT). BSCS: trait self-control.

*** $p < 0.001$. Rows with grey shading indicate statistical significance.

Table 1.3

Testing the Condition × Self-Control Interaction (Study 2)

Dependent Variable	Predictor	<i>df</i>	<i>b</i>	<i>t</i>	<i>p</i>	<i>sr</i> ²	95% CI
Aggression	condition	355	0.09	0.81	0.42	0.00	[0.00, 0.01]
	BSCS	355	-0.05	-0.63	0.53	0.00	[0.00, 0.01]
	condition × BSCS	355	0.01	0.07	0.94	0.00	[0.00, 0.00]

Note. Aggression refers to the product of blast intensity and blast duration in the Competitive Reaction Time Task (CRTT). BSCS: trait self-control. There are no significant interactions.

Table 1.4

Exploring Other Personality Moderators of Priming Mindfulness (Study 2)

Dependent Variable	Predictor	<i>df</i>	<i>b</i>	<i>t</i>	<i>p</i>	<i>sr</i> ²	95% CI
Aggression	condition	346	0.07	0.60	0.56	0.00	[0.00, 0.01]
	KIMS	346	0.06	0.57	0.57	0.00	[0.00, 0.01]
	BSCS	346	0.02	0.21	0.83	0.00	[0.00, 0.00]
	BAQ	346	0.23	2.57	0.01*	0.02	[0.00, 0.05]
	condition × KIMS	346	-0.05	-0.39	0.69	0.00	[0.00, 0.00]
	condition × BSCS	346	0.02	0.16	0.87	0.00	[0.00, 0.00]
	KIMS × BSCS	346	0.05	0.60	0.55	0.00	[0.00, 0.01]
	condition × BAQ	346	-0.05	-0.36	0.72	0.00	[0.00, 0.00]
	BSCS × BAQ	346	0.06	0.67	0.51	0.00	[0.00, 0.01]
	condition × KIMS × BSCS	346	-0.04	-0.41	0.68	0.00	[0.00, 0.00]
condition × BSCS × BAQ	346	-0.04	-0.37	0.71	0.00	[0.00, 0.00]	

Note. Aggression refers to the product of blast intensity and blast duration in the Competitive Reaction Time Task (CRTT). KIMS: trait mindfulness; BSCS: trait self-control; BAQ: trait aggression. There are no significant interactions.

* $p < 0.05$. Rows with grey shading indicate statistical significance.

Table 1.5

Testing the Condition × Self-Control Interaction (Study 3)

Dependent Variable	Predictor	<i>df</i>	β	<i>t</i>	<i>p</i>	<i>sr</i> ²	95% CI
Aggression	condition	428	0.04	0.45	0.65	0.00	[0.00, 0.00]
	BSCS	428	-0.07	-1.06	0.29	0.00	[0.00, 0.01]
	condition × BSCS	428	0.16	1.64	0.10	0.01	[0.00, 0.02]
Positive Affect	condition	428	-0.03	-0.36	0.72	0.00	[0.00, 0.00]
	BSCS	428	0.20	3.01	< 0.01**	0.02	[0.00, 0.05]
	condition × BSCS	428	0.02	0.19	0.85	0.00	[0.00, 0.00]
Negative Affect	condition	428	-0.05	-0.59	0.56	0.00	[0.00, 0.01]
	BSCS	428	-0.28	-4.26	< 0.01***	0.04	[0.00, 0.07]
	condition × BSCS	428	-0.09	-1.04	.30	0.00	[0.00, 0.01]
State Hostility	condition	428	-0.01	-0.16	0.88	0.00	[0.00, 0.00]
	BSCS	428	-0.31	-4.84	< 0.01***	0.05	[0.01, 0.09]
	condition × BSCS	428	-0.01	-0.13	0.90	0.00	[0.00, 0.00]
State Hostility (feeling mean)	condition	428	-0.01	-0.10	0.92	0.00	[0.00, 0.00]
	BSCS	428	-0.29	-4.45	< 0.01***	0.04	[0.01, 0.08]
	condition × BSCS	428	-0.03	-0.30	0.77	0.00	[0.00, 0.00]
State Hostility (aggravation)	condition	428	-0.01	-0.11	0.92	0.00	[0.00, 0.00]
	BSCS	428	-0.31	-4.79	< 0.01***	0.05	[0.01, 0.09]
	condition × BSCS	428	-0.01	-0.10	0.92	0.00	[0.00, 0.00]

Note. Aggression refers to the product of blast intensity and blast duration in the Competitive Reaction Time Task (CRTT). BSCS: trait self-control. There are no significant interactions.

** $p < 0.01$, *** $p < 0.001$. Rows with grey shading indicate statistical significance.

Table 1.6

Exploring Other Personality Moderators of Priming Mindfulness (Study 3)

Dependent Variable	Predictor	<i>df</i>	β	<i>t</i>	<i>p</i>	<i>sr</i> ²	95% CI	
Aggression	condition	420	0.05	0.58	0.56	0.00	[0.00, 0.01]	
	KIMS	420	0.06	0.84	0.40	0.00	[0.00, 0.01]	
	BSCS	420	0.02	0.32	0.75	0.00	[0.00, 0.00]	
	BAQ	420	0.20	2.69	< 0.01**	0.02	[0.00, 0.04]	
	SOPT	420	0.15	2.31	0.02*	0.01	[0.00, 0.03]	
	IAT	420	-0.08	-1.12	0.26	0.00	[0.00, 0.01]	
	condition × KIMS	420	0.03	0.30	0.77	0.00	[0.00, 0.00]	
	condition × BSCS	420	0.12	1.11	0.27	0.00	[0.00, 0.01]	
	condition × BAQ	420	0.05	0.50	0.62	0.00	[0.00, 0.00]	
	condition × SOPT	420	0.00	0.02	0.99	0.00	[0.00, 0.00]	
	condition × IAT	420	0.12	1.26	0.20	0.00	[0.00, 0.01]	
	Positive Affect	condition	420	-0.01	-0.10	0.92	0.00	[0.00, 0.00]
		KIMS	420	0.29	3.78	< 0.01***	0.03	[0.00, 0.06]
BSCS		420	0.14	1.80	0.07	0.01	[0.00, 0.02]	
BAQ		420	0.12	1.56	0.12	0.01	[0.00, 0.02]	
SOPT		420	0.07	1.04	0.30	0.00	[0.00, 0.01]	
IAT		420	0.06	0.88	0.38	0.00	[0.00, 0.01]	
condition × KIMS		420	0.01	0.06	0.96	0.00	[0.00, 0.00]	

	condition × BSCS	420	-0.02	-0.20	0.84	0.00	[0.00, 0.00]
	condition × BAQ	420	-0.02	-0.23	0.82	0.00	[0.00, 0.00]
	condition × SOPT	420	0.02	0.17	0.87	0.00	[0.00, 0.00]
	condition × IAT	420	-0.04	-0.40	0.69	0.00	[0.00, 0.00]
	condition	420	-0.05	-0.62	0.54	0.00	[0.00, 0.01]
	KIMS	420	-0.29	-4.10	< 0.01***	0.03	[0.00, 0.06]
	BSCS	420	-0.05	-0.73	0.47	0.00	[0.00, 0.01]
	BAQ	420	0.24	3.54	< 0.01***	0.02	[0.00, 0.05]
	SOPT	420	0.01	0.10	0.92	0.00	[0.00, 0.00]
	IAT	420	-0.04	-0.58	0.56	0.00	[0.00, 0.00]
Negative Affect	condition × KIMS	420	0.02	0.25	0.81	0.00	[0.00, 0.00]
	condition × BSCS	420	-0.11	-1.06	0.29	0.00	[0.00, 0.01]
	condition × BAQ	420	-0.06	-0.62	0.54	0.00	[0.00, 0.01]
	condition × SOPT	420	0.14	1.61	0.11	0.00	[0.00, 0.02]
	condition × IAT	420	0.01	0.16	0.874	0.00	[0.00, 0.00]
		condition	420	-0.03	-0.34	0.74	0.00
	KIMS	420	-0.24	-3.49	0.01***	0.02	[0.00, 0.04]
	BSCS	420	-0.07	-1.08	0.28	0.00	[0.00, 0.01]
State Hostility	BAQ	420	0.25	3.75	< 0.01***	0.02	[0.00, 0.05]
	SOPT	420	0.11	1.87	0.06	0.01	[0.00, 0.02]
	IAT	420	-0.11	-1.71	0.09	0.01	[0.00, 0.02]
		condition	420	-0.03	-0.34	0.74	0.00

	condition × KIMS	420	0.04	0.44	0.66	0.00	[0.00, 0.00]
	condition × BSCS	420	-0.03	-0.30	0.77	0.00	[0.00, 0.00]
	condition × BAQ	420	0.00	0.02	0.99	0.00	[0.00, 0.00]
	condition × SOPT	420	0.07	0.88	0.38	0.00	[0.00, 0.01]
	condition × IAT	420	0.03	0.36	0.72	0.00	[0.00, 0.00]
	condition	420	-0.02	-0.25	0.81	0.00	[0.00, 0.00]
	KIMS	420	-0.23	-3.30	< 0.01**	0.02	[0.00, 0.04]
	BSCS	420	-0.05	-0.72	0.47	0.00	[0.00, 0.01]
	BAQ	420	0.26	3.78	< 0.01***	0.03	[0.00, 0.05]
	SOPT	420	0.13	2.24	0.03*	0.01	[0.00, 0.02]
	IAT	420	-0.12	-1.78	0.08	0.01	[0.00, 0.02]
State Hostility (feeling mean)	condition × KIMS	420	0.07	0.72	0.47	0.00	[0.00, 0.01]
	condition × BSCS	420	-0.06	-0.55	0.58	0.00	[0.00, 0.00]
	condition × BAQ	420	0.03	0.28	0.78	0.00	[0.00, 0.00]
	condition × SOPT	420	0.08	0.92	0.36	0.00	[0.00, 0.01]
	condition × IAT	420	0.05	0.56	0.57	0.00	[0.00, 0.00]
	condition	420	-0.02	-0.25	0.80	0.00	[0.00, 0.00]
	KIMS	420	-0.24	-3.36	0.01***	0.02	[0.00, 0.04]
	BSCS	420	-0.09	-1.28	0.20	0.00	[0.00, 0.01]
	BAQ	420	0.24	3.48	< 0.01***	0.02	[0.00, 0.05]
	SOPT	420	0.07	1.20	0.23	0.00	[0.00, 0.01]

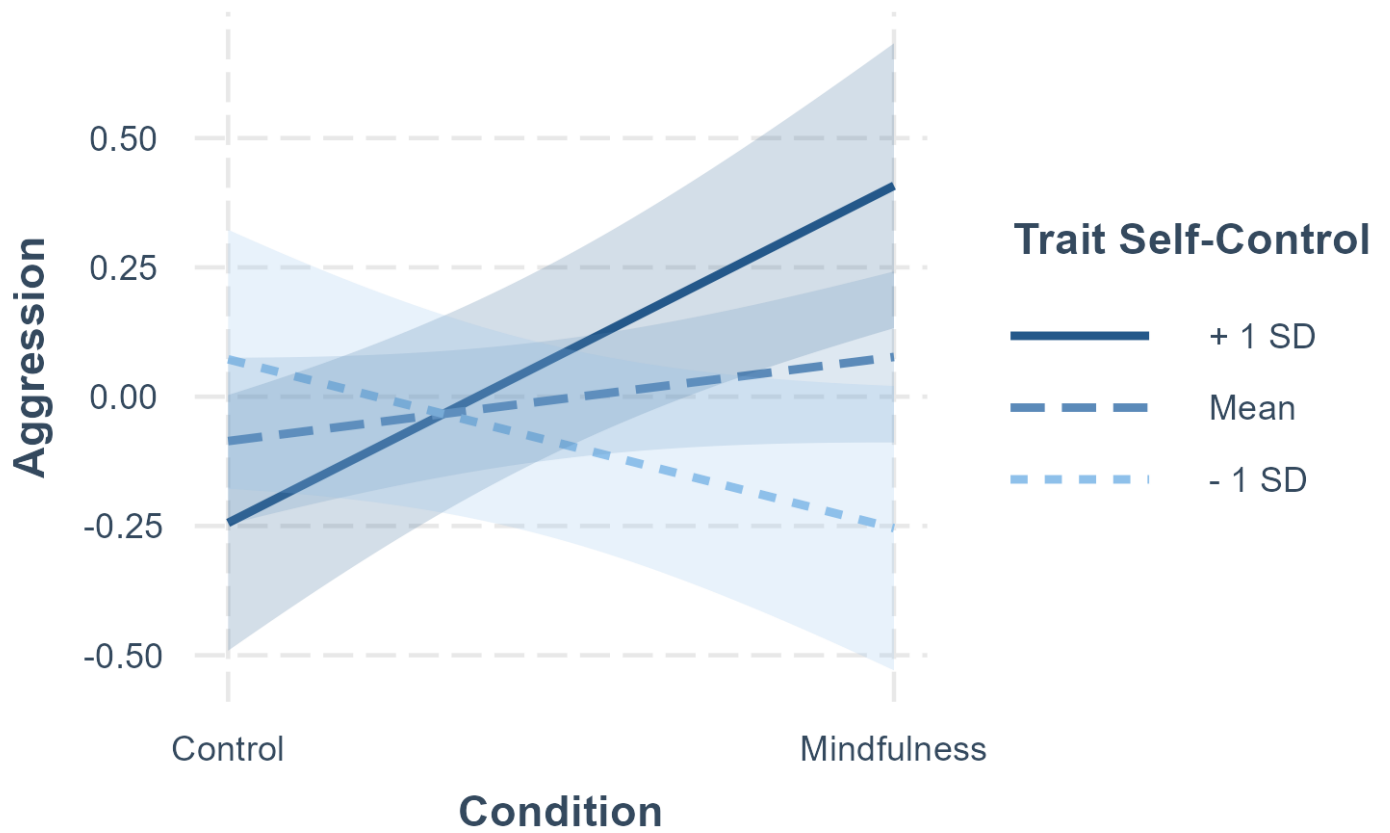
IAT	420	-0.09	-1.39	0.17	0.00	[0.00, 0.01]
condition × KIMS	420	0.01	0.13	0.90	0.00	[0.00, 0.00]
condition × BSCS	420	-0.02	-0.18	0.85	0.00	[0.00, 0.00]
condition × BAQ	420	-0.03	-0.29	0.77	0.00	[0.00, 0.00]
condition × SOPT	420	0.08	0.98	0.33	0.00	[0.00, 0.01]
condition × IAT	420	0.02	0.22	0.82	0.00	[0.00, 0.00]

Note. Aggression refers to the product of blast intensity and blast duration in the Competitive Reaction Time Task (CRTT). KIMS: trait mindfulness; BSCS: trait self-control; BAQ: trait aggression; SOPT: working memory; IAT: implicit aggression. There are no significant interactions.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Rows with grey shading indicate statistical significance.

Figure 1.1

Simple Slopes of the Self-Control by Condition Interaction



Note. Simple slopes of the self-control by condition interaction. Note. Interaction effects between trait self-control (at +1/-1 standard deviation) and the experimental conditions on the composite aggression index. The interaction is significant.

3. SEGMENT TRANSITIONNEL

L'article 1 avait pour but de d'évaluer pour *qui* l'activation *situationnelle* de la présence attentive (via une tâche d'amorçage) réduit le plus les niveaux d'agression. Les résultats de cet article suggèrent que, ultimement, cette tâche d'amorçage de la présence attentive n'affecte pas de manière fiable les niveaux d'agression; les effets de cette tâche seraient peut-être plus spécifiques aux variables orientées vers le soi et qui aident à récupérer d'une menace à l'égo. Par conséquent, il ne semble qu'aucun des traits de personnalité testés (trait de contrôle de soi, trait d'agressivité, trait de présence attentive, agression implicite et mémoire de travail) ne modère l'effet de l'activation situationnelle de la présence attentive sur les niveaux d'agression.

L'article 2, quant à lui, avait pour but d'évaluer dans *quelles circonstances* une activation *soutenue* de la bienveillance (via deux types d'entraînement à cet effet) réduit le plus les niveaux d'agression (tel que dans l'article 1) et augmente le plus les niveaux de prosocialité (une nouveauté de l'article 2). Une transition de la présence attentive vers une méditation spécifiquement axée sur la bienveillance avait pour but de maximiser les effets prosociaux qui était le but de cette démarche. Cet article visait également à déterminer si la méthode de pratique de la bienveillance (via la méditation ou la réflexion) venait influencer ces dynamiques sociales. Enfin, il visait également à évaluer la stabilité des effets *dans le temps*. Celui-ci explore donc ces questions via un essai randomisé contrôlé de six semaines et inclut, au temps 2 (immédiatement après l'intervention), une manipulation de fatigue cognitive visant à évaluer la stabilité de ces effets malgré des circonstances visant à réduire les ressources cognitives. Un suivi six semaines après la fin de l'intervention permet également de répondre à la question de la stabilité dans le temps.

4. ARTICLE 2 : IS THE MIND MORE POWERFUL THAN THE HEART? A RANDOMIZED CONTROLLED TRIAL OF TWO LOVING-KINDNESS INTERVENTIONS

Cette section introduit l'article 2. De nombreuses traditions spirituelles, comme le bouddhisme, soulignent l'importance de développer des sentiments d'amour et d'altruisme envers les autres quelles que soient les circonstances, mais cela peut être difficile, en particulier dans les situations

où les individus ont peu de ressources de contrôle de soi, par exemple en raison de la fatigue cognitive. Ces traditions proposent des techniques pour entraîner l'esprit, comme la méditation de bienveillance, afin de faciliter la réalisation de cet objectif altruiste. Cependant, les preuves en faveur des avantages prosociaux de la méditation de bienveillance sont rares et parfois contradictoires.

Pour étudier les effets à long terme de l'entraînement à la bienveillance sur la prosocialité et l'agressivité, nous avons étudié les changements dans une série de variables prosociales avant, après et au suivi dans un essai contrôlé randomisé préenregistré comparant trois groupes : méditation de bienveillance ($N = 58$), réflexion sur la bienveillance ($N = 53$) et groupe témoin par liste d'attente ($N = 92$). De plus, pour établir la robustesse de l'entraînement à la bienveillance par rapport à des circonstances difficiles telles que la fatigue cognitive, nous avons introduit une tâche d'épuisement mental lors du post-test.

Les analyses de contrastes planifiées ont suggéré des effets sur l'affect positif uniquement pour le groupe de méditation, sur l'autocompassion pour les groupes de méditation et de réflexion, et sur les attitudes sociales explicites, le comportement agressif, l'accessibilité cognitive d'un comportement altruiste récent, la volonté d'aider, uniquement pour le groupe de réflexion. Il n'y avait pas d'autres différences de groupe dans les attitudes envers l'agression, la déshumanisation, l'agression implicite ou les dons de charité. Les analyses de modération ont révélé des interactions à trois voies entre le groupe, les attitudes implicites et l'épuisement de l'égo, comme prévu, mais uniquement pour la compassion et l'accessibilité cognitive d'un comportement altruiste récent.

En conclusion, les résultats suggèrent que les effets de la réflexion sur les concepts de bienveillance fournissent des effets prosociaux robustes et fiables, peut-être plus que la méditation sur ces concepts. Même ces avantages sont limités à certains comportements, cognitions ou affects, et ils ne se généralisent pas à de nombreux comportements courants tels que les dons de charité. Cependant, l'épuisement de l'égo ne semble modérer les attitudes implicites que lorsque la bienveillance est incarnée par la méditation et non lorsqu'elle se limite à l'esprit intellectuel.

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

Rémi Thériault*, Romane Masson, & Stéphane Dandeneau[†]
Department of Psychology, Université du Québec à Montréal, Québec, Canada

Declarations. [†]Stéphane Dandeneau is now affiliated to the Department of Psychology at the Memorial University of Newfoundland. *Funding:* This work was supported by the Mind & Life Institute Francisco J. Varela Research Grant [grant number A-95109536]. The sponsors had no role in the study design; in the collection, analysis, and interpretation of data; in the report writing; or in the decision to submit the article for publication. Any views, findings, conclusions, or recommendations expressed in this publication do not necessarily reflect those of the Mind & Life Institute. *Competing interests:* The authors report no conflict of interest. *Ethics approval & Consent:* A university research ethics board approved this study, and participants consented to participate. *Data, Materials and Code availability:* We preregistered this study on the Open Science Framework (OSF; <https://osf.io/gkd8s>). The data, analysis scripts, analysis reports, and supplemental materials are available on the corresponding OSF project (<https://osf.io/5khjm>). *Acknowledgements:* The authors thank Pierre-Vincent Breault-Ruel for preparing the recordings of the meditations and podcasts, Marion Archambault for help with preparation of the study materials and pilot studies, Frederick L. Philippe, Marina Doucerain, Simon Grégoire, and Yoona Kang for comments on a previous version of this manuscript, Hugues Leduc for critical statistical guidance, Despina Z. Artenie for help with questionnaire translations, Yoona Kang for experimental advice, Jay Olson for advice on data visualization, and the Mind & Life Institute for supporting this research. *Author contributions:* Conceptualization, Methodology, and Funding acquisition: Rémi Thériault, Stéphane Dandeneau; Software, Data Curation, and Visualization: Rémi Thériault; Formal analysis, investigation, Writing - original draft preparation, and Project administration: Rémi Thériault, Romane Masson; Writing - review and editing: Rémi Thériault, Romane Masson, Stéphane Dandeneau; Resources and Supervision: Stéphane Dandeneau. **Correspondence:* Correspondence concerning this article should be addressed to Rémi Thériault, Department of Psychology, Université du Québec à Montréal, C.P. 8888, Succursale Centre-Ville, Montréal, Québec, Canada, H3C 3P8. E-mail: theriault.remi@courrier.uqam.ca.

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.

4.1 Introduction

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 (Frieze & Hofmann, 2009; Hofmann, Gschwendner, Frieze, 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 (Frieze 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.

4.1.1 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.

4.2 Materials and Methods

4.2.1 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>.

4.2.2 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.

4.2.3 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.

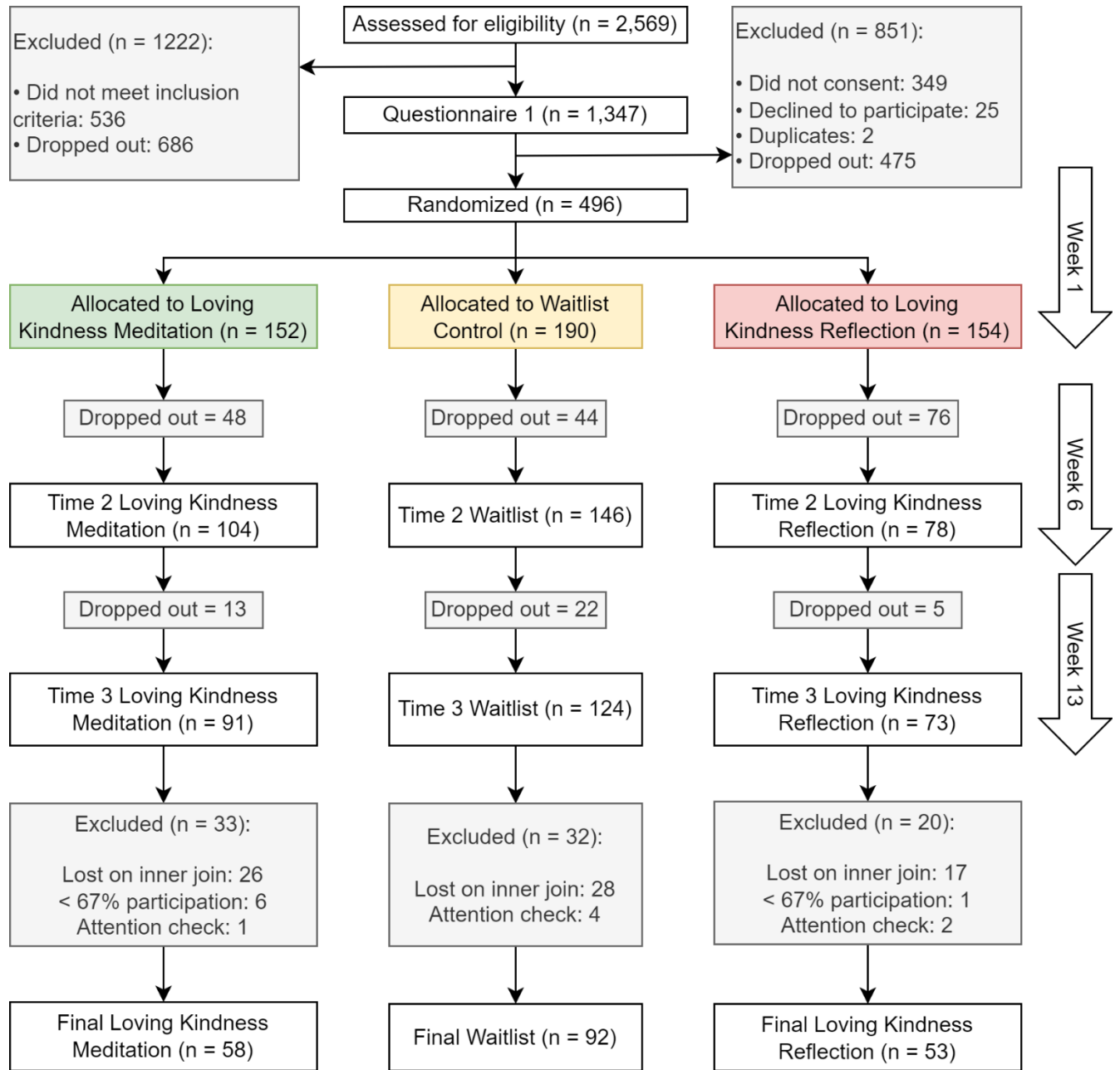
We recruited participants through online Facebook groups, as well as online and physical ads, in majority (but not exclusively) aimed at students in the greater Montreal area. Data collection started in October 2020 and ended in December 2022¹. Our inclusion criteria required that participants (1) be 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 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 2.1 for the participant flow diagram). Participant demographics for the final sample are available in

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.

Table 2.1. 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 2.1

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 2.1

Sample Demographics Split by Intervention Group

Characteristic	Meditation (<i>N</i> = 58)	Reflection (<i>N</i> = 53)	Waitlist (<i>N</i> = 92)	F / χ^2	<i>df</i>	<i>p</i>
Age	25 (\pm 4.5)	25 (\pm 3.7)	24 (\pm 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.

4.2.4 Description of Groups

4.2.4.1 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.

4.2.4.2 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.

4.2.4.3 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.

4.2.5 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.

4.2.6 Measures

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

Trait aggression. We used the *Brief Aggression Questionnaire* ($\omega = .79$; 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 website² (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* ($\omega_{T1} = .85$, $\omega_{T2} = .88$, $\omega_{T3} = .89$; 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; $\omega_{T1} = .94$, $\omega_{T2} = .96$, $\omega_{T3} = .96$; 9 items). Groups represented include Blacks, homeless, 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).

³ “Competitive Reaction Time Task (Complete) - English”, available at: <https://www.millisecond.com/download/library/competitivereactiontime/>

Dehumanization. We measured dehumanization through the *Dehumanization Scale* ($\omega T1 = .96$, $\omega T2 = .97$, $\omega T3 = .96$; 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; $\omega T1 = .67$, $\omega T2 = .68$, $\omega T3 = .69$; 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* ($\omega T1 = .94$, $\omega T2 = .95$, $\omega T3 = .95$; 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 website⁴ 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 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., 2014b; 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

⁴ “Color Word Stroop with Keyboard Responding - English”, available at: <https://www.millisecond.com/download/library/stroop/>

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* ($\omega = .67$; 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.

4.2.7 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.

4.3 Results

4.3.1 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).

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 internationale, 21 – Greenpeace, 22 – Fonds mondial pour la nature (WWF), 23 – Médecins sans frontières (MSF), 24 – Armée du Salut).

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 $3 \times 10 = 30$ analyses in total.

4.3.2 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 ($\chi^2 = 26.57, p = < .001$; 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 ($\chi^2 = 3.57, p = .168$; 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 2.2 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.

4.3.3 Group Differences at Time 1

Despite the differential attrition identified, ANOVA analyses presented in Table 2.2 suggest groups did not differ on variables measured at baseline (Time 1).

Table 2.2

Baseline Differences by Intervention Group

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

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

4.3.4 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 2.3).

Figure 2.3 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 2.4).

Table 2.3

Planned Comparisons at Time 2

Dependent Variable	Comparison	<i>df</i>	<i>t</i>	<i>p</i>	<i>d</i>
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

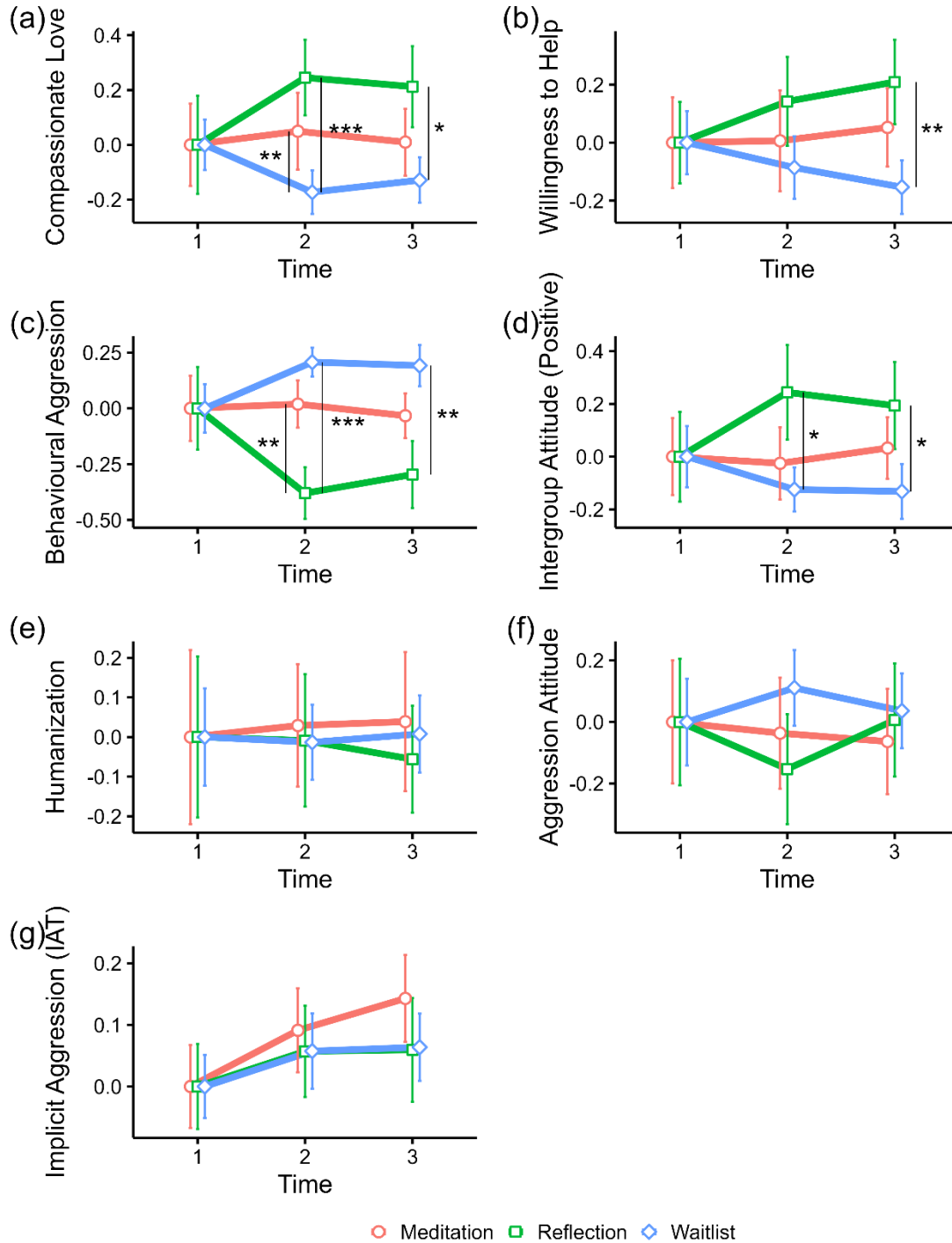
	Reflection - Waitlist	197	1.17	.243	-0.01
CLS	Meditation - Reflection	197	-1.10	.274	-0.03
	Meditation - Waitlist	197	2.46	.015*	0.42
	Reflection - Waitlist	197	3.61	< .001***	0.45
Charity	Meditation - Reflection	197	-0.38	.704	-0.09
	Meditation - Waitlist	197	-1.37	.174	-0.19
	Reflection - Waitlist	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 \times 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 2.3

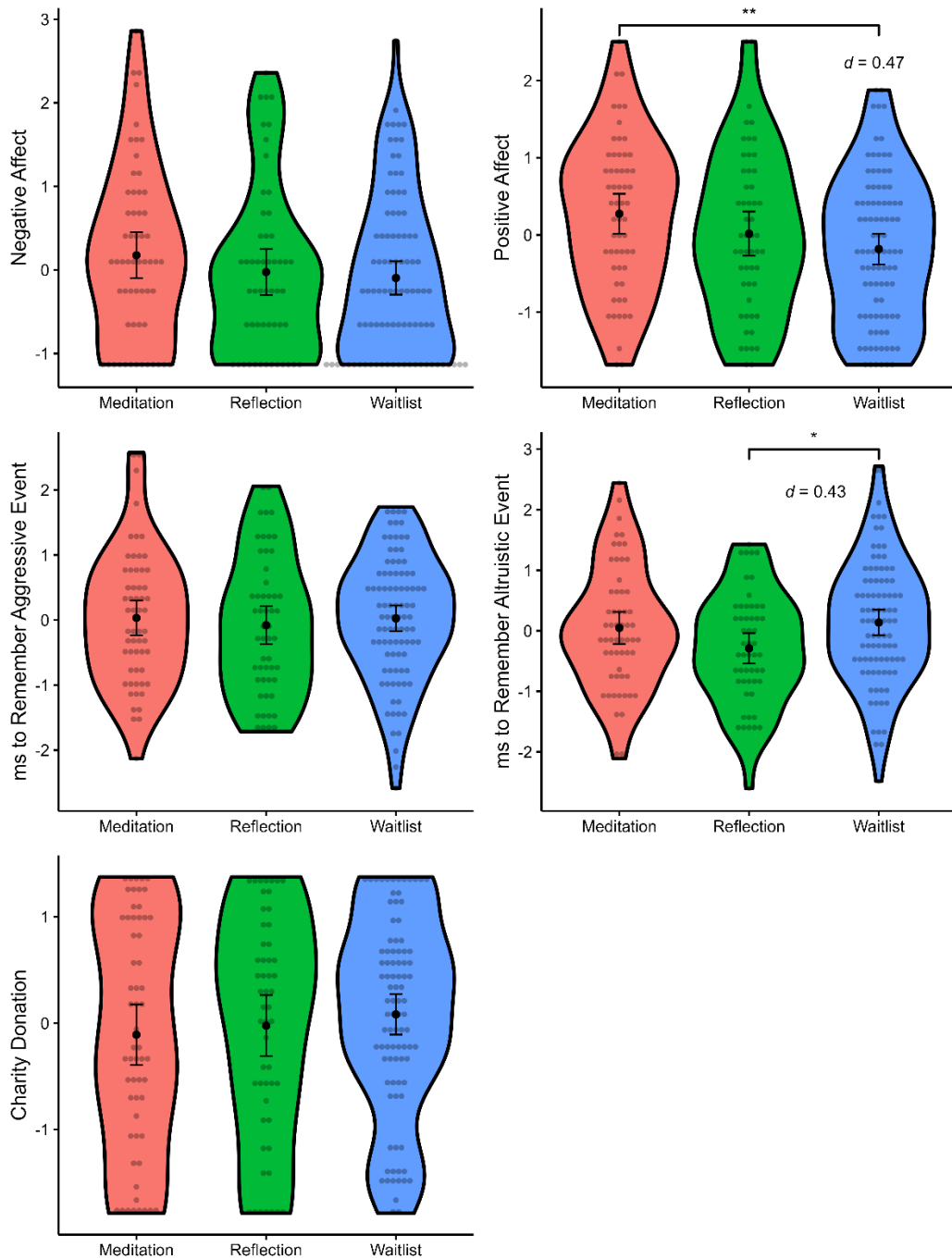
Plot of Group Means Over Time



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 2.4

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.

4.3.5 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 2.4). 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 2.4
Planned Comparisons at Time 3

Dependent Variable	Comparison	<i>df</i>	<i>t</i>	<i>p</i>	<i>d</i>
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 \times duration); WHS = Willingness to Help Scale; CLS = Compassionate Love Scale.

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

4.3.6 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.

4.3.7 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 $2 \times 4 = 8$ 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 \times level of ego depletion \times *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 \times ego depletion \times explicit aggression interactions on all of the outcome measures.

Table 2.5 and Table 2.6, Figure 2.5), and not on behavioural aggression, charity, helping, or cognitive accessibility of a recent aggressive behaviour.

As shown in Figure 2.5, 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 2.5, 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 \times ego depletion \times explicit aggression interactions on all of the outcome measures.

Table 2.5

Moderation Analyses for Three-Way Interaction on Compassionate Love

Dependent Variable	Predictor	<i>df</i>	<i>b</i>	<i>t</i>	<i>p</i>	<i>sr</i> 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 \times IAT	191	1.72	2.88	.004**	.04	[0.00, 0.08]
	GroupMeditation \times IAT	191	1.81	2.39	.018*	.03	[0.00, 0.07]
	GroupReflection \times ConditionDepleted	191	0.09	0.13	.899	.00	[0.00, 0.00]
	GroupMeditation \times ConditionDepleted	191	-1.91	-2.62	.010**	.03	[0.00, 0.07]
	IAT \times ConditionDepleted	191	1.98	3.49	.001***	.05	[0.00, 0.11]
	GroupReflection \times IAT \times ConditionDepleted	191	-0.00	-0.00	.998	.00	[0.00, 0.00]
GroupMeditation \times IAT \times 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 2.6

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

Dependent Variable	Predictor	<i>df</i>	<i>b</i>	<i>t</i>	<i>p</i>	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.

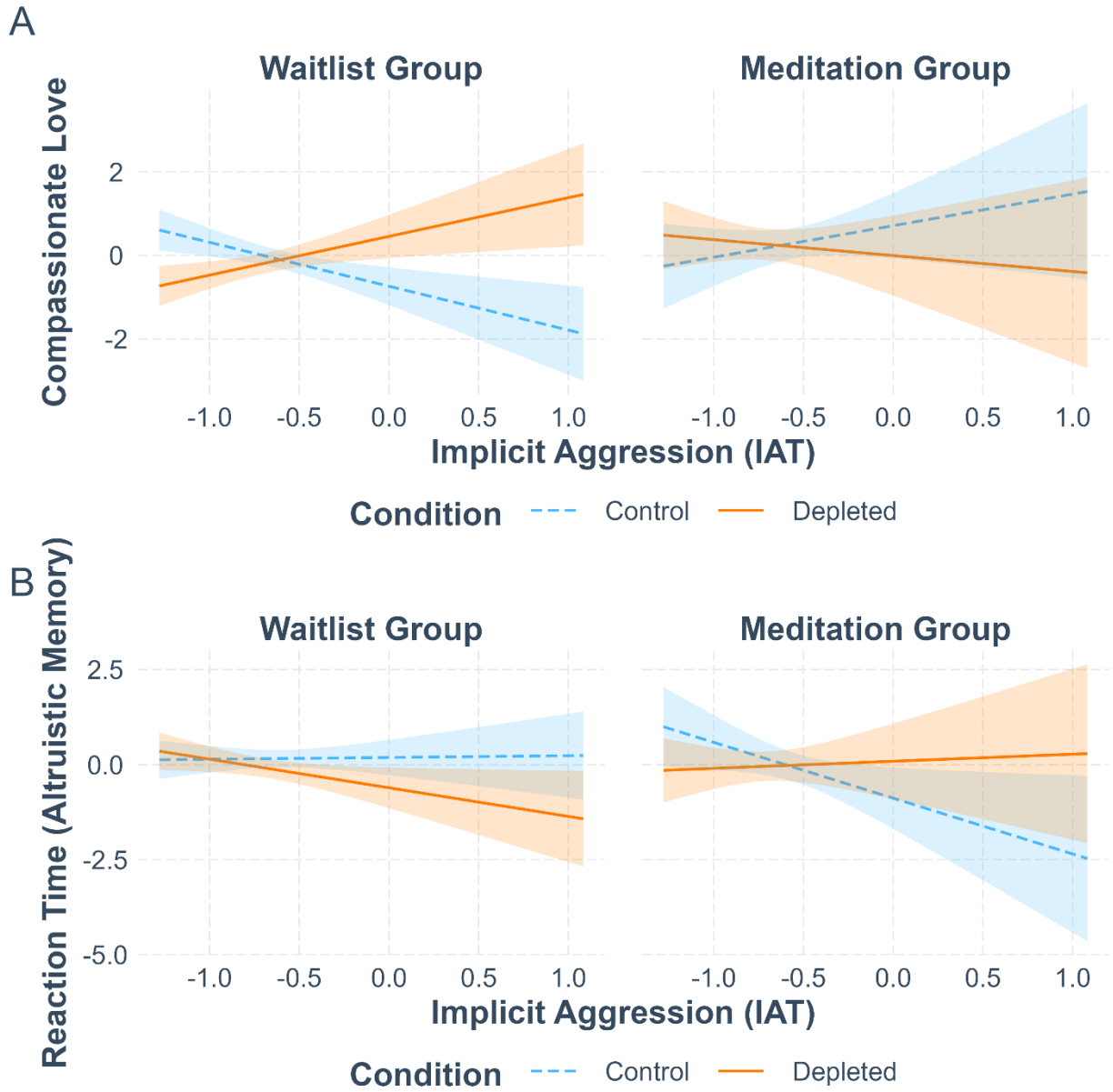
We report the squared semi-partial correlation (sr^2), also known as the delta R squared (ΔR^2), as an index of effect size. The sr^2 allows us to quantify the unique contribution (proportion of variance

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 2.5

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



Note. Moderation Plots show two three-way interactions.

4.4 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 2.7.

Table 2.7

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.

4.4.1 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.

4.4.2 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.

4.4.2.1 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).

4.4.2.2 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).

4.4.2.3 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., 2014a), 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., 2014a). 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.

4.4.2.4 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; Leiberger 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).

4.4.2.5 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., 2019b; 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).

4.4.3 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 \times 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.

4.4.4 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.

4.4.4.1 Implicit Attitudes and Compassion

Implicit aggression influenced compassionate love in a three-way interaction in the meditation vs waitlist contrast (IAT \times Meditation Group \times Depletion). There was no three-way interaction in

the reflection vs waitlist contrast (IAT \times Reflection Group \times 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 (Frieze 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.

4.4.4.2 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 \times Meditation Group \times Depletion). There was no three-way interaction in the reflection vs waitlist contrast (IAT \times Reflection Group \times 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 (Frieze 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.

4.4.4.3 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 2.7), 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).

4.4.5 Limitations

This study has many limitations. First, 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.

Second, 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.

Third, 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.

Finally, the intervention for all groups was 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.

4.4.6 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.

4.4.7 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.

5. DISCUSSION GÉNÉRALE

Cette section comporte trois sous-sections. La première résume les résultats des articles 1 et 2. La deuxième sous-section souligne les retombées théoriques, contributions et implications des résultats conjointement considérés. Enfin, la troisième sous-section discute des limites des présentes études et propose de nouvelles pistes de recherche pour le futur.

5.1 Résumé de la thèse et synthèse des résultats

Inspiré de la perspective de l'interaction entre « la personne et la situation » (Ross et al., 2011), cette thèse visait d'une manière générale à investiguer pour *qui* les interventions de présence attentive et d'amour-bienveillant produisent les plus grands effets sur l'agression et la prosocialité, dans *quel contexte*, en fonction de *quelle durée d'intervention* et enfin en fonction de *quelle méthode de pratique*. Le premier article investiguait une exposition très minimale à la présence attentive (activation *situationnelle*), à travers une brève tâche d'amorçage (priming), alors que le deuxième article investiguait une très grande exposition à l'amour-bienveillant (activation *chronique*), à travers un entraînement quotidien sur six semaines.

L'article 1 investiguait pour qui l'effet de la tâche d'amorçage de la présence attentive était le plus puissant, soit en testant de nombreux modérateurs sous forme de traits de personnalité. L'article 2 quant à lui testait l'effet modérateur additionnel de la *situation* (au lieu de traits de personnalité comme dans l'article 1), soit l'interaction entre les attitudes (implicites et explicites) et l'accessibilité des ressources cognitives, via une manipulation expérimentale de fatigue cognitive (plus connu sous le nom de « ego depletion »). L'article 2 testait également l'efficacité de la méthode de pratique en comparant deux interventions d'amour-bienveillant de six semaines

chacune, soit un groupe qui *méditait* sur l'amour-bienveillant ainsi qu'un groupe qui *réfléchissait* à l'amour-bienveillant. Les résultats des présentes études ont permis de répondre en grande partie à ces questions.

5.1.1 Des effets pour qui et pour quelle durée d'intervention

Les résultats de l'étude 1 de l'article 1 ont d'abord suggéré que parmi tous les modérateurs de traits de personnalité testés (trait de contrôle de soi, trait d'agression, trait de présence attentive, agression implicite, et mémoire de travail), seul le trait de contrôle de soi interagissait avec la condition d'amorçage de la présence attentive pour prédire le comportement agressif (Thériault & Dandeneau, 2023). Que le trait de contrôle de soi interagisse avec une condition expérimentale dans la prédiction du comportement était consistant avec le modèle dualistique de l'impulsivité et du contrôle de soi (Hofmann et al., 2009). Cependant, le patron de résultats était quelque peu surprenant et à l'inverse de nos hypothèses; en effet, relatif au groupe témoin, la condition d'amorçage de la présence attentive semblait augmenter le niveau de comportement agressif chez les individus avec un haut niveau de contrôle de soi, possiblement car la tâche invite au « lâcher prise ».

Une étude de suivi ciblée, simplifiée et préenregistrée, néanmoins, n'a pas permis de répliquer cette interaction entre le trait de contrôle de soi. Puisque cette étude de suivi différait de la première étude dans la mesure où les personnes participantes complétaient dans l'étude originale beaucoup plus de tâches cognitivement exigeantes, nous avons testé dans une troisième étude la possibilité que l'effet original ne ressorte que lorsque les personnes participantes complétaient d'abord ces tâches cognitivement exigeantes. Nous avons donc inclus dans cette troisième étude toutes les mêmes mesures que dans l'étude originale, en tentant une étroite répllication.

Néanmoins, même cette étroite répllication n'a pas permis de retrouver le même patron de résultats que dans l'étude originale. À vrai dire, la condition d'amorçage de la présence attentive n'a interagit avec aucun des traits de personnalité testés (trait de présence attentive, trait de contrôle de soi, trait d'agression, mémoire de travail, agression implicite) non seulement pour l'agression, mais également pour l'affect positif, l'affect négatif, ou l'état d'hostilité.

Les études préenregistrées aident à réduire la probabilité de faux positifs (Nosek et al., 2019; Nosek et al., 2018), et nos deux études de suivi préenregistrées n'ont pas relevé d'interaction avec la tâche d'amorçage de la présence attentive. Ceci nous permet d'établir avec un certain degré de confiance que cette tâche n'interagit pas avec de multiples indicateurs de personnalité pour prédire des variables interpersonnelles telles que le comportement agressif et l'état d'hostilité, même si elle affecte d'autres aspects cognitifs et affectifs dans certains contextes, par exemple lorsqu'elle est précédée d'une menace au concept de soi (Bergeron et al., 2016; Bergeron & Dandeneau, 2016).

Ainsi, à la question à savoir pour *qui* l'amorçage de la présence attentive est la plus efficace, une réponse simplifiée est *personne*, du moins pour les variables interpersonnelles testées dans la présente étude. À la question à savoir *quelle durée de l'intervention* de présence attentive est nécessaire pour ces variables interpersonnelles, la réponse semble donc également être qu'un simple bref amorçage (activation *situationnelle*) n'est pas suffisant pour causer des effets observables sur un comportement d'agression explicite. Il est possible qu'une durée d'intervention de présence attentive plus grande (par exemple en utilisant une méthodologie différente d'un amorçage temporaire) ait pu permettre de détecter des effets modérateurs de différents traits de personnalité sur ces variables interpersonnelles.

5.1.2 Durée d'intervention, méthode de pratique et contexte

Durée d'intervention. Ceci nous amène donc à la question à savoir si une durée d'intervention considérablement plus grande, soit une pratique quotidienne sur six semaines (activation *soutenue*), serait quant à elle suffisante pour trouver des effets observables et significatifs. C'est un des aspects que l'article 2 visait à éclairer. Si l'on adopte l'approche contemplative classique basée sur la méditation, l'article 2 permet de répondre à cette question via la comparaison du groupe de méditation vis-à-vis du groupe témoin en regardant les effets directs de l'intervention.

Donc à la question à savoir quelle durée d'intervention est nécessaire, il semble que six semaines d'entraînement quotidien soit en effet suffisant pour trouver des effets, mais seulement sur certaines variables, telles que l'amour-compassionné et l'affect positif. On ne trouve ainsi pas d'effet sur l'attitude envers l'agression, l'attitude envers divers groupes sociaux, le sentiment de

déshumanisation envers différents groupes sociaux, l'agression implicite, l'affect négatif, l'accessibilité cognitive de souvenirs altruistes ou agressifs, la motivation à aider, le comportement de don, ou le comportement agressif—malgré un entraînement quotidien de six semaines de méditation.

Une question connexe à la durée d'intervention est la question de la durée des effets lorsque les personnes participantes arrêtent leur pratique de méditation. Ainsi, six semaines après la fin de l'intervention (soit la même durée que l'intervention elle-même), nous avons fait un suivi avec les personnes participantes pour évaluer si d'une part de nouveaux effets émergeaient et d'autre part si les effets détectés immédiatement après l'intervention se maintenaient dans le temps. Cette question est importante puisque les études sur la méditation sont fréquentes, mais celles qui regardent les effets dans le temps sont rares. Pourtant, les implications d'un effet persistant d'une pratique contemplative dans le temps est cruciale puisqu'en pratique, de nombreuses personnes pratiquent la méditation ou font une retraite avant de prendre une pause ou d'arrêter complètement. Les résultats de l'article 2 suggèrent que le seul effet trouvé pour le groupe de méditation qui était également mesuré lors du suivi, soit sur l'amour-compassionné, ne persiste pas six semaines plus tard, malgré l'activation supposément soutenue de la bienveillance sur six semaines. Cela suggère qu'il est important de conserver la pratique pour préserver les effets bénéfiques de la méditation d'amour-bienveillant, et conséquemment, que la durée d'intervention est fort importante.

Méthode de pratique. Pour répondre à la question de la durée d'intervention, nous avons jusqu'ici assumé l'approche contemplative classique basée sur la méditation. Cependant, la méthode de pratique utilisée pour transmettre les bienfaits de l'amour-bienveillant pourrait aussi jouer un rôle important. Pour répondre à ce questionnement, nous avons également développé une nouvelle intervention de *réflexion* sur l'amour-bienveillant, dans laquelle les personnes participantes étaient invités à écouter des balados et à lire des extraits de livre sur le sujet. Étant donné la plus grande proximité de notre échantillon hautement éduqué (une majorité ayant suivi un cours de psychologie) au monde des idées et du mental que celle du rapport au corps et à l'incarnation, il est possible que nos personnes participantes soient plus réceptives à une approche plus intellectuelle et cognitive qu'une approche incarnée, affective, et attentionnelle.

Les résultats de l'essai randomisé contrôlé de l'article 2 suggère en effet qu'une approche cognitive et réflexive à l'amour-bienveillant semble avoir de nombreux bénéfices, qui éclipsent peut-être même les bienfaits de l'approche méditative à l'amour-bienveillant. En effet, relatif au groupe témoin, le groupe de réflexion sur l'amour bienveillant a démontré des bénéfices au niveau des attitudes envers divers groupes sociaux, au niveau de l'accessibilité de souvenirs de comportement altruistes, au niveau de l'amour-compassionné, et au niveau du comportement agressif, des effets qui se sont également maintenus six semaines après la fin de l'intervention (sauf pour la tâche des souvenirs qui n'était mesurée qu'une seule fois). Qui plus est, lors du suivi six semaines après la fin du traitement, un nouvel effet est apparu sur la motivation à aider autrui, ce qui suggère que l'effet de la réflexion continue à faire effet et à progresser au-delà de l'intervention en elle-même.

Ainsi, non seulement le groupe de réflexion a démontré plus de bénéfices relatifs au groupe témoin que le groupe de méditation, le groupe de réflexion a également montré un niveau d'agression comportemental statistiquement significativement plus bas que le groupe de méditation. Le seul bénéfice que le groupe de méditation a démontré relatif au groupe témoin et pour lequel le groupe de réflexion n'a pas démontré est au niveau de l'affect positif. Ces résultats nous permettent donc de conclure que la méthode de pratique de l'amour-bienveillant est un facteur important à considérer. Ainsi, ces résultats suggèrent qu'il est important d'arrimer la méthode de transmission des connaissances au public cible.

Contexte. Enfin, l'objectif de cette thèse était également de tester l'influence du contexte, et plus spécifiquement de la fatigue cognitive, sur la relation entre les attitudes, la prosocialité et les interventions de présence attentive et d'amour-bienveillant, basé sur le modèle dualistique de l'impulsion et du contrôle de soi (Hofmann et al., 2009). Nos résultats suggèrent que la fatigue cognitive semble modérer l'effet des attitudes sur la prosocialité différemment pour le groupe de méditation que pour le groupe témoin. Cependant, cela ne semble être le cas que pour les attitudes implicites : la fatigue cognitive ne démontre un effet modérateur des attitudes explicites pour aucune des variables dépendantes testées.

Même dans le cas des attitudes implicites, l'effet modérateur de la fatigue cognitive ne ressort que pour deux variables, la compassion et l'accessibilité de souvenirs altruistes, soit une mesure

explicite (auto-rapportée) et une mesure implicite (basée sur le temps de réaction). Cependant, aucun effet modérateur n'a été trouvé au niveau comportemental (agression, don de charité) ou au niveau de l'accessibilité de souvenirs agressifs ou de la motivation à aider autrui. Il semble donc que l'effet modérateur de la fatigue cognitive dans le contexte de l'amour-bienveillant soit spécifique aux attitudes implicites, à la compassion et à l'accessibilité de souvenirs altruistes.

5.2 Contributions et implications

Cette thèse contribue à l'avancement des connaissances en apportant de nombreuses retombées théoriques à l'intersection de la présence attentive, de l'amour-bienveillant, et du modèle dualistique de l'impulsion et du contrôle de soi. Au niveau des contributions aux interventions de présence attentive et d'amour-bienveillant, un premier apport est méthodologique, car nous avons ultimement testé trois différentes méthodes d'interventions : une exposition brève à la présence attentive (via un amorçage), une exposition longue à l'amour-bienveillant via la méditation et une exposition longue à l'amour-bienveillant via la réflexion (via des extraits de texte et balados).

5.2.1 Activation situationnelle implicite de la présence attentive

Bien que prometteuse, la tâche d'activation implicite et situationnelle de la présence attentive est à ce jour peu utilisée, ce qui limite notre compréhension de son champ d'application. Bien que nous sachions qu'elle possède de nombreux bénéfices au niveau *intrapersonnel* (Bergeron et al., 2016; Bergeron & Dandeneau, 2016), nous ne savions à ce jour pas si ces bénéfices s'étendaient également au niveau *interpersonnel*, par exemple au niveau de l'agression et de l'hostilité. Nous savons maintenant que cela n'est pas le cas, ce qui nous permet de mieux préciser le champ d'application de cette tâche.

De surcroît, nous ne savions également pas si cette tâche avait des effets directs (donc pas en interaction avec d'autres variables) et si une menace préalable au concept de soi est nécessaire. Nous savons maintenant que cette tâche n'a pas d'effets directs (du moins sur l'agression, l'hostilité, et l'affect) et qu'une menace préalable au concept de soi soit nécessaire. En résumé, en identifiant les conditions limites (« boundary conditions ») de la tâche d'activation de la

présence attentive, nous permettons au domaine de recherche de mieux préciser ses effets et d'informer les recherches futures quant aux contextes dans lesquels cette tâche peut faire avancer la théorie.

5.2.2 Intervention de réflexion sur l'amour-bienveillant

Cette thèse ajoute à la littérature en montrant les effets uniques d'une intervention adaptée aux personnes non-méditantes et à une population relativement hautement éduquée (plus cognitivement orientée), à savoir que la lecture de livres et l'écoute de balados sur la bienveillance peuvent aider à réduire l'agressivité comportementale, les attitudes sociales explicites et à augmenter la compassion, l'accessibilité des souvenirs altruistes et la volonté d'aider.

Peu d'études sur la méditation ont utilisé des interventions de discussion ou de réflexion comme groupe témoin actif. L'étude de l'article 2 a elle-même été inspirée par une série d'études qui comparaient des interventions de méditation et de discussion sur l'amour-bienveillant (Kang et al., 2014; Kang et al., 2015). La seule mesure commune (et donc comparable) entre les études de Kang et la présente étude est celle des attitudes explicites et implicites, bien que même ces mesures diffèrent quelque peu. Le fait que l'intervention de réflexion ait affecté à la fois des mesures explicites et implicites contraste avec les études de Kang, qui ont démontré qu'une intervention de discussion sur l'amour-bienveillant (dont notre intervention de réflexion est inspirée) ne diminuait pas les préjugés explicites et implicites, ou les attitudes positives explicites envers les autres, par rapport à un groupe témoin.

Nous remarquons que le groupe de réflexion de la présente étude diffère à plusieurs égards de ces groupes de discussion précédents, ce qui pourrait expliquer en partie les différences de résultats. Tout d'abord, les textes de réflexion des études de Kang consistaient en de très courtes notes de synthèse en préparation de la première discussion de groupe (semaine 1), d'un court poème ou d'une histoire bouddhiste (semaines 2, 4 et 6), chaque texte prenant peut-être 30 secondes à deux minutes à lire, ou de courtes présentations vidéo par des personnalités bouddhistes (semaines 3 et 5). Au cours de leur 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.

Par conséquent, dans la présente étude, nous avons tenté d'égaliser les interventions de discussion et de méditation en termes de temps, d'effort et d'activité en incluant des devoirs consistant en des lectures et des balados sur le thème de la bienveillance. En revanche, les personnes participantes à la présente étude ont dû lire de nouveaux extraits de livres beaucoup plus longs (l'équivalent d'environ 10 minutes par jour) sur le thème de la bienveillance tous les *jours*, au lieu d'une fois par semaine. La quantité d'engagement et d'effort cognitif exigée des personnes participantes doit donc avoir été beaucoup plus importante dans la présente étude.

Deuxièmement, l'intervention de discussion dans les études de Kang comprenait des cours de discussion de groupe hebdomadaires, ce que notre groupe de réflexion n'avait pas. Au lieu de cela, notre groupe de réflexion a écouté des balados sur les lectures de la semaine, ce qui a peut-être permis une meilleure intégration conceptuelle des leçons des extraits du livre. Enfin, les études de Kang ont toutes été menées en personne, alors que notre étude était entièrement en ligne. Cette différence entre la transmission de l'intervention en ligne, versus en personne et en groupe, est importante.

En effet, dans le cas d'interventions basées sur la présence attentive, par exemple, certaines études suggèrent que les facteurs les plus importants dans la prédiction des bénéfices ne sont pas la pratique méditative en soi, mais plutôt les facteurs sociaux tels que les relations avec les instructeurs et les autres membres du groupe de pratique (Canby et al., 2021). Selon ces chercheurs, ces facteurs sociaux permettent notamment de créer des liens, exprimer des sentiments et susciter un sentiment d'espoir. Dans notre étude, les facteurs sociaux ont été minimisés, en évitant des interactions directes avec l'instructeur ou d'autres personnes participantes, pour mieux isoler les effets de la pratique méditative en tant que telle. Ceci nous a permis d'évaluer les effets directs de l'intervention sans confusion d'avec les facteurs sociaux habituellement présents dans ce type d'étude.

5.2.3 Modèle duel du contrôle de soi et de l'impulsivité

Cette thèse contribue également à la littérature sur l'épuisement de l'égo et la fatigue cognitive (Hofmann et al., 2009), en montrant que des interventions spécifiques d'entraînement mental, comme la méditation de bienveillance, peuvent changer la relation entre les attitudes implicites (mais pas les attitudes explicites) et les résultats explicites (auto-déclarés) et implicites (temps de réaction). Il s'agit de la première étude à montrer que cette relation peut être modifiée grâce à un entraînement mental. Il ajoute également à la littérature en montrant que chez les personnes participantes d'un groupe témoin, les effets de l'agression implicite ne sont pas toujours prévisibles ou intuitifs pour les tâches auto-rapportées (amour-compassionné) et basées sur le temps de réaction (accessibilité des souvenirs altruistes).

La thèse ajoute à la fois à la littérature sur l'épuisement de l'égo et à la littérature sur l'amour-bienveillant en montrant expérimentalement que les personnes méditantes sur l'amour-bienveillant peuvent surcompenser leur agressivité implicite lorsqu'ils contrôlent pleinement leurs ressources d'autorégulation, mais que cette surcompensation se dissipe une fois qu'ils sont épuisés cognitivement, ce qui suggère que ce changement résulte d'un effort cognitif et qu'il n'est pas automatisé (Hofmann et al., 2009). Peut-être que ce schéma de disparition de la surcompensation lors de l'épuisement disparaîtrait, par exemple, chez les personnes méditantes à long terme (> 6 semaines) où l'exposition chronique à la bienveillance aurait eu plus de temps pour complètement automatiser ce processus et, de sorte que les individus n'auraient plus à compter sur leur contrôle de soi et leurs ressources cognitives.

5.3 Limites et recherches futures

Ces recherches comportent de nombreuses limites. Le format des présentes études limite le niveau de certitude avec lequel nous pouvons répondre à certaines questions. Par rapport à la question relative à la durée d'intervention, par exemple, nous avons comparé les résultats des trois études de l'article 1 (brève exposition) et de l'essai randomisé contrôlée de l'étude 2 (longue exposition). Cependant, nous n'avons pas systématiquement varié la durée d'intervention au sein d'une même étude. Ainsi, d'autres différences au niveau des devis des

différentes études limitent les conclusions que l'on peut porter sur l'influence de la durée d'intervention.

Des limites spéciales s'appliquent à la question du contexte et plus spécifiquement de la manipulation expérimentale de fatigue cognitive (« ego depletion »), puisque le concept est contesté (Dang et al., 2020; Friese et al., 2018; Hagger et al., 2016; Lurquin et al., 2016; Vohs et al., 2021) et que des études pilotes non-publiées ont révélé des difficultés à développer une tâche qui crée une fatigue cognitive réelle et de manière fiable, même en utilisant des tâches précédemment validées. Il est ainsi possible que les effets limités de notre tâche de fatigue cognitive soit dû au fait qu'elle n'était pas particulièrement efficace chez nos personnes participantes. De futures études s'intéressant à la question de l'effet de la fatigue cognitive devraient s'entendre sur la meilleure tâche à utiliser, en déterminant idéalement la référence par excellence (« gold standard ») du domaine.

L'ensemble des quatre études (des articles 1 et 2) ont été conduites en ligne plutôt qu'en laboratoire, avec toutes les limitations qui en découlent, comme le plus faible niveau de contrôle de l'attention lors des tâches et questionnaires ainsi que lors des interventions ou manipulations expérimentales. De plus, pour l'article 1, l'étude 1 n'était pas préenregistrée et exploratoire, et les trois études ont été conduites via une plateforme de recrutement en ligne, avec les limitations additionnelles associées (Aruguete et al., 2019; Chmielewski & Kucker, 2020; Deetlefs et al., 2015; Zhou & Fishbach, 2016).

De futures recherches auraient intérêt à tester directement la question du rôle du dosage par exemple soit en manipulant expérimentalement le niveau d'exposition au traitement (de méditation ou de lecture). Alternativement, il pourrait également être possible d'utiliser des méthodes statistiques avancées telles que le CACE (Complier Average Causal Effect), également connu comme LATE (Local Average Treatment Effect), pour tester l'influence de l'adhérence au traitement, puisque présumément, les personnes participantes qui méditent ou réfléchissent plus à l'amour-bienveillant devraient voir des effets plus importants (Sagarin et al., 2014). Des analyses de survie seraient également pertinentes pour évaluer comment l'attrition différentielle évolue au fil du temps et en fonction du groupe expérimental.

Alors que nous expliquons en partie les différences entre les groupes de méditation et de réflexion par le niveau de difficulté des pratiques et de leur popularité chez une population hautement éduquée, il serait pertinent lors de futures études de tester cette hypothèse en incluant des questions portant directement sur le niveau de difficulté perçu des activités, immédiatement après chaque activité. D'une part, ceci permettrait de calculer un niveau de difficulté moyen par activité / texte et d'adapter les activités ou les textes en conséquence. D'autre part, ceci permettrait également d'établir un niveau de difficulté moyen par intervention, pour voir si les deux groupes sont comparables à ce niveau, et si les différences de résultats peuvent effectivement être expliquées par le niveau de difficulté.

Une autre limite à mentionner concerne en particulier le groupe de réflexion. En effet, une assomption ici est que les effets observés dans le groupe de réflexion sont dus au processus de *réflexion* associés aux lectures. En particulier, cette assomption est justifiée par le texte de réflexion que les personnes participantes de ce groupe devaient écrire après chaque lecture. Cependant, il est possible que ces personnes participantes aient écouté les balados et complété les lectures et exercices d'écriture plutôt passivement, sans réflexion particulière ou subséquente après les exercices en soi. Il est ainsi possible que l'intervention fonctionne même lorsque les personnes participantes ne s'engagent pas dans une réflexion spécifique ou continue après les activités. De futures études pourraient mesurer le niveau de réflexion rapporté par les personnes participantes quotidiennement pour vérifier que c'est bel et bien le mécanisme d'action de l'intervention, ou du moins tester le pourcentage de variance que ce facteur pourrait expliquer.

Pour plus directement évaluer les mécanismes de changement au fil du temps résultant des interventions, il serait pertinent pour les futures études d'inclure des mesures quotidiennes d'évaluation écologique momentanée (expérience d'échantillonnage), qui permettent de tester le rôle modérateur du changement dans la pente de régression de mécanismes d'action précis, tel que les niveaux d'acceptation, d'équanimité, de présence, ou d'affect positif (Linz et al., 2022; Shiffman et al., 2008).

6. CONCLUSION

L'objectif de cette thèse était de clarifier les circonstances dans lesquelles les interventions de présence attentive et d'amour-bienveillant ont le plus grand impact sur l'agression et la prosocialité. Nous voulions globalement savoir comment l'effet de ces interventions varie en fonction de la *durée d'intervention*, de *l'individu*, du *contexte* et de la *méthode de pratique*. Considérés conjointement, nos résultats suggèrent que la durée d'intervention est importante, puisque l'on trouve des effets via une longue durée (six semaines de pratique quotidienne), mais pas lors d'une brève durée contextuelle (amorçage). Nos résultats suggèrent aussi que les traits de personnalité n'ont pas d'impact sur l'effet d'une brève exposition contextuelle (amorçage) de la présence attentive sur l'agression.

Cette absence d'effet pourrait être dû à la petite durée d'intervention, puisque lors d'une grande durée d'intervention de méditation, on trouve que les attitudes implicites prédisent différemment la prosocialité, pour certaines variables, en fonction du niveau de fatigue cognitive. Ce résultat au niveau de la fatigue cognitive met simultanément en évidence l'importance du contexte dans l'influence des interventions d'amour-bienveillant sur la prosocialité. Enfin, nos résultats suggèrent également que la méthode de pratique est importante, puisqu'une intervention d'amour-bienveillant sous forme de lecture d'extraits de livres populaires et de balados semble plus prometteuse pour une population novice qu'une intervention classique basée sur la méditation. Somme toute, il faut souligner que ces résultats, dans l'ensemble, peuvent ne pas se généraliser et ne pas être répliqués dus aux limites de la présente méthode.

Ultimement, cette thèse contribue de manière significative à la littérature sur la présence attentive et l'amour-bienveillant en apportant des réponses à de nombreuses questions, en ouvrant la porte à de nombreuses nouvelles questions, et en contribuant une toute nouvelle intervention prometteuse (réflexion sur l'amour-bienveillant) que des recherches futures pourront continuer à investiguer.

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