Meditation Inhibits Aggressive Responses to Provocations

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Abstract

Meditation has been advocated as a mental practice designed to reduce suffering and increase virtuous behavior. Although it has been previously linked to altruistic acts, its ability to reduce aggression and related retributive behaviors remains open to question. Here we report on an experiment in which participants were randomly assigned to a mindfulness meditation or active control condition three-weeks prior to facing a real-time provocation known to evoke aggression. Participants' capacities for executive control were also assessed subsequent to training. Results showed that three-weeks of daily meditation practice substantially reduced aggressive behavior even in the absence of any enhanced executive control capabilities. These results suggest that meditation attenuates aggression through direct reductions in motives to cause harm to others.

From its origins, a primary goal of Buddhist meditation has been an increase in virtuous behaviors meant to counteract human suffering (Gethin, 1998; Lutz, Brefczynski-Lewis, Johnstone, & Davidson, 2008). In accord with this view, the ability of meditation to foster prosocial behavior has recently become a topic of scientific focus, with early results showing promise (Ashar et al., 2016; Condon & DeSteno, in press; DeSteno, 2015). For example, techniques related to compassion meditation have been shown to engender prosocial economic decisions (Ashar et al., 2016; Leiberg, Klimecki, & Singer, 2011; Weng et al., 2013). Similarly, the practice of mindfulness meditation has been found to increase compassionate acts meant to relieve the suffering of strangers (Condon, Desbordes, Miller, & DeSteno, 2013; Lim, Condon, & DeSteno, 2015).

While such findings demonstrate meditation's effects on altruistic behaviors toward neutral others, the question of whether it would attenuate retributive aggression toward targets who evoke anger remains open. Although Buddhist scholars have traditionally advocated meditation as a method with which to foster a state of equanimity, and thereby to prevent aggression directed at those who would normally provoke it (Gethin, 1998), little evidence exists to support this notion (Fix & Fix, 2013). Nonetheless, given meditation's demonstrated ability to motivate behavior meant to reduce suffering, a scientific basis for postulating an ability to attenuate aggression exists.

To investigate this possibility, the present experiment utilized a design similar to that used in our previous work on meditation and prosocial behavior to examine whether contemplative practice can reduce aggression (cf. Condon et al., 2013; Lim et al., 2015). In brief, it compares the behaviors of new meditators to non-meditators in response to an aggressive provocation, with

meditation training having been guided by monitored home use of a mindfulness app that has been shown to produce compassion-relevant behavioral results similar to that deriving from studying with an ordained lama over a brief period (cf. Lim et al., 2015). Additionally, since prolonged meditation practice has been associated with mild increases in executive control (Teper & Inzlicht, 2013), we also examined whether the brief training protocols used here might produce a similar result. While we did not expect such brief practice to produce substantive changes in executive control, examination of this possibility is warranted given that an increased capacity for control has been shown to decrease aggressive behavior (Finkel, 2014; Finkel et al., 2012).

Method

Participants

We were able to recruit seventy-seven native-English speakers, none of whom had prior meditation experience, to take part in a three-week study billed as examining the effects of mind-training on cognitive and perceptual skills. Participants ranged in age form 18-24 and as compensation, received course credit and were entered into a lottery for a chance to win one of five $100 gift vouchers. Twenty-four of the original 77 participants did not complete the required number of sessions and were subsequently removed from analysis (see description of inclusion criteria below). Participant attrition was relatively equal across the meditation and control groups ($f_{\text{meditation}} = 13, f_{\text{control}} = 11, \chi^2\ (1) = .04, p = .85$). Three additional participants were removed for indicating suspicion (all participants were assessed for suspicion at the end of the experiment using a series of funneled questions) and one for failing to follow directions during the lab-based session (see below). Three more were excluded based on aggression scores that were identified

as extreme outliers using Tukey's box-and-whisker criterion of scores beyond the outer fence. Note that such extreme outliers are typical when using the hot sauce paradigm to assess aggression, as a few individuals do not read the directions thoroughly and invariably fill the sample cups. The final sample consisted of 46 participants.

Because the level of attrition was slightly greater than the normal rate of 20% for meditation studies involving monitored daily practice, we conducted a power analysis to ensure that the resulting sample size maintained adequate sensitivity to find the predicted effect (i.e., power ≥ .70). The anticipated effect size of Cohen's $d = 0.75$ was calculated as the mean found from four previously published experiments meant to examine the effect of meditation on prosocial behavior using a similar design and behavior-based dependent variable to that used here (cf. Ashar et al., 2016; Condon & DeSteno, 2013; Lim et al., 2015; Weng et al., 2013). For a comparison involving means from two independent groups with an alpha = .05, 46 participants are needed to achieve a prospective power of .70.

Procedure

Participants were randomly assigned to one of two conditions: (1) a three-week online mindfulness-based meditation program that was self-administered via the Headspace app, or (2) an active control group which involved the completion of logic problems on a daily basis for three weeks. In this way, the control group was active in nature; like the meditation condition, it required daily engagement in a cognitive task. The entire study unfolded in three phases: briefing, training period, and lab-based session. All measures, manipulations, and exclusion criteria utilized are disclosed below.

Briefing. Participants arrived for an introductory session during which they provided informed consent and received instruction on how to use the website and app that contained the

content of the respective training programs. They were also informed of the criteria for successful study completion.

Training Period. Participants in both conditions were instructed to complete 21 training sessions over a three-week period. They were told that the experimenters were interested in examining how doing certain types of mental exercises (i.e., meditation or logic problems) might affect cognitive abilities. The Headspace training program included mindfulness-based exercises such as focusing on a selected object (e.g. the body or the breath), monitoring the activity of the mind, noticing mind-wandering, and developing a non-judgmental orientation toward one's experience (i.e., equanimity). The training did not include any references to loving-kindness, compassion, or related terms. Of import, the training modules were designed by an individual with substantial monastic training and have been shown to enhance altruistic behaviors similar to those deriving from training for a short period of time with a Buddhist Lama (cf. Lim et al., 2015). Participants in the control group were instructed to complete a single logic problem for each day which was hosted on an online survey website. These problems consisted of word problems, geometric puzzles, analogies, etc. None were exceedingly difficult to solve. As such, they would not induce any frustration, but rather simply require some thought and attention to identify an answer. This use of an active control group was important, as it ensured an equal level of motivation and dedication in both groups.

Both types of sessions took approximately 15 minutes, and participants were instructed to complete no more than one session per day. To ensure compliance, participants in the meditation condition were required to complete a follow-up quiz which tested their comprehension of the day's session (e.g. what was the main topic covered today). An equivalent quiz was constructed for the control group which contained filler questions that probed for user experience (e.g. how

challenging was today's problem). Throughout the training period, research staff had access to participants' Headspace accounts or survey responses to monitor daily progress. Reminder emails were sent to the any participants who had missed two sessions during the training period. Participants had to complete a minimum of 17 of the 21 headspace or logic problem sessions, depending on their group assignment, to remain in the study.

Lab-based session. After each participant completed the training program, he or she was scheduled to come to the lab. Although participants believed they were coming to have their cognitive abilities measured, the primary goal was to assess differences in aggressive responses to a provocation as well as any differences in executive control. We first had participants complete a Stroop task meant to assess executive control. Next, under the guise of a speech and language task, we exposed participants to an aggression provocation procedure, following which we measured their feelings of anger (in addition to other emotional distractors). Finally, we gave participants the opportunity to retaliate via an act of aggression towards the provocateur. This opportunity took the form of a "taste perception task" in which participants had the chance to administer a sample of hot sauce to the provocateur for oral consumption with full knowledge that it would cause pain (cf. Liebermann, Solomon, Greenberg, & McGregor, 1999, cf. Condon & DeSteno, 2011; DeSteno, Valdesolo, & Bartlett, 2006). Once the hot sauce sample had been prepared, the experiment ended and participants were debriefed.

Measures

Stroop task. Participants completed a Stroop task modeled on that used by Teper & Inzlicht (2013). They were presented with a series of color words, with each word being depicted in either a congruent or incongruent color (e.g., an incongruent color might be the word "blue" presented with a red font). Using a keyboard with 1ms accuracy, participants were asked to

identify the color in which the word was printed. Participants completed 10 blocks, each consisting of 32 congruent and 16 incongruent trials that were presented in a random order. A trial consisted of a fixation cross, presented for 500ms, followed by the stimulus word, presented for 200ms. The inter-trial interval was 1000ms. We calculated two measures for subsequent analyses. The first was the total number of errors made by each participant; the second was a Stroop incongruency effect in which reaction times on congruent trials were subtracted from those on incongruent trials, using correct trials only.

Provocation. After the completion of the Stroop task, participants were told that they would engage in a speech and language task using a video conference platform to engage with another participant located in a separate lab space. This task was adapted from one developed by Denson and colleagues that has been shown to be effective in evoking anger, aggression, and related physiological changes (Denson, Capper, Oaten, Friese, & Schofield 2011; Memedovic, Grisham, Denson, & Moulds, 2010).

Before the actual task began, participants were given 10 minutes to prepare a two-minute speech on their life goals and future plans. When they were ready, the experimenter began the video conference. Unbeknownst to participants, the other person in the video conference had been pre-recorded with the use of actors who played the role of the provocateur. The actor was always of the same gender as a given participant. The pre-recorded video was scripted to give the illusion that the interaction was genuine and that participants were actually giving a speech to another participant who was actively listening. The video was scripted in such a way that the actor was the first to give the speech while the participants listened for 2 minutes. After the actor finished, participants would give their speech for 2 minutes. After the video conference ended, the computer informed participants that the two would exchange feedback on their speeches. In

order to provoke anger, the written feedback participants received from the actor was the following: “Honestly, I wasn’t impressed. Your speech was boring and you sounded like you haven’t given much thought at all to your future goals. What a waste of my time listening to you”.

Anger. Participants reported their affective state by indicating the degree to which they were currently experiencing different emotions. Responses were recorded using a five-point scale ranging from (1) not at all to 5 (very much). Anger was assessed as the mean response value for two items: angry and annoyed (Cronbach alpha = .80).

Measurement of aggression. To assess aggressive behavior, we used a measure developed and validated by Lieberman et al. (1999). Participants were told that they and the provocateur would engage in a taste perception task. They were also told that, in order to reduce any experimenter bias, one of them would be assigned at random to be the administrator of the taste task while the other would be the recipient. Each was also asked to indicate preferences for different types of flavors. In actuality, participants were always assigned to be the administrator, which required them to prepare a taste sample for the provocateur.

Once participants had been told that they would be the administrator, they were given a box of condiments with which to prepare the taste sample. The box contained three condiments representing different flavor conditions (i.e., hot sauce, chocolate syrup, and lemonade powder), as well as the provocateur’s taste preference measure which indicated a dislike of spicy food. Instructions in the box informed participants that they had been randomly chosen to prepare a spicy sample for the provocateur. It was also explicitly made known to them that once the sample had been prepared, it would be given to the other participant for consumption in its entirety. That is, whatever amount of the sample they placed in the cup would be placed directly

into the other participant's mouth. Using a condiment cup with a sealable lid, participants then prepared the sample. Aggression was measured as the amount of hot sauce in grams poured into the cup, as the substance was known to be aversive and somewhat painful in a dose-dependent manner.

Results

As expected, the anger provocation proved effective. Member of both the meditation and control groups demonstrated elevated levels of anger (i.e., anger that was greater than a value of 1 [feeling no anger at all]: $M_{\text{meditation}} = 2.81$, $SD_{\text{meditation}} = 1.19$; $M_{\text{control}} = 2.79$, $SD_{\text{control}} = 0.99$; *t's* > 7.70, *p's* < .001) that did not differ from each other, $t < 1$, *ns*.

Of greater import, we found support for our primary prediction that meditation would attenuate aggressive responses to the provocation. Meditators poured significantly less hot sauce into the sample cups to be given to their partners than did non-meditators, thereby indicating a desire to cause less pain, $t(44) = 2.81$, $p = .007$, $d = 0.84$ [95% CI $0.22 < d < 1.44$] (see Figure 1). This effect size, though large, is in accord with previously cited work showing mean effect size of $d = 0.75$. As such, it is unlikely to reflect an overestimate of the target effect resulting from sampling error. Nonetheless, because the minimum value of the dependent variable is bounded by zero, the distributions for each experimental condition possessed a mild positive skew which can result in increased variance as the mean value of each distribution increases. Subsequent comparison of the standard deviations of the amount of poured hot sauce using Levene's test for the equality of variance confirmed that this was indeed the case ($SD_{meditators} = 3.08$, $SD_{control} = 6.19$; $F = 8.64$, $p = .005$).

Although standard t-tests are somewhat robust against minor violations of the distributional assumptions of homogeneity of variance and normality, we nonetheless decided to re-analyze our data using Kruschke's Bayesian Estimation Supersedes the *t*-Test (BEST) model (Kruschke, 2013). Because the BEST model relaxes the distributional assumptions of homogeneity of variance and strict normality, it provides more accurate parameter estimation and comparisons than does the standard *t*-test model, especially for smaller samples. Using the BEST (burn-in and MCMC samples of 20,000) confirmed a credible decrease in aggression by the meditation group as compared to the control group (mean difference between groups = 3.91g, 95% High Density Interval 0.61g – 7.01g; effect size = 0.81, High Density Interval 0.10 – 1.53).

The question next turned to whether this decrease in aggression might stem from differences in executive control. Ruling out this possibility, meditators and non-meditators did not differ with respect to the number of errors made on the Stroop ($M_{\text{meditators}}$= 12.08, M_{control} = 10.06, $t < 1$) or to the incongruency effect in reaction times ($M_{\text{meditators}}$= 89.56ms, M_{control} = 69.20ms, $t < 1$). Note that due to a technical malfunction, data on the Stroop was not recorded for two participants. Given the clear absence of any difference in the number of errors or response times as a function of meditation condition, this loss could not be expected to alter any inferences

Discussion

Of import, this finding supports the view that relatively brief periods of meditation training can substantially reduce aggression without any concomitant change in executive control. We theorize that meditation exerts its influence primarily by enhancing a desire to avoid suffering. That is, we suspect it reduces aggression by attenuating what Finkel (2014) has termed *impellance* – the drive to aggress when an instigating event occurs – as opposed to inhibiting a

desire to cause harm, which Finkel (2014) has argued often requires enhanced executive control. This is especially the case since both meditators and nonmeditators reported experiencing a sense of anger, but the drive to aggress only appears to have occurred among nonmeditators. Indeed, this view is consistent with findings from an earlier experiment examining prosocial behavior in which it was found that meditation not only increased people's drive to remove the pain felt by others, but did so without any associated increase in higher order cognitive processes like perspective taking (Lim et al., 2015). Rather, it appears that meditation may simply foster a direct motivation for compassionate or prosocial acts.

It should be noted, however, that while we believe the current findings strongly suggest that meditation can reduce aggressive behavior, the aggression measure we utilized is relative in nature. Although it is certainly true that those who meditated acted less aggressively than did those who did not, it is theoretically possible that some aspect of the protocol used for the active control group might have increased aggression. Without including a passive control condition (i.e., one in which participants experienced a provocation without having taken part in any type of training), firm conclusions about baseline levels of aggression are not possible. However, we believe the possibility that some aspect of the active control condition increased aggression is unlikely. First, as noted above, completion of the logic problems was not particularly demanding and, among a population of college students, should not be perceived as taxing and thereby capable of causing chronic frustration. Moreover, at the time of the aggression measure, groups did not differ in the level of anger and annoyance they were feeling. Second, the level of aggression exhibited by meditators in the face of provocation was fairly similar to the average level we have found in control conditions (i.e., conditions without any anger-provoking event) in our previous work using the same paradigm (M = 2.05g, cf. Condon & DeSteno, 2011; DeSteno,

Valdesolo, & Bartlett, 2006). When considered with the fact that feelings of anger were similar across both groups in response to the provocation, the decreased aggression shown by meditators strongly suggests that they were avoiding hostile actions.

While future work focused on illuminating the specifics of the underlying mechanisms by which meditation reduces aggression is warranted, the potential for designing and evaluating meditation-based interventions to reduce violence should also be considered. The readily scalable nature of the smartphone-based training procedure used here suggests that similar methods amenable to rapid deployment and ease of use might successfully be employed in populations at risk for violence at a relatively low cost. Indeed, at least for brief periods of training, the magnitude of the increase in compassionate behavior deriving from mobile-app based instruction like that used here appears somewhat similar to that resulting from training face-to-face with an expert teacher (cf. Condon et al., 2013). As such, meditation might profitably be used as an intervention to reduce escalations in violence that typically stem from provocations.

Ethical Standards

Ethics approval for the conduct of this experiment was granted by the Northeastern University Institutional Review Board to ensure that procedures were in accord with the standards noted in the 1964 Declaration of Helsinki and its later amendments. To ensure that individuals taking part in the study were participating voluntarily and that they understood their rights as well as the presence of any potential risks, all participants were offered the opportunity to ask questions prior to signing a statement of informed consent and beginning the experiment.

Conflict of Interest Statement

The authors declare that they have no conflict of interest. Use of the Headspace app was made freely available by Headspace, Inc. for this research project. Headspace did not provide any other financial support and none of the authors have any ties, financial or otherwise, to Headspace.

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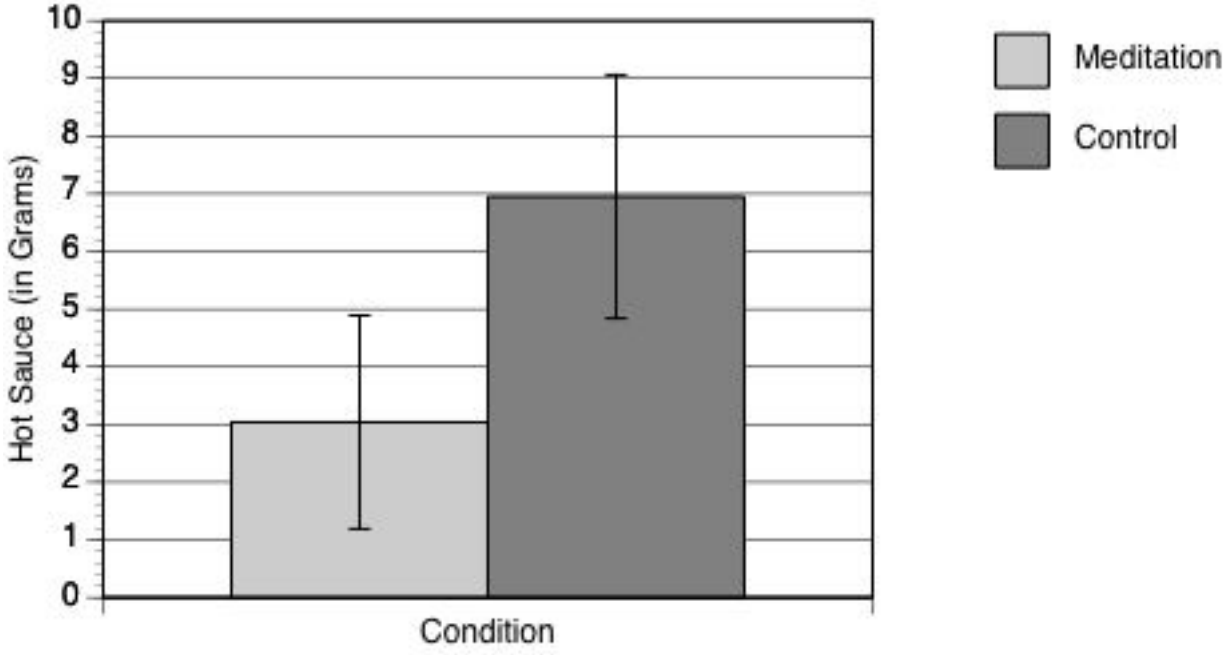
Author Contributions

DD, DL, FD, and PC designed the study. DL and FD executed the study. DD conducted the data analysis in collaboration with DL and FD. DD wrote the manuscript which was edited by DL, FD, and PC.

Figure Caption

1. Mean grams of hot sauce poured as a function of condition. Error bars indicate 95% confidence intervals.


10
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Hot Sauce (in Grams)
Condition
Meditation
Control