Attention bias modification training for adolescents with chronic pain

Research Paper

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Attention bias modification training for adolescents with chronic pain: a randomized placebo-controlled trial

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Abstract

Attention bias for pain-related information is theorised to maintain chronic pain, indicating that changing this bias could improve pain-related outcomes. Modifying attention biases in adolescents, when chronic pain often first emerges, may be particularly beneficial. We report here a randomized, placebo-controlled, parallel-group trial of attention bias modification (ABM) training in adolescents with chronic noncancer pain. Adolescent patients (N = 66) were randomly assigned to complete multiple sessions of dot-probe ABM training (N = 23), placebo training (N = 22), or no training (waitlist; N = 21) across a period of 4 weeks. Patients completed all assessments at a hospital-based pediatric pain clinic and completed all training at home. We examined the relative effects of ABM on attention bias and attention control, as well as pain symptomatology (primary outcome), pain catastrophizing, anxiety and depression symptoms, and functional disability (secondary outcomes) immediately after training and 3 months later. We found no evidence that ABM changed attention bias or attention control in comparison with placebo training or no training. We also found that pain and pain-related outcomes were no different for those undergoing ABM compared with placebo training or no training when tested immediately after training or 3 months later. Overall, we found no evidence to support the efficacy of dot-probe ABM for improving pain-related outcomes in adolescents with chronic pain. This study was registered on the NIHR Clinical Research Network Portfolio in August 2014 (UK Clinical Trials Gateway: CPMS 17251) and funded by a Research Training Fellowship awarded to Lauren Heathcote by Action Medical Research for Children.

Keywords: Attention bias modification, Cognitive bias, Pediatric chronic pain, Randomized controlled trial

1. Introduction

Attending toward and focusing on pain are central components in many psychological models of chronic pain.[25,26,48,58,62] In particular, an "attentional bias" toward pain, the tendency to select pain-related information over nonpain information, is argued to increase the salience of pain, enhance fear and catastrophizing, motivate avoidance of painful activities, and increase disability. Given this crucial role, studies have begun to investigate whether pain-related attention biases can be modified and whether this modification impacts pain and pain-related outcomes. One tool for this purpose is attention bias modification (ABM), a computerized training protocol that is proposed to train participants' attention away from pain-related stimuli, possibly by altering top-down attention control.[5,35]

Attention bias modification has been shown to increase cold-pressor pain thresholds in healthy adults,[3,43,54] and to significantly reduce pain intensity and frequency in adults with acute back pain.[53] In chronic pain samples, one uncontrolled study[52] and one placebo-controlled study[9] provided evidence that ABM decreases pain symptoms, although a third placebo-controlled study did not replicate these findings.[53] Studies with chronic pain samples have also provided evidence that ABM significantly reduces pain-related outcomes, such as anxiety sensitivity,[43,53] functional disability,[53] and anxiety and depression symptoms.[52] Taken together, there is preliminary evidence that ABM may be efficacious for improving pain and pain-related outcomes, although findings across studies are mixed and additional, randomized controlled trials are needed.[59]

No studies have yet employed ABM for adolescents with chronic pain, despite previous findings that selective attention biases characterize paediatric patients with chronic pain[2,4] and that therapeutic techniques targeting attention, particularly attention control, can moderate adolescents' cognitive–affective response to pain as well as pain outcomes.[33,34] Strategies such as ABM that can encourage improvements in attention control ability—particularly at a developmental juncture when there are

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age-typical changes in mastery over attention[15,44] and associated maturation of brain regions engaged in goal-directed attention control[44,46,57]—may be especially beneficial. In addition, as chronic pain often first emerges in adolescence,[47] investigating the plasticity and impact of pain-related attention during adolescence may allow consideration of its role in the early emergence and maintenance of chronic pain.

Here, we report a 3-arm randomized, placebo-controlled, parallel-group trial of multi-session ABM in adolescents with chronic noncancer pain. We predicted that ABM, relative to placebo training and no training (waitlist), would significantly reduce attention bias to pain-related stimuli while also increasing attention control. We also expected that ABM would lead to relative improvements in pain symptoms and pain-related outcomes, namely pain catastrophizing, anxiety and depression symptoms, and functional disability. We examined effects of ABM on pain and pain-related outcomes immediately after training and 3 months later, to examine whether training effects maintained or emerged over time.

2. Methods

This study was registered on the National Institutes of Health Research Clinical Research Network Portfolio in August 2014 (UK Clinical Trials Gateway: CPMS 17251).

2.1. Participants

Adolescents aged 10 to 18 years with chronic noncancer pain were recruited from the Oxford Centre for Children and Young People in Pain, part of the Nuffield Orthopaedic Centre at the Oxford University Hospitals. The Oxford Centre for Children and Young People in Pain is part of an orthopaedic musculoskeletal referral system receiving referrals from gastroenterology, neurology, orthopaedics, spinal units, rheumatology, and general practitioners. Patients were eligible for the current study if they reported recurrent or persistent pain for more than 3 months. Patients were excluded if they were currently experiencing severe distress based on expert clinician judgment by a consultant clinical psychologist or did not understand English. Patients were only eligible to take part in the study if they were not scheduled to receive multidisciplinary inpatient treatment, or regular psychological treatment, during the intervention (training) phase. However, patients were permitted to continue attending infrequent (once per week or less) outpatient sessions, including with the occupational therapist or physiotherapist, primarily for assessments. Some patients were also seeking additional complementary and alternative treatments outside of the hospital setting, including acupuncture and nutritional support. These treatments were also permitted if they were being received infrequently (once per week or less). We recruited participants between October 2014 and April 2016, and the final assessment session took place in August 2016. Participants were recruited primarily for the current study investigating ABM training, but we also collected measures of interpretation bias at baseline (data reported elsewhere[32]). Patients and a parent or guardian were first approached about the study either following their first assessment session at the clinic or following their second visit to the clinic during which they attended a pain education class with a small group of other patients and families. During these sessions, a paediatric rheumatologist or the consultant clinical psychologist approached patients and their families and asked if they would like to learn more about taking part in a research study. A member of the research team subsequently provided the families who were happy to be approached with an information sheet, answered questions about the study, and scheduled appointments. Seventy-three patients were interested in the study, and 67 agreed to take part. Reasons for not participating in the study were not recorded for patients who were approached but declined to take part. A CONSORT diagram illustrating the flow of participants through each stage of the trial is shown in **Figure 1**. The first patient was recruited as a pilot participant and only completed a small number of measures to examine feasibility of testing in the hospital setting, and so the data from this patient were excluded from analyses. Thus, 66 patients were enrolled in the full study (55 female; mean = 13.97 years; SD = 2.13). This study was approved by the UK National Research Ethics Service (REC reference 14/SC/0246).

2.2. Procedures

Recruitment and assessment sessions took place at the Nuffield Orthopaedic Centre. At the baseline assessment session, parents/guardians first provided informed consent for their children. In addition, participants aged 16 years or older provided informed consent for themselves. Participants younger than 16 years provided informed assent. Randomisation took place following consent.

2.2.1. Randomisation and allocation concealment

Eligible participants were randomly assigned to 1 of the 3 treatment arms (ABM, placebo, or waitlist), with an equal allocation ratio, based on a computerized random number generator (www.random.org/). The principal investigator (L.C.H.) performed randomisation following successful recruitment and consent, thus allocation to treatment arm was fully concealed to participants and to the research team prior to assignment.

2.2.2. Blinding

Participants assigned to ABM or placebo training were blinded to their treatment condition throughout the trial. Given that participants assigned to the waitlist condition were told that they would not receive the training tasks until after the final assessment session, it was not possible to blind participants assigned to the waitlist condition. Study physicians involved in the participants' otherwise normal care were blind to allocation. Only the principal investigator (L.C.H.), who conducted all assessment sessions, was aware of the participants' treatment conditions out of necessity for sending participants the correct training protocol.

2.2.3. Training protocols

Participants in the ABM and placebo training groups were asked to complete 8 training sessions, twice per week over 4 weeks. Participants were encouraged to do the training during normal waking hours, and to allow at least 1 day between the sessions. SMS reminders were sent to participants, parents, or both, depending on the family's preference. Participants in the waitlist group did not complete any training between the baseline and post-training assessment but were given the opportunity to complete the training after the final (follow-up) assessment session.

2.2.4. Assessment sessions

At the baseline assessment session, we collected demographic information and clinical history. At all assessment sessions (baseline, post-training, and follow-up), participants completed

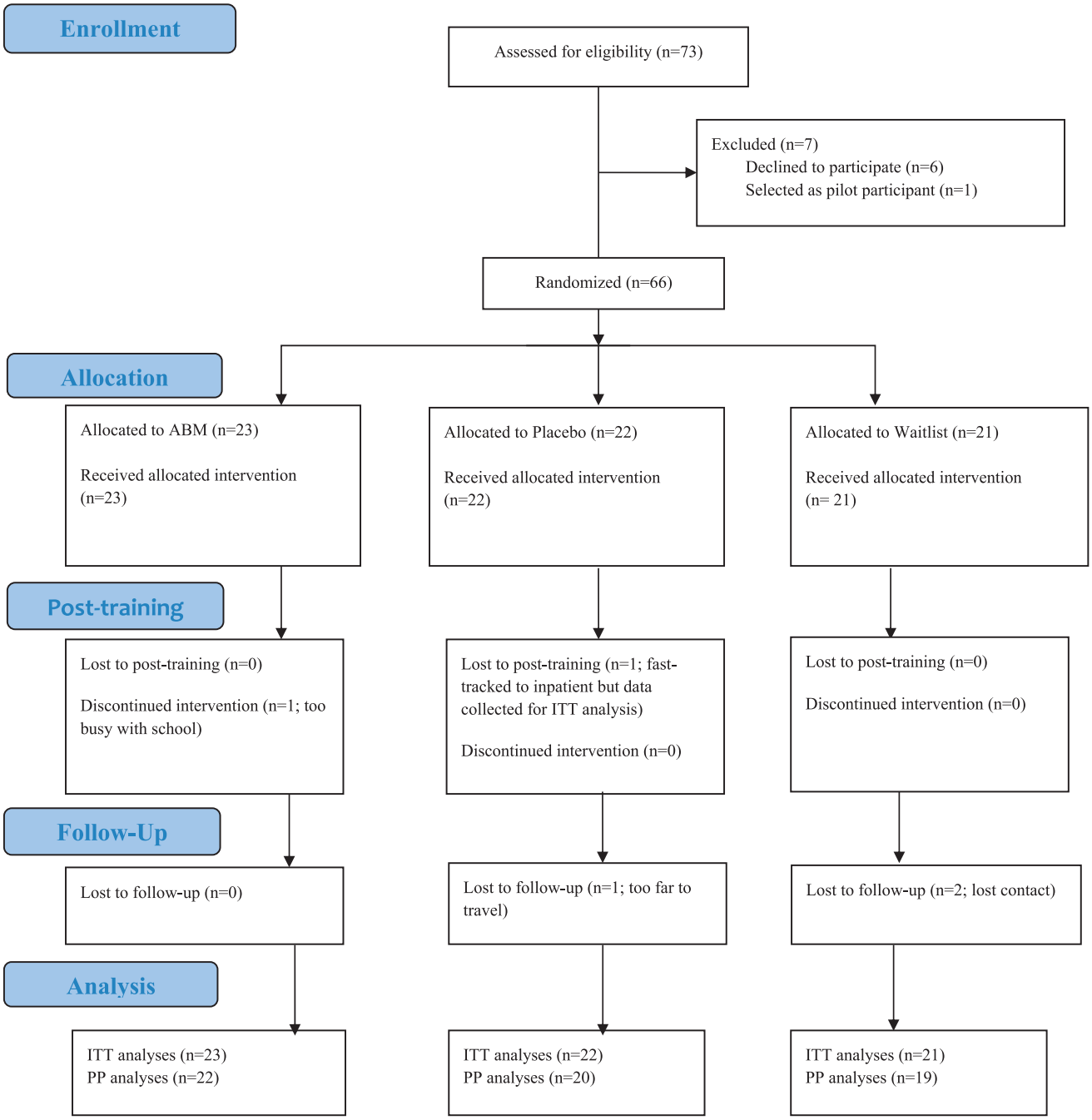

Figure 1. CONSORT diagram illustrating the flow of participants through the study.

the measure of pain symptoms (primary outcome), followed by the measures of attention bias and attention control, physical tests (as an index of functional disability), and finally self-reported functional disability, pain catastrophizing, and anxiety and depression symptoms. The post-training assessment was completed within 7 days of completing the last training session, and the follow-up assessment was completed 3 months after the post-training assessment. In addition to the measures described above, participants completed 2 additional questions at the post-training assessment to provide subjective feedback on the training. Participants were compensated with an Amazon voucher after each assessment session (vouchers worth £100 in total), and travel expenses were reimbursed to parents. All adverse events, regardless of whether they were study related, were monitored throughout and reviewed by the principal investigator (L.C.H.) at each study assessment.

2.2.5. Attention bias modification and placebo training

Attention bias modification and placebo training were delivered using modified versions of the dot-probe task[39] (**Fig. 2**). During ABM and placebo training, stimuli were presented against a black background. Each trial began with a 500-ms presentation of a white fixation cross in the middle of the screen. Participants were instructed to fixate their gaze on this location. Then, one stimulus pair comprising a pain-related stimulus and a neutral stimulus appeared and remained visible for 500 ms (for a similar protocol see Ref. 53). One stimulus was presented above and one below the fixation cross, with the

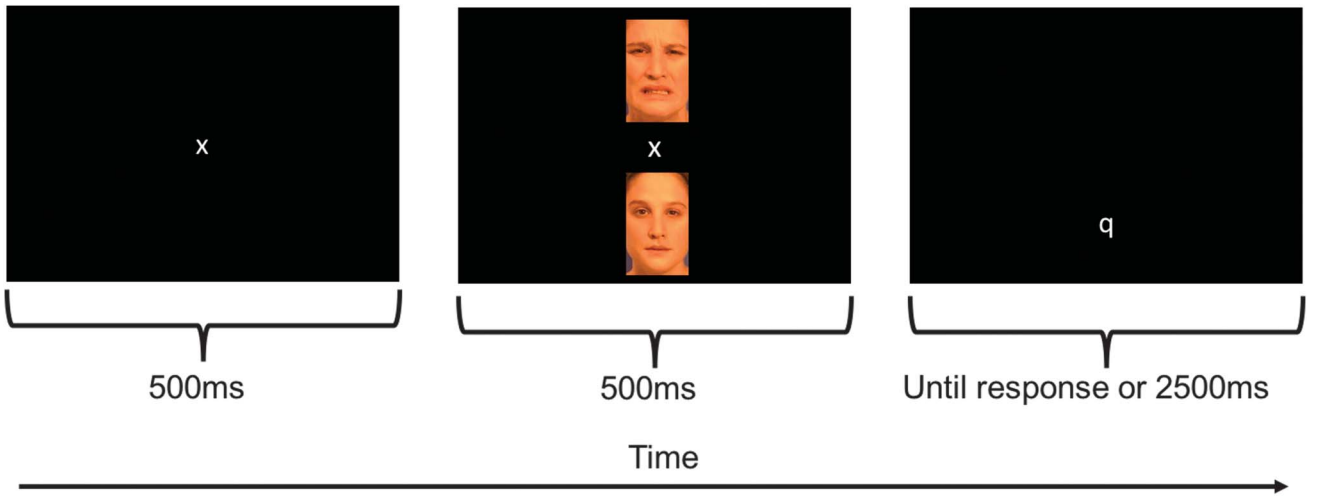

Figure 2. Example trial from one of the ABM training tasks. The stimuli used during ABM were either faces (shown) or words. On each trial, a fixation cross was presented, followed by 2 stimuli (pain/neutral or neutral/neutral), followed by a probe (p or q) to which the patients had to respond. During ABM (shown), the probe appeared behind the neutral stimulus; the placebo ABM condition was identical in every respect other than that the probe was equally likely to appear behind either the neutral or the pain stimulus. Nb.: Stimuli are not presented to scale. ABM, attention bias modification.

distance from the stimuli to the centre of the screen as one-fourth the height of the screen. Immediately after the offset of these 2 stimuli, a letter "p" or "q" would appear in place of one of the stimuli (the probe). During ABM training, the probe *always* appeared in the previous location of the neutral stimulus, and stimulus pairs were randomly presented in each of the 2 possible combinations (probe up/pain stimulus down; probe down/pain stimulus up). Hence, the aim of the ABM training was developed to train participants to attend *away from* pain-related stimuli. During placebo training, the probe appeared equally often in the location of the pain-related stimulus and the neutral stimulus, and thus, stimulus pairs were randomly presented in each of the 4 possible combinations (probe up/pain stimulus up; probe up/pain stimulus down; probe down/pain stimulus down; and probe down/pain stimulus up). In both training conditions, participants had to indicate whether the probe was a "p" or a "q" by pressing the corresponding button on the (QWERTY) keyboard as quickly as possible. The "q" key was pressed with the left index finger and the "p" key was pressed with the right index finger. A new trial started after each response or automatically when 2500 ms elapsed without response. When a participant responded erroneously, the term "error" briefly appeared on the screen (200 ms). To ensure that participants maintained gaze at the middle of the screen, a number of digit trials were presented (see eg, Refs. 34,51). In these trials, the fixation cross was followed by a random digit between 1 and 9 for a duration of 150 ms. Participants were instructed to type the number on the keyboard. The intertrial interval was 1000 ms. In the context of the current study, *congruent* trials were those where the probe was presented at the same location as the pain stimulus. *Incongruent* trials were those where the probe was presented at the same location as the neutral stimulus. Each training session consisted of 210 training trials (96 congruent trials, 96 incongruent trials, and 18 digit trials), taking 12 minutes for participants to complete. Stimuli were presented in a randomized order across trials and participants, and trials were intermixed and randomly presented in 2 blocks, with a break between block 1 and block 2.

The dot-probe tasks were programmed and presented using the INQUISIT Millisecond software package (INQUISIT 4.0). INQUISIT measures response times (RTs) with millisecond accuracy (Inquisit 4.0; Millisecond Software, Seattle, WA, USA). Participants completed all ABM and placebo training at home, using the INQUISIT Web interface. This interface allows participants to access each training task using a unique webpage address. The INQUISIT programme is downloaded to the participant's local computer using a plug-in. Each time the participant finished the task, a data file with the participant's accuracy and RT scores is securely saved to the experimenter's INQUISIT account, allowing the experimenter to verify that the participant had completed each training task and to monitor the participant's performance.

2.3. Measures

All primary and secondary outcome measures, and measures of attention bias and attention control, were completed at baseline, post-training assessment, and follow-up assessment. Details of training tasks and assessment measures are given below.

2.3.1. Demographics

At baseline, participants reported on demographic information, including gender, age, ethnicity, school status, and number of siblings. Participants also reported on the onset of their pain (sudden or gradual), time since pain onset (months), and body locations where they experience pain.

2.3.2. Primary outcome: pain symptoms

To assess pain symptoms, participants completed two 11-point visual analogue scales (VASs) indicating their average pain intensity in the last week, and their pain intensity at the body part they experienced most problems within the last week (hereafter referred to as "most problematic pain") (0 = no pain; 10 = worst pain possible), as well as one 4-point VAS indicating the average duration of their pain each day in the last week (1 = less than 1 hour; 4 = all day). All items were derived from the Brief Pain Inventory.[14]

2.3.3. Secondary outcomes

2.3.3.1. Physical disability

Participants' physical disability was assessed with 1 self-report measure and 2 physical tasks. The self-report measure

(Functional Disability Inventory [FDI]) was the primary measure of physical functioning.

First, participants completed the FDI that assesses perceived difficulty in performing common activities in the domains of school, home, recreation, and social interactions. Participants rate the difficulty they had in carrying out each activity in the preceding 2 weeks (0 = no trouble; 4 = impossible). The FDI consists of 15 items and yields a total score that can range from 0 to 60, with higher scores indicating greater disability. The FDI has yielded good reliability and validity for children and adolescents[63] and for youth with chronic pain.[37] Cronbach alpha for our sample was 0.90 at baseline.

Second, 2 objective assessments of physical function were conducted. Participants completed the sit-to-stand task, which comprised participants attempting to move from a seated to a standing position as many times as possible within 30 seconds. Participants also completed the cardio wall task (http://embeddedfitness.nl/en/exergames/cardio-wall-2/), an interactive task in which participants are required to tap (using their hands) lights on a wall for 30 seconds. Participants must reach high and bend down low to tap the lights, and the maximum height can be adjusted according to the height of the participant.

2.3.3.2. Pain catastrophizing

We assessed participants catastrophic thoughts about pain using the Pain Catastrophizing Scale for Children.[18] Pain catastrophizing comprised rumination, magnification, and helplessness. Participants rated 13 items on a 0 to 4 scale to create a total score from 0 to 52. Higher scores indicate more pain catastrophizing. The Pain Catastrophizing Scale for Children has good reliability and validity for children with chronic pain above 9 years.[18] Cronbach alpha for our sample was 0.88 for the total score at baseline.

2.3.3.3. Generalized anxiety and depression

Generalized anxious and depression symptomatology was assessed using the Revised Child Anxiety and Depression Scale (RCADS).[11] The RCADS consists of 47 items, which are scored on a 4-point Likert scale from never to always. Higher scores indicate increased symptoms of anxiety and depression. The RCADS comprises 6 subscales but here we only calculated scores for Generalized Anxiety Disorder and Major Depressive Disorder as we were interested in whether ABM training would improve general anxiety and depression symptomatology rather than symptoms related to other anxiety disorders, such as separation anxiety or social anxiety. The RCADS has yielded good reliability and validity for children and adolescents.[10,28,42] Cronbach alpha in this study was 0.86 for the Generalized Anxiety Disorder subscale and 0.86 for the Major Depressive Disorder subscale at baseline.

2.3.4. Attention bias measurement (dot-probe) task

The dot-probe task used to measure attention bias was similar to the placebo training tasks (described above), in which participants were presented with stimulus pairs comprising one pain-related and one neutral stimulus and were required to identify a probe (a letter "p" or "q"). Only child picture stimuli (not adult faces or pain words) were used in the attention bias measurement task. Like in the placebo training, the pain-related stimuli and probe were presented equally often at the top or bottom position of the screen in 4 possible combinations: pain stimulus top/probe top; pain stimulus top/probe bottom; pain stimulus bottom/probe bottom; and pain stimulus bottom/probe top. Thus, the probe was equally likely to replace either a pain-related stimulus or neutral stimulus. Stimuli were presented in a randomized order across trials and participants. Trials were intermixed and randomly presented in 2 blocks, with a break between block 1 and block 2. However, unlike the training tasks, each measurement task also began with 20 practice trials comprising 16 neutral stimulus pairs, none of which appeared in the experimental trials, as well as 4 digit trials. Thus, in total, each test phase (baseline, post-training, and follow-up) comprised 274 trials (96 congruent trials, 96 incongruent trials, 64 neutral–neutral trials, and 18 digit trials), taking 12 minutes for participants to complete. Neutral–neutral trials were included as filler trials to reduce possible habituation to pain-related stimuli that might occur if all trials contained pain-related information.[34] Participants completed all attention bias measurement tasks at the hospital, using the INQUISIT Lab programme on an experimenter-provided computer.

2.3.5. Attention control measures

Participants' levels of attention control were assessed with one self-report measure and one experimental measure.

The Attention Control Scale (ACS)[23] is a self-report questionnaire that comprises 20 items and yields a total score that can range from 20 to 80, with higher scores indicating good attention control. The ACS has 2 subscale scores for attention focusing and attention shifting. The ACS has shown good reliability and predictive utility, predicting resistance to interference in Stroop-like spatial conflict tasks as well as attentional disengagement from threat stimuli among highly anxious people.[23] Furthermore, Matthews et al.[41] showed greater activation in brain areas related to top-down regulation of emotion (ie, rostral anterior cingulate cortex) in those reporting greater attention control. Attention control has also been measured with good reliability and validity in children.[45] Cronbach alpha in our sample was 0.81 for the total score at baseline.

The fish flanker task[12,49] is an experimental measure of attention control developed by Christ et al.[12] For this task, participants completed a series of computerized trials in which they saw a horizontal row of 5 fish. On each trial, participants were asked to respond as quickly as possible as to whether the centre fish was facing to the left or right (using the corresponding arrow keys on the computer keyboard). Response times were measured. Two trial types were administered: congruent and incongruent. On congruent trials, all 5 fishes in the stimulus array pointed in the same direction. On incongruent trials, the 4 distracting fishes pointed in the opposite direction of the central target fish. For each trial, stimuli were presented until a response was made or until more than 3000 ms elapsed. After an intertrial interval of 1500 ms, a new trial was presented. If participants failed to respond within 3000 ms, a tone and a message stating "Too slow" were presented. If participants pressed the incorrect arrow button, a tone and a message stating "Wrong response" were presented. Participants completed 120 experimental trials (60 congruent and 60 incongruent trials) that were randomly intermixed. Presentation was balanced such that all possible stimulus–flanker pairings were equally likely to occur. At intervals of 40 trials, children were offered a timed 30 seconds break. Response times on congruent and incongruent trials comprised outcome variables for this task.

2.3.6. Subjective feedback on training

At the post-training assessment, participants completed two 6-point VASs (0 = not at all; 5 = a lot) to gauge their enjoyment in completing the training ("How much did you enjoy doing the training?") and the degree to which participants thought the training helped them ("How much do you think the training helped you?").

2.4. Materials

2.4.1. Dot-probe (attention bias) training and measurement stimuli

Three sets of picture stimuli and 1 set of word stimuli were used in the ABM training tasks. Only picture stimuli were used in the dot-probe assessment tasks, whereas a mixture of picture and word stimuli was used for training.

Stimulus set 1 was used at the baseline assessment session and for 2 of the training sessions. Stimulus set 2 was used at the post-training and follow-up assessment sessions but not during the training. Thus, attention biases were examined at the post-training and follow-up assessments for nontrained stimuli. For the test phase, both stimulus sets comprised 16 facial image stimuli, taken from 8 children (4 male and 4 female participants). For each stimulus set, 8 picture pairs were created. Each pair comprised 2 pictures of the same child. For each set, in 2 of the pairs the child presented a neutral face in both pictures. These picture pairs were used as filler trials (filler trials were not included during the training phase[34]). In the remaining 12 picture pairs (6 pairs per set), there was 1 picture with the child presenting a neutral face and 1 picture with the child presenting a pain face. All pain pictures were drawn from video clips of children taking part in a cold-pressor pain experiment at Ghent University. Stimuli were 160 × 264 pixels in size, presented on an Apple Macbook Pro with a 13-inch screen. As these child stimuli were not previously validated, we asked participants at the post-training assessment session to rate each child face for pain intensity using a 0 to 10 Numeric Rating Scale (ie, "How much pain has the child displayed on the picture; anchors"; "no pain at all"; "a lot of pain") (see Supplementary Materials A for results confirming that stimuli were attributed correctly, available online at http://links.lww.com/PAIN/A492).

Stimulus set 3 was used for 2 of the training sessions. It comprised 12 facial image stimuli, presenting 6 adult faces (3 male and 3 female participants). Stimuli were 160 × 264 pixels in size. All pictures were drawn from 1-second video clips of simulated facial expressions of pain. These pictures were taken from a larger collection of stimuli, previously created and validated in the laboratory by Simon et al.[56] For these stimuli, actors were videotaped while producing neutral facial displays and simulated facial expressions of different pain intensities. Using these 12 pictures, 6 study slides were generated. Each slide consisted of 2 pictures of the same adult, presenting a neutral face or a moderate pain face. These stimuli have been used in prior research examining attention bias to pain.[60] The validity of this stimulus set used is supported by previous findings of significantly different observer pain ratings between neutral and moderate pain facial expressions.[61]

Stimulus set 4 was used for 4 of the training sessions. The total stimulus set comprised 54 pain/neutral word pairs that were matched for length and frequency (see Supplementary Materials B for full list of words, available online at http://links.lww.com/PAIN/A492). However, only 12 word pairs were selected for each participant. These pairs were selected to best match the quality of pain experienced by each individual participant. Therefore, word stimuli were idiosyncratically matched for each participant. To select the 12 word pairs for each participant, participants were provided with the list of 54 pain-related words at baseline and were asked to indicate how well each word described his/her pain (VAS: 0 = not at all; 5 = perfectly). Based on these ratings, we first selected words with the highest ratings (ie, 5 of 5). Next, we selected words with successively lower ratings until a total of 12 word pairs were selected. If more than 12 words were possible to select (ie, with the highest ratings), we selected the words with the shortest length.

2.5. Statistical analysis plan

2.5.1. Incomplete outcome data

Per-protocol (PP) and intention-to-treat (ITT) analyses were conducted. Following guidance from the Committee for Proprietary Medicinal Products, the ITT analysis is considered primary and the PP analysis is considered supportive[31] (PP analysis is presented in Supplementary Materials C, available online at http://links.lww.com/PAIN/A492). Regarding ITT analysis, we used the Baseline Observation Carried Forward method. Five participants (7.58% of the full sample) dropped out of the study at either the post-training or the follow-up assessments and thus did not provide complete outcome data (see **Fig. 1** for details). For those participants in attendance at each session, any questionnaires in which participants did not complete at least 75% of items are removed for that participant. Mean imputation using the relevant subscale for the individual participant's score is used for questionnaire items where less than 25% of the scale is missing.

2.5.2. Sample size determination

Given that there are no similar published studies on ABM in youth with chronic pain, nor on ABM in adults with chronic pain that compare 3 groups (ie, ABM, placebo, and waitlist), a minimum sample size of N = 20 in each group was justified a priori on the basis that (1) this exceeds the sample size determined by Sharpe et al.[53] for their study of ABM in adults with chronic pain, for which sample size estimations revealed that n = 12 participants per group was necessary to achieve 82% power with a significance level of 0.05 (based on findings of an effect size of 0.79 from Ref. 40), (2) this exceeds the sample size determined by Eldar et al.[27] for their study of ABM in youth with anxiety disorders (n = 10-15 in each group), and (3) Simmons et al.[55] suggest a minimum of N = 20 in each group to reduce the likelihood of false-positive findings.

Furthermore, earlier ABM studies of adult chronic pain reported large within-group comparisons across pain severity indices from preintervention to postintervention, from 0.88 (measure of total pain in Schoth et al., 2013) to 1.27 (VAS of current pain severity in Carleton et al., 2011). The more recent ABM trial reported by Sharpe et al. (2001), which did use a control group, reported a between-group effect size of 0.92 on average pain in their first study and a within-group effect size of 0.49 on pain disability in their intervention group in the second study. To the extent that these effect sizes are generalizable to our sample and design, n = 20 in our intervention and control training group should provide 80% power to detect these effects at $P < 0.05$ (2-tailed).

2.6. Analyses

All analyses were conducted using SPSS 23.0 software. We first performed a series of 1-way analyses of variance (ANOVAs) and χ^2 tests to determine whether there were any differences between the 3 randomized groups (ABM, placebo, and waitlist) on demographic variables, outcome variables, and task measures at baseline. Next, we performed *t* tests to determine if participants demonstrated a significant attention bias towards pain at baseline, followed by a 2 (Congruency: congruent, incongruent) × 3 (Time: baseline, posttreatment, follow-up) × 3 (Group: ABM, placebo, waitlist) ANOVA on attention bias and attention control indices to establish whether there was a training effect. The main analyses comprised a series of 3 (Time: baseline, posttreatment, follow-up) × 3 (Group: ABM, placebo, waitlist) ANOVAs for outcome variables. If analyses yielded any significant interaction effects that, when decomposed, revealed a significant effect of ABM training compared to placebo or waitlist, we planned to perform Pearson correlation analyses to assess whether change in attention bias or attention control was associated with change in the outcome variable. If so, formal mediation analyses were planned.

If the sphericity assumption was violated across multiple ANOVA tests (ie, if Mauchly test of sphericity was $P < 0.05$), Greenhouse–Geisser corrections (with adjusted degrees of freedom) would be performed and reported for tests. Of note, due to technical difficulties with INQUISIT, dot-probe (attention bias) data were unavailable for 1 participant (in the waitlist group) at baseline. Data from this participant were retained in all other analyses.

2.6.1. Dot-probe task data preparation

Digit trials were first discarded. Consistent with previous research,[7,34,51,61] trials with errors and responses shorter than 200 ms or longer than 2000 ms were also discarded. Within the present sample, the number of errors made by participants ranged from 2 to 30 (mean = 11.06) for the baseline assessment, from 1 to 73 (mean = 11.63) for the post-training assessment, and from 1 to 36 (mean = 9.20) for the follow-up assessment. After discarding these error trials, 0.2% of RTs in the remaining dataset at baseline, 0.4% of RTs in the remaining dataset at post-training, and 0.2% of RTs in the remaining dataset at follow-up fell outside the range of 200 to 2000 ms. Probe detection latencies that were 3 SDs above or below the individual mean RT of correct responses for each trial type were also considered outliers and thus excluded from analyses.[7,34,38,61] This was the case for 1.4% of the RTs in the remaining dataset at baseline, 1.6% of the RTs in the remaining dataset at post-training, and 1.7% of the RTs in the remaining dataset at follow-up. Filler trials (neutral/neutral trials) were also discarded before mean values and SDs were calculated.

2.6.2. Fish flanker task data preparation

Consistent with previous research,[12] trials with errors and responses shorter than 200 ms or longer than 3000 ms were discarded. Within the present sample, 2% of trials were removed from the baseline assessment, 2.3% of trials were removed from the post-training assessment, and 2.3% of trials were removed from the follow-up assessment. As with the dot-probe data preparation, RTs that were 3 SDs above or below the individual mean RT of correct responses for each trial type were also considered outliers and thus excluded from analyses. This was the case for 1.7% of the RTs on congruent trials and 1.7% of the RTs on incongruent trials in the remaining dataset at baseline, 1.8% of the RTs on congruent trials and 1.8% of the RTs on incongruent trials in the remaining dataset at the post-training assessment, and 1.9% of the RTs on congruent trials and 1.6% of RTs on incongruent trials in the remaining dataset at the follow-up assessment.

3. Results

3.1. Participant characteristics

Participant characteristics are presented in **Table 1**. Time since pain onset varied between 5 and 170 months (mean = 45.7 months, median = 35.5 months). Most participants reported multiple pains, with the most common pain problems including joint pain (N = 36, 54.5%), pain in legs or feet (N = 35, 53%), back pain (N = 33, 50%), and pain in hands or arms (N = 30, 45.5%). Twelve participants (18.2%) reported pain all over their body. When asked to indicate the body location where participants experienced the most pain, they most often indicated their back (N = 16, 24.2%), legs or feet (N = 15, 22.7%), or joints (N = 10, 15.2%). Participants reported moderate levels of disability (as indexed by clinical reference points for the FDI; mean = 22.79, SD = 10.74[37]). Forty-four participants (66.7%) reported experiencing pain every day in the last 3 months, 20 (30.3%) reported pain on most days, and 2 (3%) reported pain on about 1 day per week. Thirty-one participants (47%) reported that they had taken pain medication at some point in the weeks leading up to the study. Randomisation resulted in a balanced distribution across all conditions at baseline, with no significant group differences on demographic, outcome, and task assessment measures (**Tables 1 and 2**). As only 1 participant dropped out of the study before completing the training, we were unable to examine whether there were any significant baseline differences between that participant and those participants who completed training. However, visual inspection of mean scores indicates that the participant who dropped out was similar across task and outcome measures.

3.2. Treatment adherence

Out of the 45 participants who commenced either ABM or placebo training, 25 participants (55.6%) completed all 8 training sessions as scheduled. Of the remaining participants, all participants completed 5 or more training sessions, with the exception of 1 participant who dropped out before completing any training sessions (participant excluded from PP analyses).

3.3. Intention-to-treat analyses

3.3.1. Attention bias analyses

3.3.1.1. Attention bias at baseline

There were no significant differences between RTs to congruent or incongruent trials at baseline (t (64) = 0.33, $P = 0.74$), and the calculated attention bias score was not significantly different from zero at baseline (t (64) = −0.33, $P = 0.74$), indicating no attentional biases to pain-related stimuli across the whole sample (see **Table 2** for mean values and SDs).

3.3.1.2. Change in attention bias following training

A 2 (Congruency: congruent, incongruent) × 3 (Time: baseline, posttreatment, follow-up) × 3 (Group: ABM, placebo, waitlist) ANOVA was conducted for dot-probe scores to assess whether

Table 1

Baseline demographic and pain characteristics.

Measure	ABM (n = 23)	Placebo (n = 22)	Waitlist (n = 21)	Total (n = 66)
Demographics				
Sex, female	20 (87.0%)	17 (77.3%)	18 (85.7%)	55 (83.3%)
Age, y	13.48 (2.15)	13.95 (2.04)	14.52 (2.18)	13.97 (2.13)
Ethnicity				
Caucasian	21 (91.3%)	21 (95.5%)	19 (90.5%)	61 (92.4%)
Asian	1 (4.3%)	1 (4.5%)	1 (4.8%)	3 (4.5%)
Other	1 (4.3%)	0 (0.0%)	1 (4.8%)	2 (3.0%)
Has siblings	21 (91.3%)	20 (90.9%)	18 (85.7%)	59 (89.4%)
Pain variables				
Time since pain onset, mo	44.70 (34.58)	39.18 (23.66)	53.48 (45.25)	45.65 (35.35)
Attends full-time school	19 (82.6%)	18 (81.8%)	14 (66.7%)	51 (77.3%)
Pain onset (gradual)	13 (56.5%)	10 (45.5%)	16 (76.2%)	39 (59.1%)
Pain in more than one area	21 (91.3%)	20 (90.9%)	17 (81.0%)	58 (87.9)
Pain at most problematic site (/10, last week)	7.48 (1.83)	6.86 (1.64)	7.52 (1.63)	7.29 (1.71)
Average pain (/10, last week)	6.09 (1.86)	6.18 (1.89)	6.33 (2.06)	6.20 (1.91)
Pain duration (/4, last week)	3.17 (0.94)	3.39 (0.87)	3.67 (0.66)	3.39 (0.87)

Categorical variables: N (%); ordinal/continuous variables: mean (SD).
Nb.: No significant differences between groups on any measures.

a reliable training effect of ABM could be determined (see **Table 2** for mean values and SDs; see **Table 3** for ANOVA results). Analyses revealed a significant main effect for time, indicating that all participants, regardless of training, had faster RTs with proceeding time. Bonferroni-corrected analyses of simple main effects revealed that RTs were significantly faster at each assessment session (baseline to post-training: $P < 0.001$; post-training to follow-up: $P = 0.01$; baseline to follow-up: $P < 0.001$). There were no other significant main effects and no significant interactions effects. Thus, the predicted training effect was not observed.

3.3.2. Primary outcome measures

A series of 3 (Time: baseline, posttreatment, follow-up) × 3 (Group: ABM, placebo, waitlist) ANOVAs were conducted on pain symptoms (see **Table 2** for mean values and SDs; see **Table 3** for ANOVA results). There was a significant main effect of time for **pain duration**, indicating that all participants, regardless of training, experienced pain for less time each day with proceeding time. Bonferroni-corrected analyses of simple main effects revealed that participants experienced significantly less pain at follow-up compared to baseline ($P = 0.01$) but not compared to post-training (SPSS adjusted $P = 1.0$), nor at post-training compared to baseline ($P = 0.09$). There were no significant main effects for average pain or most problematic pain, and contrary to our hypotheses, there were no significant interaction effects. Due to the absence of significant group differences on primary measures, effect sizes (with 95% confidence intervals [CIs]) were thought to be important for planning future studies, specifically regarding sample size considerations. We therefore calculated effect sizes of differences between the ABM and placebo groups at postintervention for: pain at most problematic site, −0.127 (95% CI: −0.712 to 0.458); average pain, −0.536 (95% CI: −1.131 to 0.05); and pain duration, 0.221 (95% CI: −0.365 to 0.807).

3.3.3. Secondary outcome measures

A series of 3 (Time: baseline, post-training, follow-up) × 3 (Group: ABM, placebo, waitlist) ANOVAs were conducted on secondary outcome measures (see **Table 2** for mean values and SDs; see **Table 3** for ANOVA results). There were main effects of time for **functional disability** (baseline to post-training: $P < 0.001$; baseline to follow-up: $P < 0.001$; post-training to follow-up: $P = 0.04$), **pain catastrophizing** (baseline to post-training: $P < 0.001$; baseline to follow-up: $P < 0.001$; post-training to follow-up: $P = 0.12$), **generalized anxiety** (baseline to post-training: $P = 0.04$; baseline to follow-up: $P = 0.01$; post-training to follow-up: $P = 0.40$), **depression symptoms** (baseline to post-training: $P = 0.37$; baseline to follow-up: $P = 0.04$; post-training to follow-up: $P = 0.21$), **sit-to-stand task** (baseline to post-training: $P = 0.03$; baseline to follow-up: $P = 0.002$; post-training to follow-up: $P = 0.12$), **and cardio wall task** (baseline to post-training: $P < 0.001$; baseline to follow-up: $P < 0.001$; post-training to follow-up: $P = 1.0$), indicating that participants reported improved scores over time on these measures, regardless of training group. There were no other significant main effects, and there were no significant interaction effects. As for primary outcomes, effect sizes (with 95% CIs) of differences between the ABM and placebo groups at postintervention were calculated for: functional disability, 0.032 (95% CI: −0.552 to 0.617); pain catastrophizing, −0.147 (95% CI: −0.733 to 0.438); generalized anxiety, −0.084 (95% CI: −0.669 to 0.501); depression symptoms, 0.084 (95% CI: −0.501 to 0.669); attention control, −0.230 (95% CI: −0.817 to 0.356); sit-to-stand task, 0.366 (95% CI: −0.224 to 0.955); and cardio wall task, 0.213 (95% CI: −0.373 to 0.800).

3.3.4. Attention control analyses

3.3.4.1. Change in attention control after training

To examine whether ABM training changed self-reported attention control, we conducted a 3 (Time: baseline, post-training, follow-up) × 3 (Group: ABM, placebo, waitlist) ANOVA with the ACS score as the outcome variable (see **Table 2** for mean values and SDs; see **Table 3** for ANOVA results). There was a significant main effect of time, indicating that all participants, regardless of training, reported higher attention control over time. Bonferroni-corrected analyses of simple main effects revealed that participants reported significantly higher

Table 2

Mean values and SDs of outcome and process measures for each group at each time point (intention-to-treat analyses).

Measure	ABM			Placebo			Waitlist		
	Baseline	Post-training	Follow-up	Baseline	Post-training	Follow-up	Baseline	Post-training	Follow-up
Primary outcomes									
Pain at most problematic site (last week; 0-10)	7.48 (1.83)	7.17 (1.85)	7.00 (2.11)	6.86 (1.64)	6.95 (1.59)	6.23 (1.95)	7.52 (1.63)	7.05 (2.01)	7.00 (2.14)
Average pain (last week; 0-10)	6.09 (1.86)	6.57 (1.67)	5.39 (2.33)	6.18 (1.89)	5.91 (0.43)	5.50 (1.92)	6.33 (2.06)	6.24 (2.00)	6.48 (2.46)
Pain duration (last week; 1-4)	3.17 (0.94)	2.91 (1.04)	2.61 (1.20)	3.39 (0.87)	3.14 (1.04)	3.18 (1.10)	3.67 (0.66)	3.48 (0.19)	3.38 (1.02)
Secondary outcomes									
Disability (FDI; 0-60)	22.14 (10.70)	19.35 (10.01)	15.65 (10.28)	23.90 (12.29)	19.73 (13.31)	19.18 (12.08)	22.69 (9.64)	19.64 (9.89)	17.43 (9.91)
Pain catastrophizing (PCSC; 0-52)	28.87 (10.96)	26.52 (10.05)	21.43 (11.91)	30.41 (10.30)	24.91 (11.77)	24.27 (10.78)	24.95 (8.44)	20.76 (10.63)	19.86 (13.44)
Depression (RCADS-Depression; 0-40)	12.17 (6.40)	11.52 (6.49)	9.43 (5.70)	12.73 (6.28)	12.09 (7.07)	11.41 (5.94)	10.24 (5.12)	9.81 (5.91)	10.24 (6.88)
Generalised anxiety (RCADS-GAD; 0-24)	7.97 (4.34)	6.65 (3.96)	6.04 (4.54)	7.36 (4.14)	6.32 (3.88)	6.59 (3.51)	5.38 (4.01)	5.86 (3.82)	4.71 (4.10)
Attention control (ACS; 20-80)	47.13 (8.22)	50.26 (8.93)	51.78 (10.49)	47.82 (9.14)	48.14 (9.50)	48.56 (10.85)	49.55 (8.42)	50.57 (10.93)	51.43 (11.18)
Sit-to-stand task	12.57 (3.73)	13.13 (3.92)	14.17 (4.03)	14.77 (4.56)	14.77 (5.01)	16.23 (5.94)	13.90 (4.10)	15.48 (3.74)	15.21 (3.71)
Cardio wall task	45.26 (11.60)	49.70 (13.79)	52.48 (12.72)	48.36 (14.08)	52.91 (16.24)	53.18 (14.64)	49.43 (16.11)	56.10 (13.41)	55.14 (14.25)
Dot-probe task									
Congruent trials (RT)	651.54 (125.25)	564.01 (141.62)	539.31 (89.38)	630.89 (124.35)	537.65 (92.77)	534.51 (91.19)	659.62 (112.32)	625.28 (125.14)	589.48 (113.93)
Incongruent trials (RT)	651.19 (129.62)	557.37 (128.63)	540.88 (91.39)	627.00 (127.31)	534.35 (97.37)	531.65 (88.64)	661.38 (114.08)	617.17 (120.39)	587.90 (112.54)
Fish flanker task									
Congruent trials (RT)	544.67 (81.64)	571.66 (139.08)	515.63 (92.92)	531.13 (131.74)	501.69 (129.29)	495.72 (89.07)	553.47 (123.27)	552.06 (107.12)	525.49 (94.38)
Incongruent trials (RT)	588.61 (90.93)	612.65 (147.01)	564.60 (99.51)	551.77 (122.45)	545.66 (140.33)	530.67 (104.95)	602.05 (161.94)	601.40 (119.24)	562.91 (109.22)

Nb.: Measure ranges noted in brackets next to each measure name.

ABM, attention bias modification; ACS, attention control scale; FDI, functional disability index; GAD, generalized anxiety disorder; PCS-C, pain catastrophizing scale for children; RCADS, revised child anxiety and depression scale; RT, response time.

Table 3

Analysis of variance results for all outcome measures (intention-to-treat analysis).

Measure	Effect	*df*	F	*P*	η_p^2
Primary outcomes					
Pain at most problematic site	Time	1.93, 121.73	2.66	0.08	0.04
	Group	2, 63	0.89	0.42	0.03
	Time × Group	3.86, 121.73	0.43	0.78	0.01
Average pain (last week)	Time	1.70, 106.78	2.30	0.11	0.04
	Group	2, 63	0.45	0.64	0.01
	Time × Group	3.39, 106.78	1.82	0.14	0.06
Pain duration (last week)	Time	1.88, 118.54	4.58	**0.01**	0.07
	Group	2, 63	2.96	0.06	0.09
	Time × Group	3.76, 118.54	0.61	0.65	0.02
Secondary outcomes					
FDI	Time	1.83, 115.20	19.79	**0.00**	0.24
	Group	2, 63	0.20	0.82	0.01
	Time × Group	3.66, 115.20	0.56	0.67	0.02
PCS-C	Time	1.70, 107.13	16.85	**0.00**	0.21
	Group	2, 63	1.38	0.26	0.04
	Time × Group	3.40, 107.13	0.97	0.42	0.03
RCADS-Depression	Time	1.71, 107.58	4.74	**0.02**	0.07
	Group	2, 63	0.61	0.55	0.02
	Time × Group	3.42, 107.58	1.99	0.11	0.06
RCADS-Generalised anxiety	Time	1.64, 103.13	6.20	**0.01**	0.09
	Group	2, 63	1.15	0.32	0.04
	Time × Group	3.27, 103.13	2.09	0.10	0.06
Sit-to-stand task	Time	1.69, 106.36	8.75	**0.00**	0.12
	Group	2, 63	1.50	0.23	0.05
	Time × Group	3.38, 106.36	1.30	0.28	0.04
Cardio wall task	Time	1.90, 119.45	15.08	**0.00**	0.19
	Group	2, 63	0.63	0.53	0.02
	Time × Group	3.79, 119.45	0.54	0.70	0.02
Dot-probe task					
Attention bias index	Time	1.81, 112.08	87.03	**0.00**	0.58
	Group	2, 62	1.09	0.34	0.03
	Congruency	1, 62	2.22	0.14	0.04
	Time × Group	3.62, 112.08	1.44	0.22	0.04
	Congruency × Group	2, 62	0.13	0.88	0.004
	Time × Congruency	1.97, 122.09	1.19	0.31	0.02
	Time × Congruency × Group	3.39, 122.09	0.34	0.85	0.01
Attention control					
ACS	Time	1.83, 113.21	5.36	**0.01**	0.08
	Group	2, 62	0.42	0.66	0.01
	Time × Group	3.65, 113.21	1.18	0.32	0.04
Fish flanker task	Time	1.75, 108.34	5.90	**0.01**	0.09
	Group	2, 62	1.01	0.37	0.03
	Congruency	1, 62	177.62	**0.00**	0.74
	Time × Group	3.50, 108.38	1.13	0.34	0.04
	Congruency × Group	2, 62	1.83	0.17	0.06
	Time × Congruency	1.82, 112.88	0.87	0.41	0.01
	Time × Congruency × Group	3.64, 112.88	2.18	0.08	0.07

Figures in bold indicate significance ($P < 0.05$ or lower).

Nb.: Greenhouse–Geisser adjusted *df* reported across all repeated-measure tests to provide conservative estimates.

ACS, attention control scale; FDI, functional disability index; PCS-C, pain catastrophizing scale for children; RCADS, revised child anxiety and depression scale.

attention control at follow-up than at baseline ($P = 0.02$). Self-reported attention control did not significantly differ between baseline and post-training ($P = 0.15$) or between post-training and follow-up ($P = 0.49$). There were no significant interaction effects.

To examine whether ABM training changed experimentally measured attention control, we conducted a 2 (Congruency: congruent, incongruent) × 3 (Time: baseline, posttreatment, follow-up) × 3 (Group: ABM, placebo, waitlist) ANOVA for RTs on the flanker task (see **Table 2** for mean values and SDs; see **Table 3** for ANOVA results). There was a significant main effect of congruency, indicating that participants were slower on incongruent than congruent trials ($P < 0.001$). There was also a significant main effect of time, indicating that participants, regardless of training, had faster RTs with proceeding time. Bonferroni-corrected analyses of simple main effects revealed that participants were significantly faster at follow-up compared to baseline ($P = 0.01$) and at follow-up compared to post-training ($P = 0.01$). Participant RTs did not significantly differ between baseline and post-training ($P = 1.0$). There were no other significant main effects and no significant interactions effects.

3.4. Adverse events

No adverse events reported by participants were study related. No participants reported adverse events immediately following the baseline session. Eight participants reported adverse life events at the post-training assessment, and 8 participants reported adverse life events at the follow-up assessment. These were flu (N = 1), illness or death of a non-immediate family member (N = 3), visits to hospital emergency departments (N = 1), accident-related injury (N = 1), parent in accident (N = 1), minor medical procedures (N = 1), difficulties at school (N = 3), and death of a pet (N = 1). Eight of these participants also reported pain or other physical symptoms as a specific adverse event; however, these were reported as unrelated to the study procedures. There were no significant differences in the number of adverse events reported between groups.

3.5. Subjective feedback

Participants who completed ABM or placebo training reported that they moderately enjoyed completing the training (mean = 3.58, range = 2-5) and that they believed the training moderately helped them (mean = 2.66, range = 0-5). When asked to predict whether they received ABM or placebo training, 29 participants (65.9%) thought that they had completed placebo training, and there was no significant difference between groups in predicted training received (ABM group: 16 [72.7% placebo]; placebo group: 13 [59.1% placebo]; X^2 (1) = 0.91, P = 0.34).

4. Discussion

This study presents a randomized placebo-controlled trial of multi-session ABM for adolescents with chronic pain. We are not able to reject the null hypothesis that the treatment, placebo training, and waitlist control groups differed. There were no significant differences between ABM training and placebo training or no training (waitlist) groups on pain symptoms. There were also no significant differences between ABM training, placebo training, or waitlist control groups on pain catastrophizing, generalized anxiety and depression symptoms, and physical functioning immediately after training or at 3-month follow-up. Attention bias modification training did not significantly impact attention bias or attention control in comparison to placebo training and no training; thus, the hypothesized training effect was not observed. Effect sizes for group comparisons between the ABM and placebo training condition from baseline to post-training were mostly small. There was a moderate effect size for the comparison of average pain level; however, this (non-significant) effect was in the opposite direction than expected, with the ABM group actually reporting increased pain at post-training and the placebo group reporting reduced pain. Findings were similar across per-protocol and intention-to-treat analyses.

Our findings are somewhat inconsistent with previous findings demonstrating improved pain-related outcomes from ABM. Most relevant for comparison are 2 placebo-controlled studies that have investigated ABM in adults with chronic pain.[9,53] First, Carleton et al.[9] found that adult patients with fibromyalgia reported reduced pain scores following ABM training. Second, Sharpe et al.[53] found that adult patients with chronic pain reported improvements in functional disability following ABM training. In addition, both studies found improvements in a small number of pain-related cognitive–affective factors, including anxiety sensitivity[9,53] and pain-related fear.[9] We did not replicate these findings with our sample of adolescents with chronic pain. A number of differences between the current and previous studies may explain inconsistencies. First, we did not measure anxiety sensitivity or pain-related fear; it may be that ABM influences only these specific constructs or that training effects are stronger in subgroups of patients with high anxiety sensitivity or fear of pain. Thus, these traits measured at baseline could moderate training effects. Second, we used a wider variety of training materials. Third, we administered fewer training trials per session, and fewer sessions overall than some prior adult ABM studies. The ABM trials conducted in children and adolescents on mental health indices offer little consensus on the optimal number of sessions to enhance effectiveness.[16] Therefore, with the goal to maintain engagement among youth (and avoid boredom and fatigue), we decided in consultation with experienced clinicians to limit training to no more than 15 minutes, resulting in around 192 trials per session. Finally, participants in the current study completed all training sessions at home, whilst in previous studies training was completed partially or entirely in the clinic. It has been argued that at-home ABM training may be negatively influenced by the participant being in comfortable and familiar surroundings, whereas a level of mild anxiety, for example, by visiting the clinic, is necessary for activating (and therefore changing) threat-related biases.[8] Completing the task at home may also have meant relatively less engagement with training by the participants due to the absence of an experimenter and/or the presence of uncontrolled distractions (although of note, participants generally completed the training with high accuracy and fast RTs).

Some of our findings are consistent with other studies of ABM for chronic pain and psychopathology populations. First, there was no evidence of a pain-related attention bias at baseline in our sample, and no evidence that biases changed following training, thus replicating findings from Sharpe et al.[53] This may explain our null findings. The absence of an attention bias at baseline could be due to a number of factors. A recent meta-analysis of attention biases in child and adolescent anxiety suggested that use of pictorial over linguistic stimuli and the presentation of "threat" stimuli for longer durations (1000 ms rather than 500 ms) were variables that moderated the link between attention bias and anxiety.[24] Moreover, anxiety-associated attention biases were more likely to characterize older than younger youth, and other studies have also suggested complex interactions between attention control, attention bias, and anxiety.[21] These methodological and sample considerations should be taken into account in future ABM trials to ensure (1) the optimal assessment of biases and (2) that the sample selected manifest biases that could be shifted through training. Speaking to this, studies of ABM efficacy for anxiety and depression disorders indicate that effects on outcomes are most consistently found when biases exist at baseline[30] and when training successfully changes biases.[13] However, Sharpe et al.[53] demonstrated effects of ABM training on pain-related outcomes despite no change in attention bias. These discrepancies may be because of methodological issues, notably that the dot-probe task does not demonstrate good reliability as a measurement tool in pain populations,[22] especially in children and adolescents where greater noise and variability may be present.[20] Changes in biases following ABM training, or a lack thereof, may therefore not be a reliable indicator of actual changes in attentional functioning. Our findings are also consistent with more recent ABM studies in anxiety and depression. Although meta-analyses of cognitive bias modification in adults with anxiety have consistently found at least small but significant effects,[17] the only child-specific meta-analysis has failed to find evidence of benefit in children and young people.[16] Where significant findings have been reported in individual trials, effect sizes are generally low, as demonstrated here as well. Given this, our trial is likely underpowered for the detection of these weak effects.

Nonetheless, our study design had significant advantages compared to previous studies on ABM in pain populations. First, word stimuli were idiosyncratically selected to match the quality of each participant's pain. Personal relevance of pain stimuli may be important for eliciting biases.[21] Therefore, ABM training may be most efficacious when using such personalized stimuli. Second, we included 2 physical tasks (the sit-to-stand task and the cardio wall task) that provided objective measures of physical functioning. These tasks are frequently used in physiotherapy sessions and reflect measures that are used in clinical practice to make decisions about patients' treatment needs and progress. Third, given recent suggestion that ABM may impact attention control rather than valence-specific attentional selectivity,[6] we considered the impact of ABM training on attention control, including both a self-report and an experimental measure. Although ABM training did not change attention control in our data, future studies may consider including other measures of attention control, for example, the Attention Network Test (that is designed to test alerting, orienting, and executive control),[29] as well as antisaccade tasks (that are reliant on inhibitory eye movement patterns).[36] These other tasks could be designed to tap control of attention in pain-specific contexts rather than general trait abilities, as measured here using the ACS.

Findings presented here also add to our understanding of intervention delivery in pediatric chronic pain populations. First, recruitment of patients into the study was very successful: most patients approached by the team agreed to take part. Anecdotal evidence from the current study suggests that the computer-game interface was appealing to participants, suggesting that future interventions using a similar interface may achieve a similarly high recruitment success rate. Second, retention was high and while we collected data remotely, training indices revealed high levels of accuracy and fast RTs. Subjective feedback also indicated moderate levels of task enjoyment and perceived usefulness of the training. This information is particularly important given current concerns over the face validity of computerized ABM training in adult samples.[1] Given the need for novel interventions that are both cost-effective and easy-to-access, we provide evidence that computerized training may be a promising platform for delivering interventions to youth with chronic pain. Of note, we also found improvements across all groups on a number of the task and questionnaire measures over time. These improvements could be explained in part by positive engagement with clinical services during assessments and by practice effects for the cognitive and physical tasks.

In conclusion, we report here the first randomized controlled trial of ABM in a clinical sample of adolescents with chronic pain. We found no evidence that ABM affected pain symptoms compared with placebo training or waitlist control, or other pain-related outcomes. Attention bias modification also did not change attention bias or attention control in comparison with placebo training or no training, thus we did not find the predicted training effect. Calculated effect sizes on primary outcomes at postintervention were also small. However, there are a number of methodological reasons and sample considerations that could have explained these null findings. Tailoring possible ABM to patients most likely to benefit from direct attentional redirection (including recruitment of patients with more homogenous pain complaints) is a priority for future ABM trials. It may be that selecting those with extreme attentional biases, fear of pain, anxiety sensitivity, and poor attention control would enable training effects to be observed more clearly. Other tools may also be needed to better shift attention biases. For example, given recent theory suggesting that attention biases are driven by biased *interpretations* of pain and pain-related information,[19,58] and recent evidence that pain-related interpretation biases characterize adolescents with chronic pain,[32] training tools that target interpretation biases (or other theoretical antecedents of attention bias) may also be useful for investigating the role of cognitive biases in pediatric chronic pain going forward.[50]

Conflict of interest statement

The authors have no conflict of interest to declare.

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Appendix A. Supplemental digital content

Supplemental digital content associated with this article can be found online at http://links.lww.com/PAIN/A492.

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