Does Mindfulness Meditation Facilitate Tacit Coordination? – An EEG Study on Working Memory and Theory of Mind

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PSB3E-BTHO: Bachelor Honours Thesis

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March 13, 2022

Abstract

Every day, our human ability of perspective-taking, also referred to as *mentalising*, is taxed to tacitly coordinate in interindividual contexts. Theory of Mind (ToM) was shown to strongly depend on Working Memory (WM) capacities as it involves the maintenance and manipulation of social information in one's mind to successfully model and predict a partner's beliefs, intentions, and actions. Previous research examined promising interventions, such as Mindfulness Meditation (MM), to improve ToM. As ambiguous conclusions emerged, the current study aims at elucidating the link between WM and ToM by investigating the effectiveness of an eight-week internet-based MM course in strengthening those functions. Participants performed a computerised tacit coordination experiment twice with their romantic partners. Whilst one partner underwent the MM intervention in the meantime, the control participant did not receive any training. EEG hyperscanning was employed to examine within-subjects and between-groups differences in P3b amplitudes since this ERP component was consistently reported to correlate with WM. Our main hypothesis that participants in the experimental group exhibit larger P3b amplitudes at the post-measurement was not supported. Yet, behavioural analyses suggest dyads to perform the task more accurately following the MM intervention. Notably, our small sample size ($n = 12$) and thus, little statistical power, should be regarded as the limiting bottleneck for our results' reliability.

Keywords: theory of mind, working memory, social cognition, tacit coordination, EEG, P3, P3b, mindfulness, meditation

Does Mindfulness Meditation Facilitate Tacit Coordination? – An EEG Study on Working Memory and Theory of Mind

Throughout history, the ability of maintaining and operating on social information was consistently within human nature and at the core of adaptive behaviour. As the ontogenetic *Vygotskian intelligence hypothesis* states, our innate need for communal cooperation shapes the development of intelligence substantially and is thereby a unique factor of human social-cognitive skills (Moll & Tomasello, 2007). This becomes further evident considering the vast number of mental activities we engage in daily: a majority of them are induced by interindividual interactions, such as conversing with others, playing games, or working in a team. Taking the example of playing tennis, successfully predicting moves of your opponent provides you with a competitive advantage. In comparison, winning a basketball match requires us to anticipate others' actions aiming at achieving a mutual rather than an individual goal. For such cooperative actions to be successful, actors must be able to reason about to the other party's intentions, beliefs, and future actions. This subsequently allows them to act accordingly by flexibly adjusting their responses strategies.

This pertinent capability of perspective-taking by theorising about other's internal states (also known as *mentalising*) is generally referred to as *Theory of Mind* (ToM) (Baron-Cohen et al., 1985; Bradford et al., 2015; Maehara & Saito, 2011). Executive functions play a key role in this (Aboulafia-Brakha et al., 2011; Perner & Lang, 1999). Whilst literature demonstrates uncertainties in the precise relationship between subcomponents of executive functions and ToM, several cognitive mechanisms were consistently reported. The most central one was proposed to be the maintenance and concurrent modification of task-relevant information in Working Memory (WM; Miyake et al., 2000). Multiple dual-task studies reported ToM application to depend on WM load (Bull et al., 2008; Qureshi & Monk, 2018). That WM capacity improves mentalising is further supported by research outlining a WM intervention which increased perspective-taking accuracy (Meyer & Lieberman, 2016). Additionally, common pervasive social conditions such as attention-deficit/hyperactivity or autism spectrum disorder were reported in patients affected by declined ToM capabilities. Those impairments were likely indirectly induced by frontal lobe impairments which highly correlate with WM skills (Kercood et al., 2014; Pineda-Alhucema et al., 2018; Smith & Jonides, 1999; Tatar & Cansız, 2020; Kofler et al., 2011).

Whilst WM is also crucial for countless tasks not involving social information, such as mental arithmetic or habitual commuting, its engagement within interactive contexts was suggested to be a even more highly advanced cognitive process (Ding et al., 2017). Especially with the modern world becoming ever more virtual and thus, multiplex, navigating in our environment requires stable WM's abilities to temporarily coordinate multiple pieces of task-relevant information. As the hazard of stimuli overload increases, simultaneously inhibiting distractions by task-irrelevant information is impeded and causes even simple tasks to become progressively demanding. These fundamental processes of selective maintenance and manipulation of sensory data and stored knowledge, which compose the term WM (Daniel, 2015), are integral to effectively plan and exhibit goal-directed behaviour in social contexts (Goldman-Rakic, 1992). For example, higher WM capacity is related to academic achievement, cognitive efficiency and emotional stability (Ackerman et al., 2005; Jha et al., 2019; Nieuwenhuis et al., 2005; St Clair-Thompson & Gathercole, 2006). Deficits are related to learning disabilities and developmental disorders in children (Holdnack et al., 2019; Martinussen et al., 2005).

Traditionally, ToM is examined with false-belief tasks (Wimmer & Perner, 1983). To investigate its relationship with social WM, however, coordination tasks characterised by outcome interdependence may be applied. Such experiments are often constructed in a game-like form. Participants "play" in groups of two aiming at making identical decisions by predicting their partner's choice strategy. If a situation does not allow for direct communication, forecasting the other's behaviour becomes more intricate and effortful (Alberti et al., 2011; Chartier & Abele, 2016; Colman, 1997; Schelling, 1960). As no single correct answer exists, made-up coordination rules must be inductively computed from the detection of salient clues and experiential learning across trials. For example, two people may play a game asking them to choose the same integer out of numerous arrays. After a few times, player *A* might notice that player *B* always chooses the lowest number within the list. Hence, the lowest number becomes the salient clue which player *A* will then similarly select in the coming round. This flexible updating of prior beliefs in shed of newly required evidence on a trial-by-trial basis allows for the construction of mutual rationales (De Kwaadsteniet et al., 2012).

Studying such ambiguous tacit task designs is seen as rather powerful for representing ToM application considering their congruency with real-life coordination scenarios, such as the classic *left turn problem*: In a country with right-hand traffic, two vehicles simultaneously approach a crossroad without priority rules. As they face each other, one vehicle intends to continue straight, yet the other signals driving to the left. Since their paths will cross, which vehicle goes first? As drivers cannot communicate explicitly, they must settle this problem by coordinating their actions implicitly (Alberti et al., 2011). Successful coordination depends then solely on both's ToM abilities.

Thus, researching means to enhance ToM through WM in both healthy and clinical populations is of utmost importance as well-operating memory systems can allow for everyday improved social efficiency next to previously mentioned benefits. Since WM involves the maintenance of goal-relevant information in our minds, it becomes evident that endogenous top-down attention exerted by the central executive is focal to cope with complex tasks given WM's capacity limitations (Baddeley, 2012; Baddeley & Hitch, 1974). In fact, several studies in the field of cognitive psychology demonstrated that attentional processes, specifically *selective attention* – the ability to focus on relevant information whilst ignoring irrelevant information –, are key to successful WM (Gazzaley & Nobre, 2012; Jha et al., 2019). Since selective attention was established to potentially be positively impacted by interventions comprised of meditation practices (Chan & Woollacott, 2007; Chiesa et al., 2011; Lutz et al., 2008), it is therefore unsurprising that a further line of research examining the influence of mindfulness meditation (MM) on WM got ignited.

Whereas the ancient art of MM emerged as a nascent but inspiring concept in psychological application and contemplative research, meditation techniques, such as Vipassana, have also become increasingly popular in everyday life within Western societies. Grounded in empirical evidence, proponents promise cognitive gains to achieve inner tranquillity, increased focus, and overall improved quality of life (Cahn et al., 2009; Chan & Woollacott, 2007; Hölzel et al., 2011). Thus, MM became a tool in education (Leland, 2015), clinical settings (Baer, 2003), and military training (Brewer, 2014). This hype is further reflected by a plethora of inconsistent definitions for the term *mindfulness* surfacing in the literature. Since our MM intervention promotes interoceptive awareness, this paper will refer to Bishop and colleagues (2004) describing mindfulness as an act of purposely guiding attention to elements of the present moment to reach a non-elaborative and accepting mental state by sustaining one's attention on one's breath or body.

Perks obtained through long-term meditation resulting in trait effects on an individual's brain and body are thought to be primarily achieved by improved attentional control and enhanced WM (Lutz et al., 2007). Significant behavioural between-group differences in studies examining MM-induced effects on executive functions were repeatedly, yet inconsistently, reported. For instance, a systematic review by Lao and colleagues (2016) found that participants undergoing an eight-week mindfulness training showed better performance on behavioural measures and neuropsychological tests compared to control groups in WM tasks, but not in distractor-tasks taxing selective attention. A further study proposed MM to possess salutary powers in that it preserves WM in face of stress-induced WM deterioration (Jha et al., 2019). In contrast, Yakobi and colleagues (2021) reported reversed findings: significant effect sizes for studies comparing behavioural performance of attentional control, such on the Digit-symbol-substitution-test, were reliably detected. Yet, test scores on WM tasks were suggested to remain unaffected by MM.

Studies indeed reporting MM-induced changes in WM showed also accompanying changes in brain activity such as persistent correlations between MM and parieto-occipital alpha and frontal-midline theta power in electroencephalogram (EEG) oscillations (Bailey et al., 2020). Whereas alpha synchronisation is supposed to mark internalised attention, theta power counts as an indicator of executive attentional control and is associated with WM (Bailey et al., 2020; Hunter et al., 2018; Lomas et al., 2015). Similarly, considering event-related potentials (ERPs), mindfulness interventions are often linked to increased P3 (or P300) amplitudes and latencies (Atchley et al., 2016; Hunter et al., 2018; Jo et al., 2016; Kakumanu et al., 2019; Lin et al., 2019). This component is thought to reflect several cognitive information processes including decision making, attention orienting, and WM abilities (Duncan-Johnson & Donchin, 1977; Nieuwenhuis et al., 2005; Picton, 1992) characterised by a large positivity approximately 200 to 500ms after stimulus onset (Nieuwenhuis et al., 2005; Steiner et al., 2013; Sutton et al., 1965). The strongest P3 is typically measured at the Pz electrode within the international 10-20 system. Yet, studies also include a cluster of electrodes, such as Pz, Fz, and Cz, all of which are located at the midline sagittal plane centroparietal on the scalp (Polich, 2007).

Notably, the P3 component can be divided into P3a and P3b, with distinct but overlapping scalp topographies and functions forming a circuit pathway from frontal to parietal and temporal sites. Whereas the P3a's frontal maximum is meant to emerge through attentional processing of sensory input 200 to 300 ms post-stimulus, the parietal P3b subcomponent peaks approximately 300 to 500 ms after stimulus onset yet prior to overt decision making. The literature depicts the P3b as a primary neuro-marker for WM as it requires those attentional mechanisms present in P3a to subsequently activate memory operations, including content maintenance and updating (Donchin & Coles, 1988; Polich, 2007, 2012; Luck, 2014).

Relating this to MM's effect on WM, improved WM performance predictively produces larger P3 amplitudes. The increased availability of attentional resources provoked by MM is thought to mediate this process (Hunter et al., 2018; Klee et al., 2020). Equivalently, impaired ToM has been linked to reduced P3b amplitudes disclosing the P3 component as pivotal to mentalising (Libsack et al., 2021; Lincoln et al., 1993). The P3 is also robustly associated with activity within the temporo-parietal junction - an area highly involved in ToM tasks (Meinhardt et al., 2011; Sommer et al., 2007). That this region is part of the ventral attentional system further outlines P3's importance in WM updating (Corbetta et al., 2008).

Whereas literature indeed supports this reasoning by reporting statistically significant differences in P3 amplitudes modulated by meditation experience (Atchley et al., 2016; Lomas et al., 2015; Yakobi et al., 2021), other studies did not find similar effects (Atchley et al., 2016; Bailey et al., 2020; Cahn & Polich, 2009; Hunter et al., 2018). Thus, the influence of MM on P3 remains ambiguous. Since MM was identified as one of the most feasible and effective interventions for attentional enhancements (Tang et al., 2012), the relevance of clarifying the relationship between neurophysiological underpinnings of WM and ToM such as the P3 with MM remains evident.

The current study aims at elucidating this by having participants in dyads perform a tacit coordination task whilst recording their brain waves simultaneously through an EEG-hyperscanning setup. This imaging technique is commonly applied to identify shared dynamics of neural patterns during interactive behaviours (Balconi & Vanutelli, 2017; Dumas et al., 2011; Montague et al., 2002; Mu et al., 2018). The present paper focuses on ERPs, specifically the P3b component, due to its relevance for WM updating focal to altercentric perspective-taking. Each participant's team consists of a generally healthy MM-naïve romantic couple. Whilst one partner underwent an eight-week internet-based mindfulness course the other served as a control participant without receiving any training. The experimental task was performed twice - before and after the MM course - to test longitudinal pre- and post-differences in ERPs.

With this design, the paper’s main objective is to assess WM’s role in tacit coordination scenarios and thus, ToM. Secondly, it is examined whether this relationship can be modulated by mindfulness practice. This project additionally offers possibilities for replicating previous findings related to indexing P3b as a measure of WM. Utilising a tacit coordination task touching ToM as the social component of WM rather than applying more conventional WM assessments similarly aims at expanding the literature and transferring previously found MM-related effects on WM to different contexts.

Therefore, our primary hypothesis is that the experimental group will show significantly higher P3-evoked amplitudes following the MM intervention. Even though methodological differences between the present study and earlier research linking ToM and P3b demand us to adopt a critical attitude when identifying cognitive resemblances and thus, neural processes, our prediction is based on aforementioned findings suggesting that the P3b maximum indexes WM capacity (Libsack et al., 2021; Polich, 2007). Higher P3b peaks are thought to represent greater WM resources through increased attentional employment facilitating information processing (Lomas et al., 2015). Within our joint coordination task, P3b increases could potentially reflect strengthened ToM skills, including improved abilities to dynamically evaluate and operate on mental representations of both one's partner's perspectives and more adept handling of social information. Thus, mindfulness training is supposed to increase WM capacity, likely mediated by improved attentional control, which would then allow for more successful perspective-taking.

As an auxiliary hypothesis, partners without any intervention in-between time points are similarly expected to exhibit higher P3b-evoked potentials at the post-measurement, yet less pronounced than the experimental group. This prediction was grounded in fundamental premises composed by Piaget, Vygotsky, and Bandura emphasising the importance of social interactions for advanced cognitive development and learning (Tudge & Winterhoff, 1993). Thereby, the coordinative nature of joint actions requiring mentalizing is meant to potentially enable carry-over effects of greater intellect from one peer to the other (Devaine et al., 2014; Tudge & Rogoff, 1999). Hence, cognitive advantages that one partner gained through mindfulness practice could be expected to transfer to their partner considering their close bond and regular collaborative activities. Literature, however, is too scarce to deduce confident conclusions about whether consistent daily interaction is sufficient for socio-cognitive WM improvements to extend to romantic partners or whether deeper interactive engagement would be required (Tudge & Rogoff, 1999; Wiley & Jarosz, 2012). Thus, our analysis is rather exploratory.

Methods

Participants

Data stemmed from participants enrolled in a project of University of Groningen researchers dr. May and dr. van Vugt investigating interpersonal mechanisms induced by online mindfulness practice compared to general well-being improvements. Participants were recruited as a convenience sample and received compensation from 25 to 30 €. Advertisements were distributed in university buildings and on social media. Several eligibility criteria applied: subjects had to participate with their romantic partner, be under the age of 50, be proficient in English, be sufficiently available to complete study, have not practised mindfulness regularly in the past year, and have had no mental health history of, for instance, psychosis.

Since the current paper is concerned with secondary effects of the registered project, only dyads in the experimental MM group were considered (18 dyads in total). Data of active controls assigned to a happiness course has been omitted (12 dyads in total). Out of the relevant participants, eleven couples withdrew prematurely, and one couple's data was unusable due to EEG sampling errors. Thus, the final sample consisted of six dyads (M_{age} = 24 years; $\text{range}_{\text{age}}$ = 19 - 29 years; 50% females). Sixty-seven percent of couples lived together and relationships lasted from four months to 2.5 years (M_{duration} = 15 months). All participants provided written informed consent upon enrolment, though subjects were blind to the exact objectives to reduce possibly confounding effects on internal validity induced by demand characteristics. The study was approved by the CETO ethics committee.

Mindfulness Intervention

Of each romantic couple, one member completed the eight-week MM programme. Dyads themselves determined who participated in the internet-based intervention. Its objective was to introduce subjects to conceptual and practical components of breath- and body-focussed MM. The programme involved engaging sets of videos within the Coursera course '*Foundations of Mindfulness'* and three one-hour interactive live discussions guided by well-trained instructor Rhoda Schuling. The videos had a summed duration of less than one hour per week. Participants were directed to practise meditation for up to 20 min and complete diary entries daily. Notably, those self-reports were excluded from the scope of this paper. To ensure homogenous experiences across participants, a document with detailed instructions was provided (see **Appendix A**). The partner not enrolled in the mindfulness programme had the opportunity of complementing the course following the experimental phase. Total participation time, including EEG assessments, was approximately 10 weeks.

Tacit Coordination Task

Stimuli and Materials

Participants conducted the tacit coordination task in a lab on two separate monitors of identical resolution connected to one computer. The computerised task was programmed with OpenSesame (version 3.3.8; Mathôt et al., 2012). All stimuli were displayed on a white screen. Texts such as task instructions were printed in American-English language in black font. The experiment's main stimuli were abstract images of two types. Each image was one of three alternating *colours* or *shapes*. The colour stimuli were adapted from Alberti and colleagues (2012). Specifically, each image was characterised by two fixed and one varying feature within each trial. Each of the ten unique stimulus type combinations was presented in 36 images in total across all trials. No image appeared repeatedly. See **Figure 1** for example image arrays.

Task design

The task entailed a mixed design. Stimulus type of presented images (*shape* vs *colour*), time (*pre* vs *post*) and *trial number* were treated as independent within-subjects variables. Allocation to the MM course or the control condition was a between-subjects factor. EEG signals (in µV) and accuracy (Boolean value) were dependent variables.

Procedure

Each dyad performed the coordination task simultaneously and brain waves were measured via EEG-hyperscanning. Participants were instructed to move as little as possible to avoid motion artefacts. Both participants sat in the same room yet were unable to communicate verbally or exchange information visually. Instead, dyads could apply task-related feedback to develop consistent decision patterns. Throughout the experiment, participants were asked to only utilise their right hand for keyboard responses.

The task began with a welcoming message and task instructions. Each trial started with a fixation dot for 100ms. After it disappeared, participants were shown four abstract images. The goal then was to select the same image as their partner. To do so, 1500ms later, response labels were added to each image and participants were asked to subsequently indicate their first-best and second-best guess about which image their partner chose by pressing keys according to response labels *(‘z’, ‘x’, ‘c’, ‘v’* for player 1 and *‘1’, ‘2’, ‘3’, ‘4’* for player 2 referring to image *1, 2, 3* and *4*, respectively). After both participants finished, the first choices of both players were shown on the screens serving as feedback for 4000ms. This allowed participants to figure out their partner’s decision strategy. For instance, participants might aim at always choosing the image characterised by the highest contrast, the warmest colours, or the smallest shapes. To ensure that choices were based on image properties rather than other information such as spatial locations, image positions were randomised for each participant. Hence, rules such as selecting the left-most image were counterproductive. Afterwards, a new trial with novel images started. For a summary of the sequences within a single trial, see **Figure 2**. Participants firstly completed a practice round of four trials to familiarise themselves with the set-up. Afterwards, the data collection started consisting of two blocks á 45 trials (90 trials overall). Between blocks, participants could rest until they were ready to continue. They began the next block by pressing either the space bar or enter key. After the last block, a short message announced the end of the experiment. The task lasted approximately one hour.

EEG Recordings

EEG was collected by utilising two 32-channel head cap systems (BioSemi, Amsterdam, Netherlands) with a sampling rate of 512 Hz. These two systems were connected using daisychain technology for concurrent recordings (see Figure 8 in Barraza et al., 2019). The 'master' AD-box received data from the 'slave' AD-box through a fibre optic cable. From there, data was forwarded to the USB receiver which then redirected all data and trigger information to the acquisition computer. The electrode placement was done in the international 10-20 system whilst impedances were kept under 30 kΩ. To detect eye movements, four external electrooculogram (EOG) electrodes (two *vertical* below and above the left eye and two *horizontal* adjacent to the lateral canthi of both eyes) were applied next to two additional electrodes serving as reference, each of which placed on each mastoid.

Statistical Analysis

EEG Pre-Processing

To facilitate ERP analyses by enhancing data's signal-to-noise ratio, several offline pre-processing steps were taken. Fieldtrip open-source software (Oostenveld et al., 2011) in MATLAB (version 2018b, MathWorks) was used. For each participant individually, data was firstly re-referenced to the average of the mastoid electrodes. To eliminate slow drifts and high-frequency noise induced by muscle movement, data was filtered with a band-pass ranging from 0.1 to 50 Hz. Similarly, to avoid edge artefacts, data was filtered with 60 seconds of mirror-padding. The data was segmented into meaningful epochs spanning from 0 to 1500ms: 0ms represents the onset of the image array and 1500ms the time when participants could start executing their responses. Detrending removed linear trends. The average of 1 second before stimulus onset served as a baseline for each epoch from each channel. Afterwards, epochs with apparent non-stereotyped artefacts over multiple channels inducing noise such as eye blinks, other muscle activity or electrical noise were rejected based on visual inspection. Trial-specific interpolations of individual channels were performed for epochs with non-stereotyped artefacts which occurred only over one or few channels. If channels included artefacts over several epochs within whole sessions, these ‘bad channels’ were interpolated across all epochs. Lastly, further noise including eye movements was rejected via independent component analysis (ICA).

EEG Analysis

Pre-processed EEG data was analysed using Fieldtrip-toolbox within MATLAB to examine ERPs (version 2018b, MathWorks, Oostenveld). Two out of eight dyads were excluded from analysis due to missing data. As described previously, this analysis will focus on P3’s parietal P3b subcomponent as it was found to be especially sensitive to task-relevant information processing in WM (Polich, 2007). For simplicity reasons, the *P3b* will subsequently be referred to as *P3*. Overall ERP grand averages of all subjects across all conditions per channel were visually inspected to determine an unbiased time window and suitable electrodes. Our analyses focus mainly on investigating P3 amplitudes at Pz located in parietal regions as it is meant to display the strongest effects for WM updating (Polich, 2007). As expected, the Pz waveform plot compared to all other electrodes showed the highest P3 peak and framed a time window from 300 to 550ms after stimulus onset which aligns with the typical window associated with P3 peaks for WM and mentalising (Donchin & Coles, 1988). Within this period, participants were expected to review previously acquired mental representations of their partner's decision strategy and integrate this with currently presented stimuli. After the window was determined, average ERP amplitudes of each trial per participant were computed within the specified channel and period. Since the combination of the midline scalp sites Fz, Cz, and Pz is commonly applied for P3 distributions in the literature (Dolu et al., 2005; Polich, 2007, 1995), this electrode combination was processed similar to the isolated Pz waveform. Resulting data was used for statistical analysis.

Behavioural and ERP Analysis

Statistical analysis was conducted in R (Version 1.4.1106; R Core Team, 2021). Behavioural data was analysed by a logistic regression model fit between the coefficients *probability of coinciding choices (accuracy)* and *trial number* grouped by *time* (*pre* and *post*) with Wald z-statistics. Only first-best guesses were considered for this paper's purposes.

To assess the experimental task's effect on neurophysiological underpinnings of WM processing moderated by MM, the P3 was analysed by loading averaged ERP amplitudes through MATLAB into R. Firstly, relevant variables such as *stimulus type* (*colour* and *shape*) and *time* were formatted, and outliers were removed. Then, previously calculated averages of single-trial EEG epochs were grouped to create new data frames of one P3 amplitude value per subject per condition, resulting in two data points per participant with six participants per group. Descriptive statistics and data visualisation were conducted. To calculate the mean P3 amplitude, values were averaged across all trials for each participant and subsequently averaged across all participants.

To draw inferential conclusions, a Repeated Measures – Analysis of Variance (RM-ANOVA) with *time* and *stimulus type* was performed to control whether utilising colour or shape stimuli affect the P3 differently. Two further distinct RM-ANOVA tables including the predictor *time* as a within-subjects factor were created to investigate the statistical significance of differences between pre- and post-measurements for experimental and control group separately. The latter analyses were performed twice for data gathered from only the Pz and a combination of Pz, Cz, and Fz. As posthoc analyses, paired t-tests for each participant examined within-subjects ERP differences between both time points. Employed Type I error rates were $p < 0.05$ to judge significance.

Results

Preliminary Analysis

Firstly, 4.3 % of trials (86 out of 2030) were identified as outliers due to ERPs two standard deviations below or above the mean. Those trials were subsequently excluded to minimise the data's skewness and prevent a possible distortion of results (see **Figure 3** to witness the data's transformation).

Behavioural Results

Behavioural data based on accuracy scores was analysed. The overall accuracy across all conditions is 73.2% (n = 1944) which is above our task's chance level (25%). Referring to previous findings that dyads' probability of convergently choosing the same image increases over time (Alberti et al., 2012), accuracy probabilities of dyads were fitted to trial numbers by multiple logistic regression grouped by time (**Figure 4**). Overall, we observed that participants were significantly more likely to choose the same image over the course of trials, proposing that trial number influences accuracy positively compared to the null-hypothesis of no difference (z = 89.14, p < .001). Specifically, whilst the probability of coinciding choices at the start of each session is higher during post-measurement, accuracy levels of both experimental sessions equalise throughout final trials. Yet, the overall difference between time points remained statistically significant (z = 42.61, p < .001) indicating that dyads performed the tacit coordination task more accurately following the intervention (M_{post} = 0.80, SD_{post} = 0.40) than at the baseline (M_{pre} = 0.67, SD_{pre} = 0.47) across all trials.

ERP Results

To assess effects of *stimulus type* and *time* on ERPs, P3 peaks as responses to presented stimuli measured at Pz electrode during the tacit coordination task were visually inspected

(**Figure 5)**. The overall mean P3 amplitude is 4.41 µV with a standard deviation of 6.64 µV (n = 1944 trials). See **Appendix B** for an overview of average P3 amplitudes per subject and condition.

Since **Figure 5** displays slightly higher elicited mean P3 values for colour than shape stimuli, we examined whether the independent variable *stimulus type* affects P3 amplitudes significantly and thus, might be a confounding factor. A two-factorial RM-ANOVA suggested that *stimulus type* does not influence P3 measures significantly (F (1, 20) = 2.371, p = 0.139, η_p^2 = 0.101). Thus, this factor is being disregarded for further analysis. The factor *time* was included to determine whether participants generally exhibited increased mean P3 values across trials during the post-test. Results indicate no statistically significant overall difference in P3 measures between time points (*F(1,20)* = 0.790, p = 0.385, η_p^2 = 0.034).

Examining the main hypothesis, analyses of ERPs acquired at Pz electrode were conducted for both groups separately. For the experimental group the predictor *time* led to statistically non-significant differences in P3 amplitude (*F(1,10)* = 0.025, p = 0.878, η_p^2 = 0.002). Similarly, P3 values did not change significantly for participants without any training (*F(1,10)* = 0.934, p = 0.357, η_p^2 = 0.085).

Since previous literature also outlined Cz and Fz as relevant for WM-related P3 amplitudes next to Pz, identical RM-ANOVAs were conducted with ERP values averaged from combined data gathered at those three sites. Effects of time on mean P3 values were again non-significant (for experimental group: *F(1,10)* = 1.299, p = 0.262, η_p^2 = .037; for control group: *F(1,10)* = 1.129, p = 0.296, η_p^2 =0.032). Hence, utilising Pz as the main electrode for our analyses does not elucidate the non-significant findings. This is further supported given that the

P3 waveform was most pronounced at Pz rather than Cz and Fz electrode (see **Methods** section). Following analyses will focus solely on Pz as initially intended.

Due to small sample size (*n* = 12), statistical power for between-group analyses is low. Hence, P3 changes were subsequently investigated on a within-subjects level. Considering **Figure 6**, no clear pattern in individual ERP differences is recognisable. Whilst most participants undergoing the MM course demonstrate moderately similar ERPs before and after the intervention, slopes of participants in the control group appear more scattered. T-tests investigating all trials for each individual were conducted to analyse the significance of any respective changes. Thus, for each group, six t-tests examining the null-hypothesis that mean P3 amplitudes are equal across time points were employed. Within the experimental group, one t-test was significant, indicating a higher P3 amplitude after the intervention for one subject (dyad 4: $t(165) = -15.42$, $p < .01$). The remaining five subjects lacked significance (dyad 1: $t(152) = 2.744$, $p = 0.111$; dyad 2: $t(155) = 2.560$, $p = 0.125$; dyad 3: $t(158) = -1.177$, $p = 0.360$; dyad 5: $t(170) = 2.740$, $p = 0.111$; dyad 6: $t(164) = -0.897$, $p = 0.464$). Surprisingly, in the control group, five out of six tests were significant. Whilst this findings suggests two participants with a reduced P3 peak at post-measurement (dyad 1: $t(152) = -11.63$, $p < .01$; dyad 5: $t(170) = -13.04$, $p < .01$), the other three exhibited increased P3 values (dyad 2: $t(155) = 7.45$, $p < .05$; dyad 3: $t(158) = 10.53$, $p < .01$; dyad 4: $t(165) = 49.91$, $p < .001$). Dyad 6’s t-test was non-significant ($t(164) = -1.551$, $p = 0.261$).

Hence, neither group showed generally significantly different mean P3 amplitudes compared to the other in either direction at either time point when considering mean P3 amplitudes per subject. Even though isolated results are significant, they cannot aid in drawing meaningful conclusions about our hypotheses considering the limited statistical power imposed by small sample size.

Discussion

This study’s overarching objective – namely elucidating the relationship between WM and ToM and the link between their underlying electrophysiological underpinnings to MM - is not well researched within a social coordination context. Yet, its urgency for clinical and everyday application is evident considering mental health issues such as autism and the omnipresence of joint interpersonal actions in daily life. The present study counts as a novel approach in studying perspective-taking and contributes to this discussion by utilising a multi-participant approach to examine the P3 component as an index of social WM. Specifically, a meditation-naïve population performed a tacit coordination task in dyads twice in a pre-post within-subjects design. Whilst one partner underwent an eight-week internet-based MM course, the other did not receive any training. As MM was found to lead to distinct enhancements of WM which consecutively ought to facilitate cooperative actions via more advanced ToM, we expected the mindfulness intervention to elicit higher P3 amplitudes within the experimental group as this ERP component was frequently reported to reflect WM operations. The control group is predicted to show similar - but less distinct - P3 increases at the post-measurement. The latter hypothesis is based on previous literature illustrating the importance of joint coordination for cognitive development. Hence, we derived that the daily interaction between dyads might lead to carry-over effects of gained WM benefits within the MM course participant to their partner in the control condition (Tudge & Rogoff, 1999).

Overall, our analyses found P3 amplitudes elicited at Pz electrode, whilst participants performed the experimental task. Given earlier research reporting intensified P3 components over parietal sites prior to and during joint actions (Kourtis et al, 2013; Tsai et al., 2006), this result supports the reasoning that dyads engage WM to arrive at coinciding choices. Hence, WM appears necessary for effectively and adaptively handling interpersonal coordination through the ability of mentalising. This becomes specifically clear by explicating the present experiment. Participants must evaluate each novel stimulus array based on mental representations of preceding arrays. By considering their partner's choice responses, dyads can produce shared coordination rules. This suggests that this task prompted participants to model their teammate's thoughts rather than solely apply pattern recognition abilities. Then, after a trial is completed, those implicit response strategies are stored within WM. As each following trial provides additional information, WM subsequently operates on those and repeatedly updates the created coordination rule. As described by the *context-updating hypothesis* (Donchin, 1981; Polich, 2003), this process is supposed to provoke the P3b ERP activity.

Yet, whilst WM and ToM were established to be pivotal in performing the task, results contradict our primary hypothesis that the experimental group showed significantly higher P3 amplitudes in post- compared to pre- measurements was not supported. Noteworthy here, however, is the low statistical power of our analyses as the most prominent bottleneck limiting the reliability of our results. Due to high attrition and technical errors during data acquisition, this study's small sample size of only six participants per group renders drawing conclusions futile. Furthermore, several EEG sessions were stroked by generally low signal-to-noise ratios but nevertheless analysed to avoid further power reductions. Hence, interpretations listed in forthcoming paragraphs should be evaluated with this impediment in mind. Hence, albeit our non-significant results suggest that participants did not exhibit greater WM engagement whilst performing the abstract matching task after learning about and actively practising MM, our result cannot be argued to refute recent hypes about MM as a modern panacea for cognitive functions including social WM and, in turn, ToM (Friedman-Wheeler et al., 2021; Van Dam et al., 2017). Nevertheless, if WM was affected by the current intervention, participants' WM resources should have been greater allowing for more effective ToM application in generating and operating on their partner's perspectives.

Considering our auxiliary hypothesis, control group participants demonstrated non-significantly different P3 values between pre- and post-measurements. This latter finding agrees with previous research as those participants did not undergo any experimental intervention. We surmised control participants to nevertheless demonstrate increased ERP amplitudes due to possible carry-over effects from the intervention's beneficial effects on their romantic partner's WM. Yet, results did not indicate significant WM improvements at the post-measurement within course participants, to begin with. If WM benefits seem to be absent for the experimental group, they consequently cannot transfer to romantic partners within the control condition. Hence, this finding does not fully counter our exploratory prediction as we cannot conclude whether regular daily engagement would suffice to allow for carry-over effects or whether deeper interactive engagement was required. To assess these hypotheses, studies should firstly ensure significant cognitive improvements within participants undergoing mindfulness interventions.

Additional ambiguous conclusions emerge when relating our null-findings of MM training to existing literature. Generally, studies focussing on social contexts reported a surplus of MM-induced benefits within affective, neuroscientific, and cognitive domains. Importantly, though, generalising outcomes of studies characterised by heterogeneous practices must be done with caution given the abundance of conceptual and methodological ambiguities (Jha et al., 2019; Kakumanu et al., 2019; Van Dam et al., 2018). Thus, other research is not universally valid meaning that our MM course teaching interoceptive body- and breath-focussed meditation might not realise similar socio-cognitive benefits as other forms of MM do.

That interventions similarly teaching body- and breath-focussed meditation as well as internal acceptance appeared to enhance mentalising abilities advocates for our MM course to result in comparable ToM improvements (Trautwein et al., 2020). Moreover, Klee and colleagues (2020) reported significantly greater P3 amplitudes in participants completing a six-week web-based MM intervention suggesting that our eight-week online MM course might be equally influential. A behavioural study further proposed MM to have advantageous effects on WM within eight weeks if listening to recorded guided meditation. Yet, the recent debates between distinct MM types used in research indicate a necessity to explore whether aspects of our employed MM course might have rendered it less effective in evoking predicted effects.

One factor pertains to our MM course being designed in an online format without in-person sessions. This reduced experimental control yet increased ecological validity due to the rising popularity of mobile courses (Wahbeh et al., 2014). Especially in the light of COVID-19 but also when considering populations without access or necessary means to engage in on-site interventions, successful remote courses are urgently needed and are often preferred by laypeople due to their convenience. Whilst literature outlining evidence for WM enhancements is considerably more extensive for in-person interventions, web-based MM appears to be at least somewhat, but not equally, powerful (Klee et al., 2020). In contrast, a review by Jha and colleagues (2019) found even two-week MM trainings to reliably influence the P3, yet the importance of in-person classes and consistent daily out-of-class practice of meditation were emphasised as potent determinants for the intervention’s effectiveness. Courses solely based on recordings and videos did not demonstrate similar effects (Banks et al., 2015; Baranski & Was, 2018).

Furthermore, face-to-face MM interventions enhancing cognitive functions allow for frequent interactive exchange between trainer and trainee (Trautwein et al., 2020). Yet, only three discussion sessions were scheduled in our meditation training although active engagement with peers and teachers was found to crucially predict successful learning. The importance of interactive learning was especially emphasised for distance education (Palloff et al., 2001; Su et al., 2005). Our study might be lacking this interactive engagement to sufficiently induce socio-cognitive changes. Additionally, the undersupply of personal interaction with a professional might have led participants to be less engaged with the material which, in turn, might have affected motivation for individual daily practice. Whilst everyone was asked to meditate for up to 20 minutes per day, it cannot be verified that participants indeed followed the instructions consistently rendering motivation for conscientious participation a potential confounding factor. That motivation also affected performance during the experimental coordination task, however, is less likely given the gamified task design fostering focus (Kakumanu et al., 2020). This is also supported by the accuracy score of 73.2% across all conditions and participants clearly above chance level (25%).

A further aspect involves the duration of the course. As previously mentioned, interventions of eight weeks or less were found to functionally as well as structurally influence neural WM-markers. However, methodological discrepancies interfere with applying those conclusions to the current project (Klee et al., 2020; Yakobi et al., 2021). Specifically, it is suggested that reported cognitive alterations in meditators versus non-meditators should be described in terms of temporary state or long-lasting trait influences. Studies describing enhanced perspective-taking abilities were often based on designs measuring state-effects. In contrast, our experiment was not administered directly after meditation practice, thereby measuring trait-effects. Research delineating MM’s long-lasting modulations within the brain is scarce. However, Kakumanu and colleagues (2019) found trait-effects on P3 values in terms of improved WM, but only in proficient meditators with MM experience of at least 5000 hours. Since our intervention’s duration of eight weeks involved far less practice than several thousand hours, it might have merely been insufficient in generating trait-like measurable influences on WM processing.

That our accuracy scores were significantly different between both time points contributes to this uncertainty. Compared to the baseline measure, dyads chose the same image more frequently following the intervention. This could suggest that subjects’ perspective-taking capabilities were enhanced. However, behavioural results are insufficient in determining whether only the partner undergoing the MM course showed increased ToM abilities and hence was better at identifying their partner’s intentions, or whether both partners benefitted from the intervention due to transfer effects which remained detected in our EEG results. Moreover, practice effects might provide for an alternative interpretation as participants might have simply been more familiar with the experimental set-up during the post-measurement and thus, could possibly direct more attention towards the task. The fact that the probability of coinciding choices was significantly higher at the start of the experimental session following the MM intervention compared to the previous session supports this line of reasoning.

In contrast, significantly higher accuracy scores in the post-measurement could also result from increased WM resources and thus, more effective ToM application. Then, the MM course was indeed effective. Since this reasoning is inconsistent with our electrophysiological results, the ERP analysis might be flawed. Klee and colleagues (2020) reported significant MM-induced WM differences by analysing P3 amplitudes through peak-to-peak measurements rather than the more commonly used baseline-peak methodology as applied in the current paper. However, the researchers’ attempt to replicate their significant findings by using the latter baseline analysis failed. Statistical results and subsequent conclusions are highly sensitive to the chosen methodology. Hence, preregistering data-analysis plans ensures careful consideration of different procedures. Establishing norms for certain statistical analyses within future contemplative research before data collection would aid in replicating results based on similar analyses procedures. Additionally, examining P3 latencies might prove useful in measuring WM as they were proposed to reflect WM processing speed (Polich, 2007). Increased behavioural accuracy might be explained through faster mental processing and thus, superior mentalising and/or decision making, instead of greater WM availability as indexed by P3 amplitudes.

Lastly, the P3 component may merely not have been the most suitable neural marker to assess MM-induced WM improvements. Meditation practices affect a plethora of cognitive functions, and thus, underlying mechanisms. Studies reported that MM indeed modifies WM causally (Van Vugt & Jha, 2011; Zeidan et al., 2010), yet those studies did not record ERPs. Instead, MM was found to produce generally altered brain patterns representing enhanced neural efficiency. Such results align with the *neural efficiency hypothesis* suggesting higher WM capacity to be represented by more highly distributed brain activation (Neubauer & Fink, 2009). For instance, meditators exhibited weaker overall neural responses but earlier activation in frontal regions allowing for faster decision-making onsets and thus, more processing time compared to controls (Bailey et al., 2020; Maurer et al., 2015). Regardless, a direct relationship between overall neural activity and WM performance seems unlikely given that MM-induced increased task accuracy might be mediated by other mechanisms. For instance, meditation was found to increase temporal and frontal activity suggesting that increased WM decisional processing and general task monitoring mediates behavioural performance (Bailey et al, 2020).

Future Directions

Reviewing the above-mentioned points, future research should target distinct limitations to elucidate our null-findings. Primarily, sufficient statistical power should be acquired through larger sample sizes. Additionally, to investigate the relationship between MM, ToM and WM in interpersonal contexts, fundamental theoretical frameworks of those concepts within the domains of social neuroscience must be developed (Sedlmeier, 2016). Particularly MM ought to be used as an umbrella term only as it is subsumed by widely differing techniques. Each study should clearly define its applied MM practice and purpose to avoid conceptual ambiguities (Van Dam et al., 2018). This would aid in designing more targeted and thus, more effective, interventions for dynamic, naturalistic coordination. Attributing behavioural and neurophysiological alterations to unique MM-induced effects is crucial for research ranging from forming hypotheses to interpreting results (Trautwein et al., 2020).

Further points concern the study design. Multimodal approaches including comprehensive neurophysiological analyses, behavioural data, and affective self-reports were argued as superior in studying concepts such as MM to assess its overall effect on individuals. A combined effort of multiple analyses angles is required to grasp the complexity of mindfulness' influences on physical and mental faculties. For instance, identifying the effect of mindfulness on general brain patterns is required rather than focussing on single ERPs to pin down MM's influences associated with WM and ToM performance. Adding qualitative interviews would provide subjective experiences which would otherwise remain unheeded yet are nevertheless relevant in assessing MM-induced global cognitive effects. Such a theory-driven exploration allows for data sources to mutually update each other to gain a richer understanding of MM-produced alterations and increases the power of identifying statistically and practically significant effects (Van Dam et al., 2017). Such interactions between theory and practical data will enable valid conclusions about inherent causal links. Finding meaningful improvements in WM through MM to increase successfully calibrated interpersonal coordination through enhanced mentalising abilities could have crucial consequences in clinical contexts and optimise everyday human behaviour.

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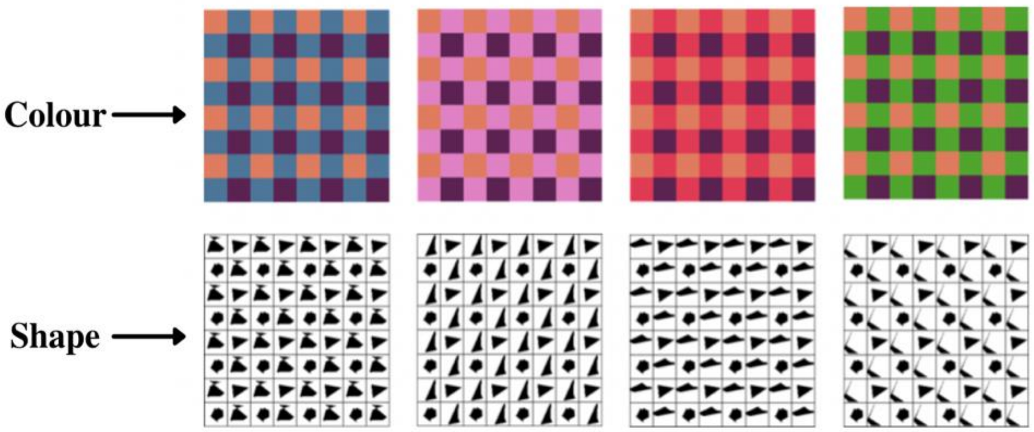
Figure 1

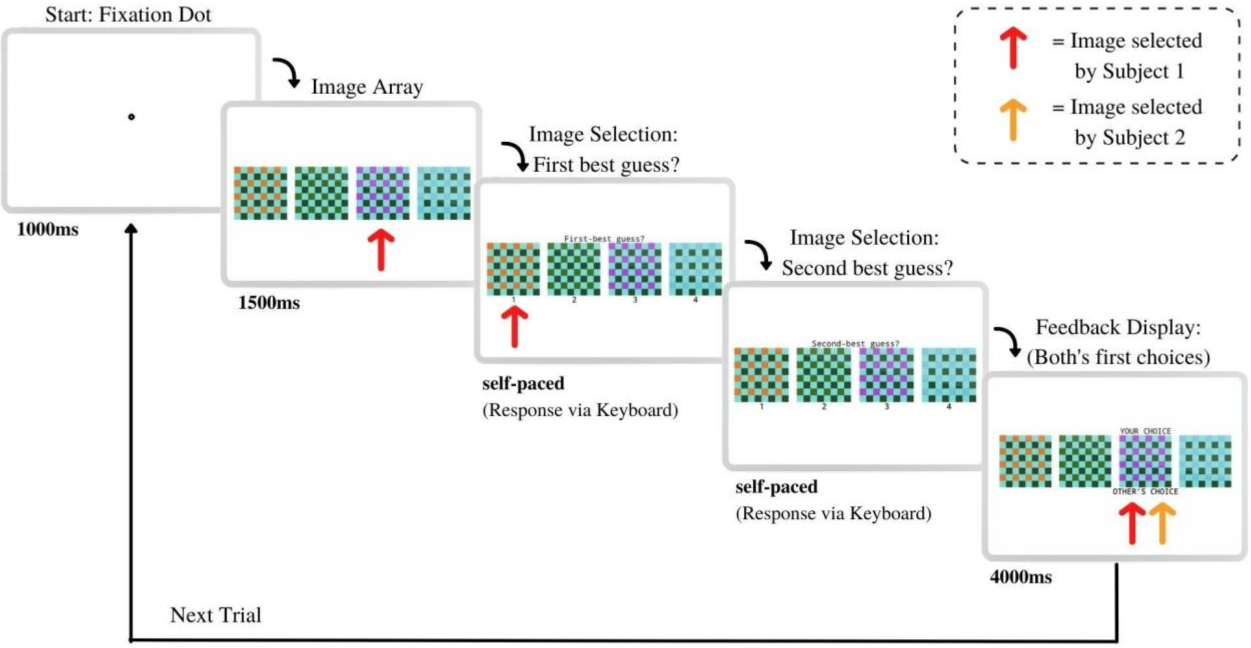
Exemplary Image Arrays



Note. Stimulus compositions within the experimental task are illustrated: *colour* in upper row, *shape* in bottom row.

Figure 2

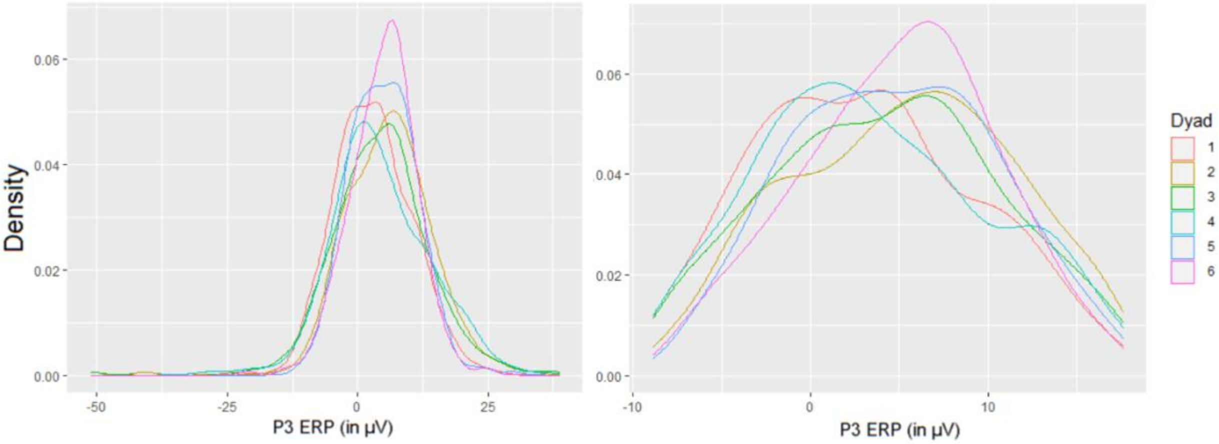
Sample Trial Sequence



Note. Sample trial from subject 1's point of view with '*colour*' stimuli. After a fixation target, the image array appeared. As soon as both partners provided their guesses, players' first-best guesses were depicted before the next trial started. Both players' choices coincided (see *Feedback Display*).

Figure 3

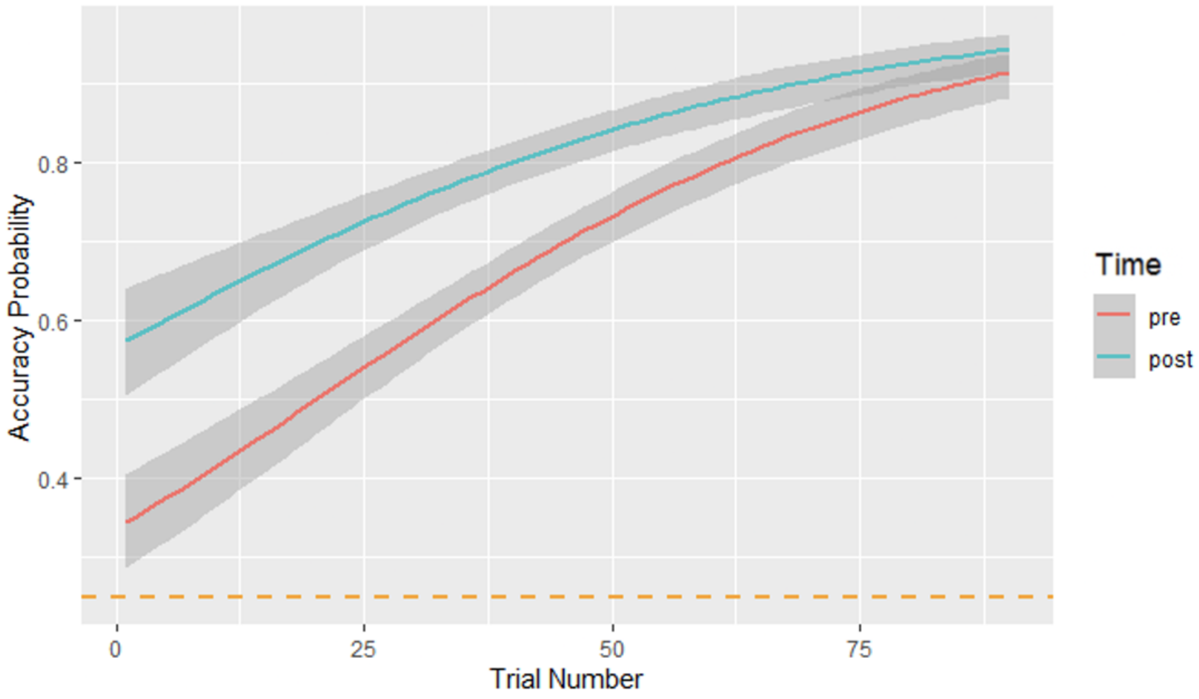
Mean P3 Amplitude Distributions before and after Outlier Removal



Note. The left plot displays the initial density distribution of P3 values at Pz. The right plot shows the distribution after outlier removal. Each line represents the P3 distribution of one dyad averaged across trials and conditions.

Figure 4

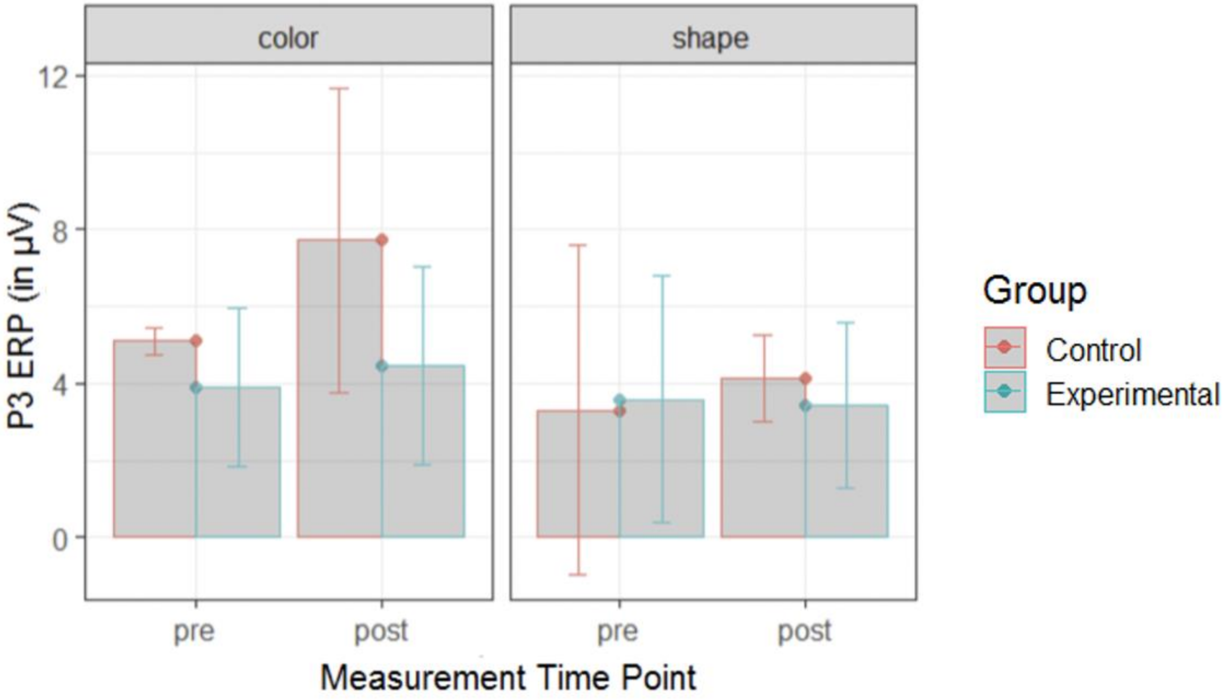
Accuracy Probabilities per Trial Number



Note. The logistic regression model fit averaged across all participants is displayed. Accuracy (y-axis) describes the probability of dyads choosing the same image depending on trial number (x-axis). The red line represents the regression at pre-measurement and the blue line models the fit at post-measurement. The dotted orange line shows the chance-level probability of coinciding choices (0.25).

Figure 5

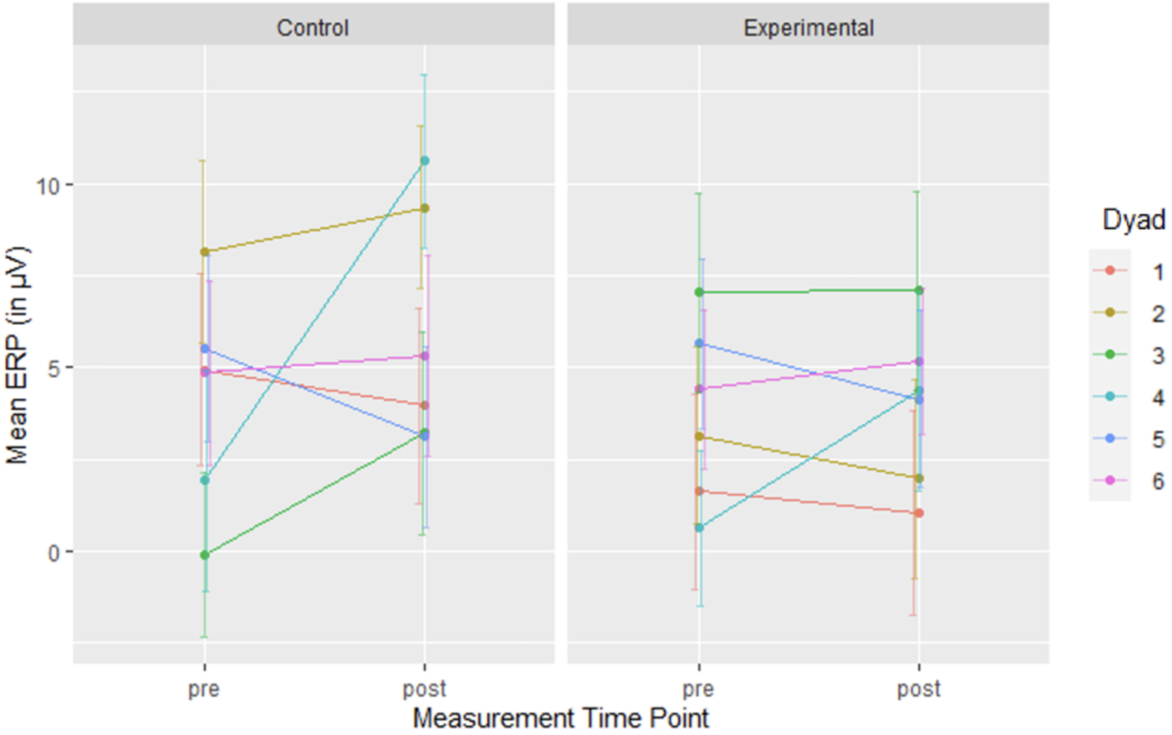
Mean P3 Amplitudes across all Conditions



Note. Each bar represents the average of participants' mean P3 amplitudes at Pz electrode (y-axis) contingent on condition. Error bars signify the range of two SDs. The experimental group underwent the MM intervention. The control group did not receive any training.

Figure 6

Within-Subjects Differences in Mean P3 Amplitudes between Measurements



Note. Each slope shows the difference in mean P3 ERP amplitudes (y-axis) at Pz electrode per dyads between time points (x-axis). Error bars represent 95% CIs. The experimental group underwent the MM intervention. The control group did not receive any training.

Appendix A

Instructions for Well-Being Course: Foundations of Mindfulness

Thank you for participating in this study! As part of this study, you will be enrolled in a course on Coursera, called, “Foundations of Mindfulness”. This contains an interesting and **engaging set of videos** which gives research-based recommendations and tools for how to lead a life with greater well-being. Specifically, you will be introduced to the conceptual and practical components of multiple types of mindfulness practice.

A critical component of this course will be not just learning about meditation, but actually **practicing meditation**. Therefore, we ask you to practice meditation each day for up to 20 minutes per day. In support of this practice, there will be three live **1-hour discussion** sessions spaced throughout the course, so you can go deeper into meditation, discuss your experiences, and receive feedback from a trained mindfulness instructor (Rhoda Schuling).

Please note that you will only be asked to do a select subset of items in the Coursera course. This document details for you exactly which videos to watch, during which weeks of the study. It also explains exactly which mindfulness practice to do each week, and where to find the right guided meditation. We ask you to please *only* watch those videos indicated in this document, and not do any of the other exercises, read the other readings, or watch any of the other videos, on the Coursera site. This is for experimental reasons, where it’s important that everyone taking this well-being course will be doing the same thing. You will still have access to this course after the study is complete. At that time, you are then more than welcome to read, watch, or do any of the material that we asked you to skip during the study period.

The Coursera course is structured differently than this study. Because of these differences, it is important to refer to this document each week to see exactly what we are asking you to do. As you'll see, it's pretty straight-forward. Occasionally, you will see "deadlines" on the Coursera site. You can ignore these. The indications on this document, rather than in the Coursera course, should be your guide for determining when to do what. If you have any questions as the course is running, you are welcome to email Liv Ziegfeld at l.u.ziegfeld@student.rug.nl

If at any point you have questions or concerns about the mindfulness practice(s), please contact this course's mindfulness instructor, Rhoda Schuling at rschuling@gmail.com. You will also meet her in week 2 of the study, during the first discussion session.

Week of 18-October through 24-October

In this first week, you will first watch a guided meditation (16 minutes long), which we ask you to do each day this week. Then, you'll watch a briefer guided meditation (3 minutes long), which you are encouraged to remember and incorporate throughout your day as it may be helpful.

Finally, you'll watch a short set of videos introducing the history, qualities, and practicalities of mindfulness.

I. Please begin by going to Week 2 in your Coursera course: The Foundations of Mindfulness. In the section called "Weekly Activity Challenges", you'll see a video called "Body Scan Meditation". Please watch and follow this video every day this week! This is the critical practice component of the course.

II. Next, please watch the video in Week 1 called "Brief Introductory Meditation". This is a less formal guided meditation practice which you can do at any point during the day this week. After a few times, you probably won't need the video file anymore: we encourage you to play with incorporating short practices at any time in your day!

III. Last, please watch the following videos at your own pace throughout this first week:

- Week 1 Video: Introducing Mindfulness (11 mins)
- Week 1 Video: Exploring the Qualities of Mindfulness Part 1 (6 mins)
- Week 1 Video: Exploring the Qualities of Mindfulness Part 2 (6 mins)
- Week 1 Video: Strategies for Cultivating Mindfulness (3 mins)

Week of 25-October through 31-October

In this second week, you'll continue to follow and practice the Body Scan Meditation daily, in addition to incorporating the Brief Introductory Meditation in your day as you find helpful.

There is also a live discussion session this week on **Thursday 28-October from 16.30 - 17.30**. Liv will contact you with details about how to join. In this session, you will meet Rhoda Schuling, a trained mindfulness instructor, to talk about how your mindfulness practice is going, and receive additional guidance as well.

Finally, please watch the following short set of videos introducing the history, qualities, and practicalities of mindfulness.

Names of Videos to Watch:

- Week 2 Video: Establishing Connection with the Body (3 mins)
- Week 2 Video: The Body as a Reference Point for Awareness (10 mins)

Week of 1-November through 7-November

In this third week, you'll be introduced to another mindfulness practice. You will alternate each day between this new practice and the Body Scan meditation from the previous two weeks.

Please begin by going to Week 3 in the Coursera course and finding the video "Awareness of Breath Guided Meditation", in the Weekly Activity Challenges section. This is the new mindfulness practice. Note: While this video is 11 minutes, we ask you to please meditate for 15

- 20 minutes. You can set your own timer and use that instead of the final bell in the guided meditation.

As mentioned above, this week you will alternate between following the Awareness of Breath Guided Meditation and the Body Scan Meditation. That is, on one day you will practice Awareness of Breath, and the next day you will practice Body Scan. You are also invited to incorporate the Brief Introductory Meditation in your day as you find helpful.

For this week, there is just one further video to watch:

Names of Videos to Watch:

- Week 4 Video: Exploring Common Obstacles During Practice (8 mins)

Week of 8-November through 14-November

In this fourth week, you'll continue to alternate between the Awareness of Breath Guided Meditation (extended to 15 - 20 minutes) and the Body Scan Meditation (and as always, you're invited to use the Brief Introductory Meditation in your day as you find helpful).

This week, there will be another discussion session with Rhoda to touch in about your practice. This will be on **Thursday 11-November from 16.30 - 17.30**. Liv will contact you with details about how to join.

Finally, please watch the following video this week:

Names of Videos to Watch:

- Week 3 Video: Emotions, Thoughts, and the Body (14 mins)

Week of 15-November through 21-November

In this fifth week, you will be provided with two guided meditations from Rhoda: a 20-minute version of the Awareness of Breath meditation and a 20-minutes version of the Body Scan meditation. Please follow one of these each day. You are welcome to choose which one you would prefer to follow on any particular day- there are no rules other than daily practice. In addition, you are invited to continue using the Brief Introductory Meditation in your day as you find helpful

Finally, please watch the following video this week:

Names of Videos to Watch:

- Week 3 Video: The Conditioned Mind (8 mins)

Week of 22-November through 28-November

In this sixth week, you will continue to choose daily between the 20-minute Awareness of Breath guided meditation and the 20-minute Body Scan guided meditation (supplemented, as helpful, by Brief Introductory Meditation).

In addition, there is the third (and final) scheduled discussion session with Rhoda this week. It will occur on **Thursday 25-November from 16.30 - 17.30**. Liv will contact you with details about how to join.

There are no more videos to watch for the remainder of the course. Once this study is complete, you are welcome to return to any of the materials we asked you not to look at while this study is ongoing.

Week of 29-November through 5-December

In this seventh week, you will continue to choose daily between the 20-minute Awareness of Breath guided meditation and the 20-minute Body Scan guided meditation (supplemented, as helpful, by Brief Introductory Meditation).

Week of 6-December through 12-December

In this eight (and final) week, you will continue to choose daily between the 20-minute Awareness of Breath guided meditation and the 20-minute Body Scan guided meditation (supplemented, as helpful, by Brief Introductory Meditation).

Appendix B

Descriptive Statistics of P3 ERPs per Conditions for Each Participant

Subject	Group	Stim. Type	Time	Dyad	Mean ERP	SD
1	**Control**	Colour	Post	2	9.348	5.409
2	**Control**	Colour	Post	3	3.209	6.743
3	**Control**	Colour	Post	4	10.600	5.786
4	**Control**	Colour	Pre	1	4.938	6.418
5	**Control**	Colour	Pre	5	5.513	6.155
6	**Control**	Colour	Pre	6	4.851	6.168
7	**Control**	Shape	Post	1	3.962	6.515
8	**Control**	Shape	Post	5	3.110	6.003
9	**Control**	Shape	Post	6	5.322	6.708
10	**Control**	Shape	Pre	2	8.118	6.077
11	**Control**	Shape	Pre	3	-0.091	5.441
12	**Control**	Shape	Pre	4	1.918	7.349
13	**Exp.**	Colour	Post	2	1.962	6.620
14	**Exp.**	Colour	Post	3	7.073	6.640
15	**Exp.**	Colour	Post	4	44.353	6.605
16	**Exp.**	Colour	Pre	1	1.625	6.511
17	**Exp.**	Colour	Pre	5	5.655	5.663
18	**Exp.**	Colour	Pre	6	4.407	5.268
19	**Exp.**	Shape	Post	1	1.046	6.796
20	**Exp.**	Shape	Post	5	4.143	5.863

21	**Exp.**	Shape	Post	6	5.170	4.870
22	**Exp.**	Shape	Pre	2	3.146	5.919
23	**Exp.**	Shape	Pre	3	7.026	6.577
24	**Exp.**	Shape	Pre	4	0.626	5.151

Note. Mean and Standard Deviation (SD) are provided in µV. Exp. stands for experimental group who completed the MM intervention. The control group did not receive any training.