Cognition 259 (2025) 106102

Contents lists available at ScienceDirect

Cognition

journal homepage: www.elsevier.com/locate/cognit

Does music training improve emotion recognition and cognitive abilities? Longitudinal and correlational evidence from children

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ARTICLE INFO

Keywords:
Music
Ability
Training
Emotion recognition
Plasticity

ABSTRACT

Music training is widely claimed to enhance nonmusical abilities, yet causal evidence remains inconclusive. Moreover, research tends to focus on cognitive over socioemotional outcomes. In two studies, we investigated whether music training improves emotion recognition in voices and faces among school-aged children. We also examined music-training effects on musical abilities, motor skills (fine and gross), broader socioemotional functioning, and cognitive abilities including nonverbal reasoning, executive functions, and auditory memory (short-term and working memory). Study 1 ($N = 110$) was a 2-year longitudinal intervention conducted in a naturalistic school setting, comparing music training to basketball training (active control) and no training (passive control). Music training improved fine-motor skills and auditory memory relative to controls, but it had no effect on emotion recognition or other cognitive and socioemotional abilities. Both music and basketball training improved gross-motor skills. Study 2 ($N = 192$) compared children without music training to peers attending a music school. Although music training correlated with better emotion recognition in speech prosody (tone of voice), this association disappeared after controlling for socioeconomic status, musical abilities, or short-term memory. In contrast, musical abilities correlated with emotion recognition in both prosody and faces, independently of training or other confounding variables. These findings suggest that music training enhances fine-motor skills and auditory memory, but it does not causally improve emotion recognition, other cognitive abilities, or socioemotional functioning. Observed advantages in emotion recognition likely stem from preexisting musical abilities and other confounding factors such as socioeconomic status.

The possibility that music training enhances nonmusical abilities has generated much excitement among researchers, the media, and the public. Numerous studies report that music training improves auditory abilities (e.g., Dubinsky et al., 2019; Schneider et al., 2023; Serrallach et al., 2016), speech perception (e.g., Kraus et al., 2014), prosody perception (Moreno et al., 2009), reading and pre-reading skills (e.g., Linnavalli et al., 2018; Moreno et al., 2009; Seither-Preisler et al., 2014), short-term memory (STM; e.g., Zanto et al., 2022), working memory (WM; e.g., Bugos et al., 2022), executive functions (e.g., Frischen et al., 2021; Moreno et al., 2011), and intelligence (e.g., Okely et al., 2022). These putative far-transfer effects influenced perspectives on behavioral and brain plasticity (Herholz & Zatorre, 2012; Moreno & Bidelman, 2014; Schlaug, 2015; Wan & Schlaug, 2010), domain specificity (e.g., Besson et al., 2011), the biological basis of music (e.g., Clark et al., 2015), and the application of music in clinical and educational settings (e.g., Jespersen et al., 2022). For example, reported cognitive benefits of playing music are used to justify interventions for disadvantaged youth (Harmony Project, 2019) and to promote the inclusion of music in school curricula (Barbaroux et al., 2019; Kraus & White-Schwoch, 2020).

If training in other domains—such as chess, working memory, video games, exergames, executive functions, and physical exercise—rarely produces far-transfer effects (Ciria et al., 2023; Gobet & Sala, 2023; Kassai et al., 2019; Sala & Gobet, 2017a; but see, e.g., Pahor et al., 2022), why would music training be an exception? Arguments for nonmusical benefits of music training are often based on correlational

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https://doi.org/10.1016/j.cognition.2025.106102
Received 15 October 2024; Received in revised form 22 January 2025; Accepted 14 February 2025

data, which preclude causal inferences (Schellenberg, 2020). Although many studies document associations between music training and enhanced performance on nonmusical tasks (e.g., Coffey et al., 2017; Talamini et al., 2017; Schellenberg & Lima, 2024; but see, e.g., Boebinger et al., 2015; Schellenberg et al., 2023), preexisting factors could play a role. Musically trained individuals differ from their untrained peers in genetic predispositions for music (Wesseldijk et al., 2023), as well as in personality, cognitive abilities, and socioeconomic status (SES; Corrigall et al., 2013)—factors that could explain observed associations.

Causal evidence that music training has nonmusical benefits is weak. Some meta-analyses of longitudinal studies conclude that music training produces cognitive gains (Bigand & Tillmann, 2022; Jamey et al., 2024; Lu, Shi, & Musib, 2025; Neves et al., 2022; Román-Caballero et al., 2022), whereas others report null effects after excluding suboptimal studies, such as those lacking random assignment or active control groups (i.e., comparisons with stimulating but nonmusical activities; Sala & Gobet, 2017b, 2020). Publication bias is another concern (Neves et al., 2022), and studies considered uninformative in critical reviews (Schellenberg & Lima, 2024) often contribute to meta-analyses with positive findings, leading to interpretative problems.

It is also curious that research has predominantly examined cognitive rather than socioemotional benefits, particularly because music is linked closely to social and emotional processes (e.g., Clark et al., 2015; Koelsch, 2014; Swaminathan & Schellenberg, 2015). Here, our primary focus was on a central aspect of socioemotional functioning—the ability to recognize emotions in vocal and facial expressions—although we also investigated associations between music training and cognitive abilities. Links between music and emotion recognition could stem from overlapping processing mechanisms (Martins et al., 2021; Nussbaum & Schweinberger, 2021; Thompson et al., 2012) that analyze acoustic cues in music and speech prosody (tone of voice; Coutinho & Dibben, 2013; Curtis & Bharucha, 2010; Juslin & Laukka, 2003). Auditory skills crucial for music, such as detecting small differences in pitch, are also important for recognizing emotions in voices, such as determining whether someone sounds happy or sad (Globerson et al., 2013). Sensitivity to music could therefore correlate with sensitivity to voices, with higher levels of musical expertise potentially improving vocal-emotion recognition.

These improvements could generalize to recognizing facial emotions. Evidence shows that some individuals with prosopagnosia, a disorder of face recognition, also have pitch-discrimination impairments (Barton et al., 2023; Corrow et al., 2019). Similarly, individuals with congenital amusia, a disorder of music processing, have impaired recognition of vocal and facial expressions (Lima et al., 2016). Moreover, music perception and social cognition share neurobiological circuits (Van't Hooft et al., 2021), with both music and prosody engaging medial prefrontal and anterior cingulate sites (Escoffier et al., 2013; Park et al., 2015) that support supramodal socioemotional processing (Peelen et al., 2010; Schirmer & Adolphs, 2017). These findings, alongside proposals that music plays a central role in social functions (Clark et al., 2015; Koelsch, 2013, 2014), motivated us to ask whether music training improves emotion recognition across auditory and visual modalities.

Among adults, musicians typically exhibit advantages in vocal-emotion recognition (Martins et al., 2021; Nussbaum & Schweinberger, 2021) for prosodic stimuli such as sentences with neutral semantics (Lima & Castro, 2011) or pseudowords (Nussbaum et al., 2024). It remains unclear, however, whether this advantage generalizes to faces and whether it is also evident in children, who have greater neurobehavioral plasticity and are more likely to be taking music lessons at the time of testing (Martins et al., 2021). Furthermore, although training could be the causal agent, it is also possible that individuals with better preexisting musical abilities are more inclined to pursue music lessons (Kragness et al., 2021) and have better auditory skills that facilitate emotion recognition.

In a study of young and middle-aged adults, Lima and Castro (2011) found that music training predicted emotion recognition in prosody even after accounting for cognitive abilities. Another study considered music training and musical abilities separately as predictors of emotion recognition in prosody and nonverbal vocalizations (e.g., laughter, crying; Correia et al., 2022). Musical abilities, as assessed with music-perception tests and self-reports, correlated with improved emotion recognition regardless of music training. In contrast, music training did not predict emotion recognition when musical abilities were held constant. In fact, adults with high levels of musical ability but no music training were as good as musicians at recognizing emotions. Thus, preexisting musical abilities may explain improved emotion recognition for adults.

Longitudinal studies of whether music training improves emotion recognition are rare and inconclusive. Three studies of individuals with cochlear implants reported null effects (Chari et al., 2020; Fuller et al., 2018; Good et al., 2017), but the small samples (7 or fewer participants per group) precluded clear conclusions. One study of typically developing children found that 1 year of group keyboard lessons improved their ability to discriminate fear from anger in prosody, but so did drama lessons (Thompson et al., 2004), raising the possibility that the effect stemmed from enjoyable group activities, not from music specifically. The effect is further qualified because it did not extend to singing lessons, the advantage for fear and anger did not generalize to happiness and sadness, and only 43 of 144 children who started the training were tested.

The present investigation used both longitudinal and correlational approaches to ask whether music training improves emotion recognition in childhood. We also sought to isolate effects of training from preexisting musical abilities and other potential confounding variables, such as general cognitive abilities and SES. Study 1 was longitudinal, conducted with 6- to 8-year-olds (M = 7.01 years). The training programs were integrated into the school curricula to promote ecological validity (Tervaniemi, 2023). Children were randomly assigned by class: two classes received music training, two received basketball training (active control), and two no training (passive control). Both music and basketball training were engaging, enjoyable group activities led by a teacher, such that any advantages of music training could be attributed specifically to music. Fig. 1 depicts the design and measures. If music training improves emotion recognition, changes over time should be larger for the music compared to the control groups.

Study 2 was correlational, conducted with 6- to 11-year-olds (M = 7.58 years) who were either musically trained (attending a music school) or untrained. They completed the same emotion-recognition tasks from Study 1 and measures of musical and cognitive abilities. Based on evidence from adults (Correia et al., 2022; Martins et al., 2021; Nussbaum & Schweinberger, 2021), we expected emotion recognition to correlate with both music training and musical abilities. We also expected that musically trained children would have higher SES and better musical and cognitive abilities, as these are known to be selecting factors for music lessons outside of the laboratory (Corrigall et al., 2013; Kragness et al., 2021). If preexisting musical abilities explain advantages in emotion recognition, these abilities should predict emotion recognition regardless of music training and other factors (SES, cognitive abilities). If music training no longer predicts emotion recognition after accounting for musical abilities or other factors, training effects would appear to be indirect or epiphenomenal. Unlike Study 1, Study 2 was not designed to examine causality but rather to (1) assess the role of preexisting musical abilities and (2) identify factors that may underlie associations with music training outside the laboratory.

1. Study 1: Longitudinal

All children were tested at Time 1 (T1), before training, and at Time 2 (T2), approximately 2 years later. The main dependent variables measured emotion recognition in prosody, nonverbal vocalizations, and faces. Additional variables measured music perception and production, motor abilities (fine and gross), general cognitive abilities that were

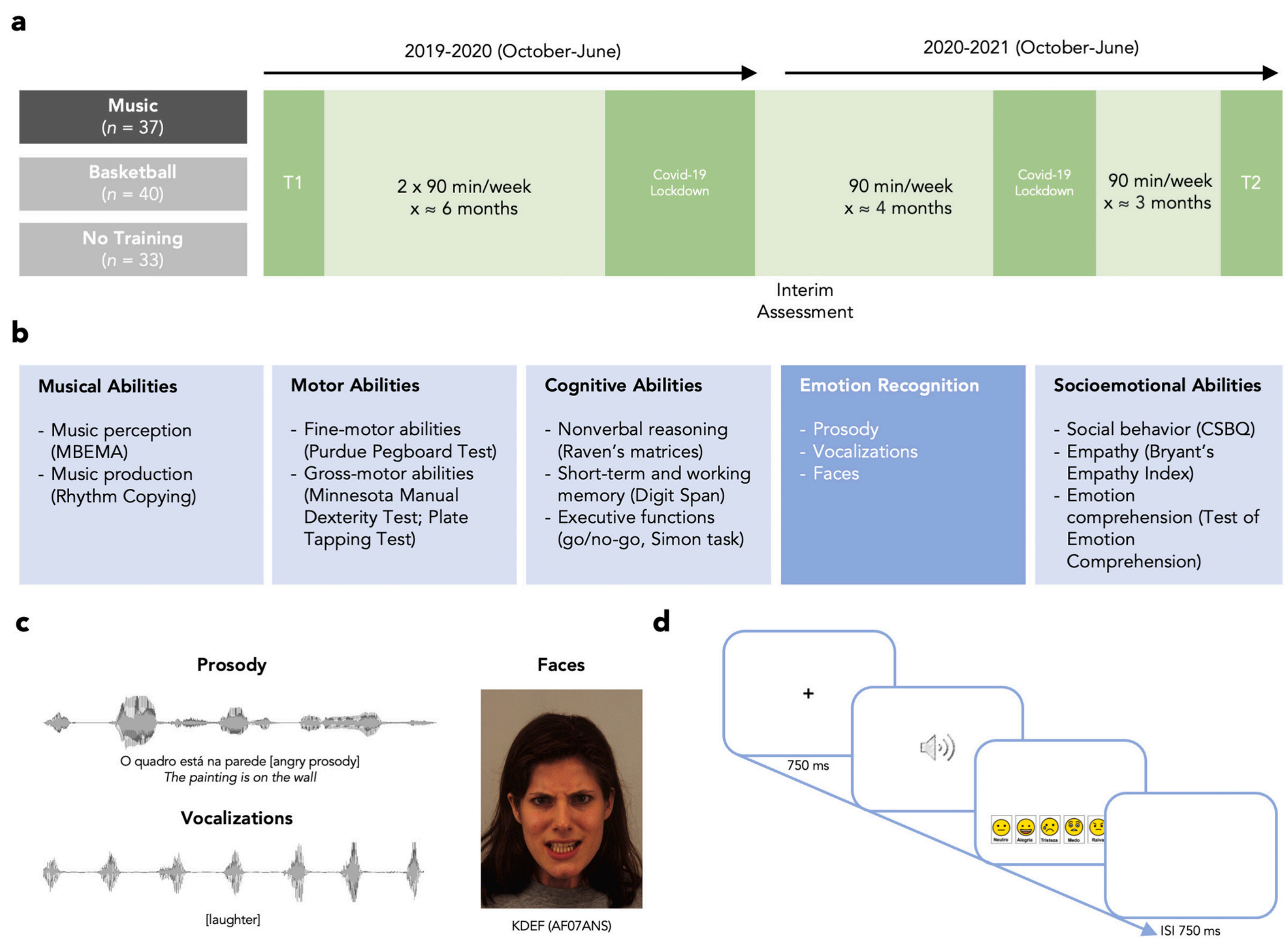

Fig. 1. Overview of the Design and Measures of Study 1. (a) Participants were assessed at two time points: before (T1) and after (T2) 2 school years of training in music, basketball, or no training. The training programs involved two 90-min sessions per week during the first school year, and one 90-min session per week during the second year. The programs were interrupted twice due to the Covid-19 lockdowns. (b) The same measures were administered at T1 and T2, including assessments of emotion recognition, musical abilities, motor abilities, general cognitive abilities, and broader socioemotional functioning. (c) Emotion recognition was measured in the auditory modality, including speech prosody and purely nonverbal vocalizations, and in the visual modality from facial expressions. (d) In the emotion-recognition tasks, each trial ended when the children responded, and no feedback was provided. MBEMA = Montreal Battery of Evaluation of Musical Abilities; CSBQ = Child Self-Regulation and Behavior Questionnaire; KDEF = Karolinska Directed Emotional Faces.

examined in previous studies of music training (nonverbal reasoning, STM, WM, executive functions), and broader socioemotional functioning (social behavior, empathy, emotion comprehension). The test battery allowed us to determine whether any positive effects of training on emotion recognition (1) influenced everyday socioemotional functioning (Neves et al., 2021) and (2) were explained by auditory or general cognitive improvements, mechanisms through which music training could improve nonmusical abilities (e.g., Degé, 2021; Patel, 2014; Schellenberg & Peretz, 2008). The design also allowed us to examine whether musical ability predicted emotion recognition *before* training.

We had additional predictions for other measured variables. One was that music training would improve fine-motor abilities, based on the results from Martins et al. (2018), who used a similar design and tests. We were uncertain about musical abilities, however, because we used standardized music perception and production tasks that were not tailored to the exact skills practiced during the training. Moreover, evidence that music training improves performance on these tasks is mixed (Kragness et al., 2021; Martins et al., 2023). Although a meta-analysis suggested that music training improves auditory processing in general, publication bias could not be excluded (Neves et al., 2022). We were also uncertain about other far-transfer effects—such as gross-motor skills, general cognitive abilities, and broader socioemotional functioning—because evidence from recent reviews and meta-analyses is inconclusive (e.g., Bigand & Tillmann, 2022; Martins et al., 2021; Neves et al., 2022; Román-Caballero et al., 2022; Sala & Gobet, 2017b, 2020; Schellenberg & Lima, 2024).

2. Method

2.1. Participants

The study was approved by the ethics committee at Iscte University Institute of Lisbon (reference 28/2019) and by the school boards. Written informed consent was obtained from a parent or legal guardian. All children provided oral assent.

We recruited 128 second-graders from three public schools in the metropolitan area of Porto (northern Portugal). A background questionnaire asked parents about their child's age and sex, neurological and/or psychiatric diagnoses, extracurricular activities including music training, and parents' years of education. Fourteen children transferred to another school, and four were excluded because of neurological disorder ($n = 2$) or unusually low scores on our test of cognitive ability (Raven's score $< 25^{th}$ percentile, $n = 2$). Thus, the final sample included

110 children from six different classes, 54 girls and 56 boys, who were 7.01 years old on average at T1 ($SD = 0.46$). All were native Portuguese speakers. Mother's and father's education were correlated, $r = .707$, $N = 105$, $p < .001$, and averaged to index SES. On average, parents had 11.1 years of education ($SD = 3.58$).

Two classes of children were assigned randomly to music training ($n = 37$, $M_{age} = 7.08$, 20 girls), two to basketball training ($n = 40$, $M_{age} = 6.96$, 17 girls), and two to a no-training group that received music or basketball training after the study ended ($n = 33$, $M_{age} = 6.99$, 17 girls). At T1, the three groups did not differ in age, SES, or sex, $ps > .5$. Sixty of 110 children had a history of involvement in extracurricular activities other than music lessons ($M_{duration} = 23.7$ months, $SD = 20.9$), with swimming being the most common (for 33 of 60). The groups did not differ in the proportion of children with a history of extracurricular activities, $p = .246$, or in duration of involvement, $p = .817$. Only two children had prior music lessons (one in the music group, one in the basketball group) that started ≤ 6 months before T1.

We recruited as many children as possible, ensuring that each group exceeded 20 participants (Simmons et al., 2011). A post hoc power analysis conducted with G* Power, version 3.1.9.6 (Faul et al., 2009) indicated that with 110 children, we had substantial power (80%) to detect medium-sized differences in improvement between the music and the other two groups ($r = .26$, alpha $= .05$, 3 predictors).[2] Our sample was larger than 85% and 75% of the samples included in the meta-analyses by Neves et al. (2022) and Román-Caballero et al. (2022), respectively.

2.1.1. *Training programs*

Training was integrated into the school curriculum with group sessions conducted for entire classes by teachers specialized in music or basketball (adapted from Martins et al., 2018, to suit 6- to 8-year-olds). Music instruction was based on the Orff approach and covered four domains: musical awareness (e.g., familiarization with musical instruments and genres), basic musical concepts (e.g., rhythm figures, notes, time signatures), rhythm and pitch skills (e.g., ear training and production of rhythmic and melodic patterns), and performance (e.g., improvisation and imitation using the voice or instruments). Playing involved instruments such as xylophones, metallophones, drums, and recorders, along with singing and body percussion. Children learned about musical concepts through playing, with progressively more complex melodic and rhythmic patterns introduced as they advanced.

Basketball training focused on both technical knowledge and skills in four domains: physical fitness (e.g., strength and flexibility), motor coordination (e.g., eye-hand, upper and lower limbs), concepts and practice (e.g., rules of basketball, ball-handling skills), and tactical planning (e.g., occupation of space, cooperation). Fitness and coordination activities progressed from general exercises to basketball-specific ones, with an emphasis on enhancing visuospatial coordination at the individual and team levels.

Both programs spanned 2 school years, consisting of 90-min sessions twice a week during the first year, and once a week during the second year. Each school year lasted approximately 9 months, from October to June, but the programs were implemented for approximately 13 months instead of 18 due to interruptions caused by Covid-19 lockdowns (April to June in the first year, and February to March in the second year). The music and basketball groups attended the same number of sessions. Teachers were hired specifically for the project. Both had experience teaching elementary-school children. A questionnaire completed by the children's primary teacher in October 2020 suggested that the impact of Covid-19 on academic and socioemotional outcomes (e.g., changes in emotional state) was similar across groups (Supporting Information).

2.1.2. *Materials and tasks*

Emotion Recognition. We measured emotion recognition in prosody, nonverbal vocalizations, and faces, using three separate tasks. Each task had 60 trials, with 10 different stimuli expressing each of six emotions: happiness, sadness, anger, disgust, fear, and neutrality. The stimuli were drawn from validated corpora (prosody, Castro & Lima, 2010; vocalizations, Lima et al., 2013; faces, Karolinska Directed Emotional Faces database, Goeleven et al., 2008). Prosodic stimuli were short sentences ($M = 1473$ ms, $SD = 255$) with emotionally neutral semantics (e.g., 'O futebol é um desporto', *Football is a sport*), recorded by female speakers to convey emotions through prosody alone. Nonverbal vocalizations were brief sounds ($M = 966$ ms, $SD = 259$), such as screams or laughs, recorded by female and male speakers. Facial expressions were photographs of male and female actors with no eyeglasses, beards, mustaches, earrings, or visible make-up. Because validation data from adults indicates that recognition accuracy is high across tasks (prosody, 78.4%; vocalizations, 82.2%; faces, 83.0%), we deemed them suitable for children.

The tasks required children to select the emotion conveyed by each stimulus from a set of six response options. Each task had six practice trials followed by two blocks of 30 trials. Stimulus order was randomized for each child. On each trial, a fixation cross appeared on the screen for 750 ms, followed by the stimulus. The trial ended when participants responded. Auditory stimuli were played once, and faces remained visible until the child responded. The interstimulus interval (ISI) was a blank screen lasting 750 ms. Responses options were illustrated using emojis (Fig. 1d), a strategy that proved effective in previous studies with children (Correia et al., 2019). No feedback was provided except during practice trials. Scores were the percentage of correct responses, averaged across all six categories for each task. We had no hypotheses about specific emotions.

Musical Abilities. Music-perception abilities were assessed with the Montreal Battery of Evaluation of Musical Abilities (MBEMA; Peretz et al., 2013). In the Melody and Rhythm subtests, children heard two melodies per trial (20 trials per subtest) and judged whether the second was identical to the first. In the Melody subtest, half of the trials included a change in one note of the second melody, creating a scale, contour, or interval violation. In the Rhythm subtest, durations of two adjacent tones were swapped. In the Memory subtest, children heard a single melody per trial and judged whether they had heard it previously in the Melody or Rhythm subtests, with 10 of 20 trials featuring new melodies. For each subtest, scores were the number of correct responses.

Music-production abilities were assessed with Moore's (2018) revised version of the rhythm-copying subtest of the Music Aptitude Tests (Overy, 2003). On each of 20 trials, children heard a wood-block rhythm and repeated it by pressing a key on a keyboard. The trials became progressively more difficult. Scores were the number of correct responses.

General Cognitive Abilities. Nonverbal reasoning was tested with Raven's Colored Progressive Matrices (Raven et al., 1998), which included 36 items. The score was the number of items answered correctly. Auditory STM and WM were assessed with the Digit Span subtest of the Wechsler Intelligence Scale for Children3rd Edition (Wechsler, 2003). The experimenter read aloud series of single-digit numbers and asked the child to repeat them in the same (forward portion, STM) or reverse order (backward portion, WM). Total raw scores were used.

Executive functions were tested with go/no-go and Simon tasks. The go/no-go task, adapted from Moreno et al. (2011), had four stimuli: red or yellow butterflies, and red or yellow birds. Children were asked to

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[2] Effects of training (three levels) were analyzed with two orthogonal contrasts: a comparison of the music and the control groups, and a comparison of the basketball and the no-training groups. Scores at T1, a third predictor, were held constant. Statistical power for the first contrast was calculated with Test family = *t*-test, Statistical test = linear multiple regression: Fixed model, single regression coefficient, and Type of power analysis = Post hoc: Compute achieved power – given α, sample size, and effect size. Input parameters were two tails, effect size $f2 = 0.0725$ (Partial $R^2 = 0.06764$. partial $R = 0.26$), α err prob. = 0.05, Total sample size = 110, and number of predictors = 3.

press a key for butterfly stimuli (*go* trials), irrespective of color, and to withhold responding for bird stimuli (*no-go* trials). The task had 100 trials (80% go trials, 20% no-go trials). On each trial, a fixation cross appeared for a variable duration (500–750 ms), followed by the stimulus, which remained until a response was provided or for a maximum of 500 ms. The ISI was a blank screen lasting 500 ms. Responses on go (hits) and no-go (false alarms) trials were used to form *d'* scores.

The Simon task, adapted from Bialystok (2006), involved presenting a cartoon fish either facing right or left on the left or right side of the computer screen. The task was to press a key indicating the direction that the fish was facing, ignoring its position on the screen. Half of the trials were congruent, with the fish facing right (or left) on the right (or left) side of the screen. The other half were incongruent, with the fish facing right (or left) on the left (or right) side of the screen. The task had 80 trials, 20 per condition. Each trial began with a 500-ms fixation cross, followed by the stimulus, which was presented until the child responded, with a maximum of 3 s. The ISI was 500 ms. The Simon effect corresponds to the difference in accuracy between congruent and incongruent trials, such that larger scores reflect more interference from irrelevant information (i.e., poorer performance).

Motor Abilities. Fine-motor abilities, particularly fingertip dexterity and hand-eye coordination, were tested with the Purdue Pegboard test (Tiffin, 1968). Children had 30 s to place small pegs into small holes arranged in columns on a board, as fast as possible. Three separate scores were recorded: the number of pins placed properly with the preferred hand, the nonpreferred hand, and both hands. For the task performed with both hands, the score was the number of pairs of pins placed properly.

Gross-motor abilities were evaluated with the Minnesota Manual Dexterity Test (Desrosiers et al., 1997), which was similar to the Purdue test but used larger round cylinders (3.7 cm in diameter) and bigger holes. Scores were the time taken to place 60 cylinders into the holes. Children were tested separately with their preferred and nonpreferred hand. The Plate Tapping test from Eurofit (Adam et al., 1993) provided a second measure of gross-motor abilities, assessing arm movement and coordination. Children tapped two plates (20 cm diameter) placed 80 cm apart. One hand tapped both plates in succession, while the other hand remained stationary in a rectangle positioned between plates. At the beginning, the tapping hand was positioned in the contralateral plate. Children performed the task with both their preferred and nonpreferred hands in two positions (tapping and stationary). The score was the time (in seconds) taken to complete 50 taps (25 on both plates) with each hand. For both the Minnesota and the Plate Tapping test, lower scores indicated better (faster) performance.

For all the motor tasks, the hand order (preferred v. nonpreferred) varied across children. For the Purdue Pegboard test, the task with both hands was always completed last.

Socioemotional Functioning. Social behavior was measured with the Child Self-Regulation and Behavior Questionnaire (CSBQ), a 33-item questionnaire completed by the teacher (Howard & Melhuish, 2017). Scale items covered seven domains: sociability, externalizing problems, internalizing problems, prosocial behavior, behavioral self-regulation, cognitive self-regulation, and emotional self-regulation. Each item was rated from 1 (*not true*) to 5 (*certainly true*). Total scores were used in the analyses. Higher scores indicated more adaptive social behavior. Subscales for maladaptive behaviors (e.g., externalizing) were reversed-coded.

Empathy was measured with the Index of Empathy for Children (Bryant, 1982), a questionnaire completed by the children. For each of 22 items, children agreed or disagreed about whether they would have an empathic response to another child's emotional situation (e.g., *I get upset when I see a girl being hurt*; *Kids who have no friends probably don't want any*). Scores were the number of responses that indicated empathy (for the examples, agree and disagree, respectively) such that the maximum score was 22.

For the Test of Emotion Comprehension (TEC), children were presented with illustrations accompanied by brief stories read aloud by the experimenter (Pons & Harris, 2000; Rocha et al., 2013). The TEC had nine sections, each measuring a different aspect of emotion comprehension, such as understanding the emotional impact of situations, hidden emotions, or mixed emotions. On each trial, children were asked to choose from a set of four facial expressions, the one that best corresponded to the emotion conveyed in the story. Each section was scored as 0 or 1, with a maximum possible score of 9.

2.1.3. *Procedure*

T1 and T2 assessments were conducted by three researchers. At both timepoints, each child participated in three individual sessions in a quiet room at their school. The sessions lasted approximately 2 h in total. The order of the sessions, and of tasks within sessions, varied across children. In one session, children completed two emotion-recognition tasks (prosody and vocalizations), the MBEMA, and the go/no-go task. A second session comprised the Digit Span, rhythm-copying, and motor tasks. A third session included the emotion-recognition task for faces, Raven's matrices, Simon task, TEC, and the empathy questionnaire.

Stimuli for the auditory tasks were presented via headphones (Sennheiser HD 201), with the volume adjusted to a comfortable level for each child. The emotion-recognition, go/no-go, and Simon tasks were implemented in SuperLab X6 (Cedrus Corporation, San Pedro, CA), running on Apple Macbook Pro laptops, with responses collected via a seven-button response pad (Cedrus RB-740).

T1 occurred at the beginning of the 2019–2020 school year, and T2 at the end of 2020–2021. Because of Covid-19, an interim assessment was conducted in October 2020, when teachers completed a questionnaire about the impact of the lockdown, and children completed the musical and motor tests. Changes in musical and motor abilities from T1 at interim testing were consistent with those at T2 (Tables S1 and S2).

The children also completed a task of authenticity recognition in laughs and cries (adapted from Pinheiro et al., 2021; Neves et al., 2018), and a magnetic resonance imaging session at T1, during which resting-state and structural scans were acquired. These data will be reported elsewhere.

2.1.4. *Data analysis*

We used both standard frequentist and Bayesian statistics (JASP 0.18.3, default priors; JASP Team, 2024). For Bayesian analyses, the Bayes factor (BF_{10}, reported with three-digit accuracy) quantified the evidence supporting the alternative relative to the null hypothesis. A $BF_{10} > 1$ indicated evidence for the alternative hypothesis, whereas $BF_{10} < 1$ indicated evidence for the null hypothesis. Following Jeffreys' guidelines (Jarosz & Wiley, 2014), we considered BF_{10} values between 1 and 3 as weak or anecdotal evidence for the alternative hypothesis, values between 3 and 10 as substantial evidence, and values >10 and > 100 as strong and decisive evidence, respectively. Reciprocal values (i.e., BF_{10} = 1–.33, .33–.10, < .10, and < .01) corresponded, respectively, to weak/anecdotal, substantial, strong, and decisive evidence for the null hypothesis. To illustrate, $BF_{10} = 15$ indicates that the data are 15 times more likely under the alternative hypothesis, providing strong evidence for an effect, whereas a $BF_{10} = .067$ (1/15) indicates that the data are 15 times more likely under the null hypothesis, providing strong evidence for no effect. We considered positive results reliable only when both $p < .05$ (uncorrected) and $BF_{10} > 3$.

2.2. *Transparency and openness*

The data used for the analyses are available at https://osf.io/u96fa/?view_only=89240e0a609f4716b9fd02bfc12198ee. We report how we determined sample size, all data exclusions, all manipulations, and all measures in the study. The study's design and analyses were not pre-registered.

3. Results and discussion

We first examined whether groups differed at T1. As shown in Table 1, one-way between-subject Analyses of Variance (ANOVAs) revealed significant *p*-values for 2 of the 21 tests, but BF_{10} was ≤ 1.57 in both cases, indicating no substantial evidence for differences. For 17 of 21 tests, there was substantial or stronger evidence for the null hypothesis. We next confirmed that scores improved from T1 to T2 across the entire sample, $ps < .001$ (Table S3). Bayesian evidence was strong for the Simon Effect, $BF_{10} = 15.3$, and decisive for all other measures, $BF_{10}s > 100$. We also examined correlations between scores at T1 and at T2, which were reliable for most measures, $rs \geq .323$, $ps < .001$, $BF_{10}s \geq 41.6$, except for the Simon Effect, $r = .233$, Go/no-go, $r = .192$, and empathy tasks, $r = .233$, where the evidence was weaker, $ps < .05$, $BF_{10}s \leq 2.30$. In summary, groups were similar at baseline, children improved over time, and individual differences were relatively stable.

3.1. Data reduction

We conducted principal components analyses to form latent variables from measures that were related conceptually and empirically. This approach increased construct validity and reduced the likelihood of measurement-specific or Type I errors. For *musical ability*, a latent variable was derived from scores on the three MBEMA subtests and the rhythm-copying test. At T1, it explained 55.5% of the variance and correlated highly with each original variable ($rs \geq .545$). At T2, it explained 60.0% of the variance and loadings were $\geq .739$. Because latent variables at both T1 and T2 had $M = 0$ and $SD = 1$, main effects of time (reported above) were precluded in subsequent analyses, but we could still detect interactions between group and time. For example, if the music group improved more than the other groups, more children in the music group would go up in rank order from T1 to T2, resulting in larger average improvement (T2 - T1).

For *fine-motor skills*, a latent variable was formed from the three Purdue subtests (preferred hand, nonpreferred hand, both hands). It explained 69.1% of the variance in the original data at T1, and 78.6% at T2, with loadings $\geq .788$ and .862, respectively. For *gross-motor skills*, a latent variable was derived from the preferred and nonpreferred subtests of the Minnesota and plate-tapping tests. It explained 71.1% of the variance at T1 and 68.7% at T2, with loadings $\geq .812$ and .805, respectively. Scores were inverted at both timepoints so that higher scores indicated better performance. Finally, for *executive functions*, a latent variable formed from the Simon and go/no-go tasks explained 54.5% and 56.5% of the variance at T1 and T2, respectively (loadings = .738 and .751).

3.2. Improvement over time

We calculated improvement scores (T2 – T1) for each variable and used planned orthogonal contrasts to maximize power. Specifically, we compared improvements between (1) the music and the two control groups combined, and (2) the basketball and no-training groups. T1 scores were held constant in all analyses to remove variance due to regression to the mean, further increasing power. Results are summarized in Table 2, Fig. 2, and Fig. S1. Although our focus was on group differences, we reported effect sizes as correlation coefficients (*r*) for consistency with Study 2, which was correlational.

The music group did not improve more than controls on the emotion-recognition tasks, although the *p*-value for prosody approached significance. Bayesian analyses favored the null hypothesis in all cases, with the evidence being substantial for faces and weak for prosody and vocalizations. For prosody and faces, *p*-values suggested smaller improvements for the basketball compared to the no-training group, but Bayesian evidence was weak. The findings remained null when we analyzed individual-trials data with mixed-effects models (Supporting Information) or examined specific emotions separately (Table S4). Across the 18 comparisons between the music and control groups (3 tasks × 6 emotions), all $BF_{10}s < 1$.

For STM, WM, and fine-motor skills, there was decisive evidence that the music group improved more than controls, with no differences between the two control groups. For gross-motor skills, there was strong evidence that the music group improved more than controls, but the basketball group also improved more than the no-training group. Follow-up comparisons revealed that the music group outperformed the no-training group, $p < .001$, $BF_{10} > 100$, but not the basketball group, $p = .238$, $BF_{10} = .386$. There were no group differences in improvements for musical ability, nonverbal reasoning, executive functions, social behavior, and empathy. The *p*-value suggested a positive effect of music training on musical ability, but Bayesian evidence was weak.

We conducted two exploratory analyses to clarify the results for musical ability and prosody, where *p*-values suggested a music-training advantage (despite weak Bayesian evidence) and a difference between the two control groups (Table 2). First, we examined individual tests of musical ability rather than the latent variable (Table S5). Effects of music training were null for the Melody and Memory subtests of the MBEMA, $ps \geq .134$, $BF_{10}s \leq .615$, whereas for Rhythm perception (MBEMA) and production (Rhythm Copying), *p* values were significant, $\leq .035$, but Bayesian evidence remained weak, $BF_{10}s \leq 2.23$. Second, we compared the music group with the basketball and no-training groups separately instead of combining them (Table S6). Compared to the music group, the no-training group improved similarly in both musical ability, $p = .384$, $BF_{10} = .347$, and prosody, and $p = .544$, $BF_{10} = .168$, but the basketball group improved less (musical ability, $p = .007$, $BF_{10} = 7.40$; prosody, $p = .007$, $BF_{10} = 5.16$). The music and no-training groups improved similarly across all tests of musical ability, $ps \geq .098$, $BF_{10}s \leq 1.04$ (Table S6). Thus, music training did not improve musical ability or prosody beyond no training. Marginal *p*-values in the main results stemmed from smaller improvements in the basketball group, even compared to no training (Table 2).

In contrast to the null results for music training, musical *ability* had significant positive correlations at T1 with emotion recognition in prosody, $r = .443$, $p < .001$, $BF_{10} > 100$, and in faces, $r = .313$, $p < .001$, $BF_{10} = 28.2$, but not in vocalizations, $r = .163$, $p = .089$, $BF_{10} = .498$. At T2, musical ability predicted emotion recognition across modalities: prosody, $r = .480$, $p < .001$, $BF_{10} > 100$; vocalizations, $r = .289$, $p = .002$, $BF_{10} = 12.3$; faces, $r = .388$, $p < .001$, $BF_{10} > 100$. Individual differences in musical ability were also stable from T1 to T2, $r = .684$, $p < .001$, $BF_{10} > 100$.

3.3. Tests of moderation

Although music training did not improve emotion recognition at the group level, we asked whether changes in STM and WM were associated with larger improvements in emotion recognition. A two-way interaction between music (v. controls) and STM gains was added to a model that included main effects and the comparison between the basketball and no-training groups, controlling for T1 scores for the outcome variable and STM. Increases in STM did not interact with music training for improvements in emotion recognition in prosody, $p = .096$, $BF_{10} = .769$, vocalizations, $p = .171$, $BF_{10} = .564$, or faces, $p = .322$, $BF_{10} = .396$. Similarly, increases in WM did not interact with music training in predicting improvements in prosody, $p = .080$, $BF_{10} = .868$, vocalizations, $p = .068$, $BF_{10} = .914$, or faces, $p = .747$, $BF_{10} = .263$. Substantial evidence for the null hypothesis was observed only for faces.

Finally, we asked whether music training benefited the children who began the study with lower emotion-recognition abilities, based on findings that music lessons improved social skills only among children with below-average skills at baseline (Schellenberg et al., 2015). We included an interaction term between music training and emotion recognition at T1 in our tests of improvements in emotion recognition. The interaction was not significant for prosody, $p = .191$, $BF_{10} = .377$, vocalizations, $p = .413$, $BF_{10} = .258$, or faces: $p = .108$, $BF_{10} = .681$,

Table 1
Descriptive Statistics at T1 for the Three Groups of Children in Study 1, with p-Values and Bayes Factors (BF_{10}) from Group Comparisons.

Variable	Music		Basketball		No Training		*p*	BF_{10}
	M	*SD*	*M*	*SD*	*M*	*SD*		
Prosody	60.00	13.93	61.67	16.65	52.73	16.36	.044	1.14
Vocalizations	84.95	7.49	82.67	8.93	82.17	6.06	.263	.256
Faces	82.30	7.41	77.67	9.28	79.55	9.50	.072	.773
MBEMA Melody	12.49	2.41	12.55	2.25	12.00	1.85	.523	.145
MBEMA Rhythm	13.73	3.11	13.37	2.63	13.09	2.44	.622	.125
MBEMA Memory	13.97	3.13	13.77	2.75	13.67	2.39	.896	.093
Rhythm Copying	5.81	3.06	5.05	3.27	5.24	3.30	.566	.136
Raven's Matrices	22.73	4.45	23.28	4.91	23.03	3.95	.868	.096
Digit Span Forward	5.65	1.18	5.55	1.04	5.76	1.15	.734	.110
Digit Span Backward	3.03	0.96	2.83	0.96	3.00	0.94	.601	.130
Simon Effect	15.54	12.94	15.88	18.10	12.42	16.64	.615	.126
Go/No-go *d'*	1.64	0.76	1.85	0.60	1.89	0.53	.202	.319
Purdue PH	11.68	1.83	11.28	1.78	11.45	1.50	.594	.131
Purdue NH	10.62	1.57	10.23	1.56	10.12	1.58	.364	.195
Purdue BH	8.49	1.79	7.88	1.62	7.88	1.52	.192	.334
Minnesota PH	101.05	12.64	107.18	15.09	106.61	13.37	.112	.526
Minnesota NH	108.76	15.36	114.23	19.70	112.48	11.82	.327	.215
Plate Tapping PH	27.78	5.03	27.45	5.18	26.45	4.08	.494	.151
Plate Tapping NH	30.14	5.37	30.28	6.23	28.55	4.54	.347	.202
CSBQ	3.73	0.56	3.99	0.49	3.67	0.60	.031	1.57
Empathy	11.81	2.94	11.53	2.73	12.39	3.53	.477	.156
TEC	6.19	1.22	6.25	1.37	6.36	1.19	.846	.098

Note. MBEMA = Montreal Battery of Evaluation of Musical Abilities; Purdue = Purdue Pegboard test; Minnesota = Minnesota Manual Dexterity Test; PH = preferred hand; NH = nonpreferred hand; BH = both hands; CSBQ = Child Self-Regulation and Behavior Questionnaire; TEC = Test of Emotion Comprehension.

Table 2
Comparisons of Improvement Over Time in Study 1 Between the Music and the Two Control Groups, and the Basketball and No-Training Groups. T1 Scores Were Held Constant. r indicates effect size.

Variable	Music v. Controls			Basketball v. No Training		
	p	*r*	BF_{10}	*p*	*r*	BF_{10}
Prosody	.057	.183	.828	.048	−.190	.953
Vocalizations	.539	.060	.191	.902	−.012	.160
Faces	.208	.123	.369	.041	−.198	1.30
Musical Ability	.041	.197	1.73	.078	−.170	1.05
Nonverbal Reasoning	.111	.154	.827	.819	.022	.257
Short-Term Memory	<.001	.498	>100	.058	.183	1.20
Working Memory	<.001	.450	>100	.555	−.057	.239
Executive Functions	.600	.051	.210	.729	−.034	.194
Fine-Motor Skills	<.001	.667	>100	.140	.143	.375
Gross-Motor Skills	.001	.306	28.7	.001	.312	34.7
Social Behavior	.267	.108	.538	.438	−.075	.403
Empathy	.363	−.088	.269	.686	−.039	.195
Emotion Comprehension	.983	.002	.164	.055	.186	.980

Note. Positive values for *r* indicate that the music group had *larger* mean improvement compared to the other children (comparison Music v. Controls), or that the basketball group had larger improvement than the no-training group (comparison Basketball v. No Training). Indexes of musical ability, fine-motor skills, gross-motor skills, and executive functions were latent variables derived from individual measures within each domain.

although substantial evidence for the null hypothesis was observed only for vocalizations.

In sum, music training improved fine-motor skills, STM, and WM, but had no significant effects on our primary outcomes—emotion recognition in voices and faces—or other cognitive and socioemotional abilities. Musical ability, however, correlated positively with emotion recognition at both the beginning and end of the study. For gross-motor skills, music and basketball training led to similar improvements, which were larger than those for the no-training group.

4. Study 2: Correlational

In the second study, we compared children with and without music training on tests of emotion recognition in voices and faces, with training varying naturally rather than being experimentally manipulated. If music training does improve emotion recognition, a real-world positive association should be evident even after accounting for confounding factors such as SES. Although the null results from Study 1 made this seem unlikely, factors related to the training, such as pedagogy, school, or classroom dynamics, may have played a role. Alternatively, if preexisting musical abilities are an important factor, they should predict emotion recognition independently of training and other confounding variables.

5. Method

5.1. Participants

Participants were 192 children, 99 girls and 93 boys, with an average of 7.58 years ($SD = 1.04$). The untrained group ($n = 156$) included 110 children from Study 1 and 46 newly recruited from three other schools. The trained group ($n = 36$) included 6- to 11-year-olds from a music school, with an average of 27.7 months of music lessons ($SD = 19.0$). Older children were included in the trained group to maximize sample size and duration of training. The sex balance did not differ between groups, $p = .203$, but musically trained children were significantly older ($M = 9.00$ years, $SD = 1.35$) than the untrained group ($M = 7.25$ years, $SD = 0.58$), $p < .001$. Age was held constant throughout the statistical analyses. Musically trained children were also more likely to have participated in other extracurricular activities, $\chi^2(1, N = 192) = 7.87$, $p = .005$, $\phi = 0.202$, with 80.6% of musically trained children (29/36) and 55.1% of untrained children (86/156) having an average of 40.2 ($SD = 23.8$) and 23.1 ($SD = 21.0$) months of activity, respectively. Because of positive skew, music training and involvement in other extracurricular activities (hereafter *other activities*) were both coded as dummy variables (1 = yes, 0 = no).

As in Study 1, mothers' and fathers' years of education were correlated, $r = .858$, $p < .001$, and averaged to index SES ($M = 11.8$ years, $SD = 3.65$). SES data were missing for three children and replaced with the group mean. As in Corrigall et al. (2013), musically trained children came from higher-SES families ($M = 15.2$, $SD = 1.93$) than the untrained children ($M = 11.0$, $SD = 3.50$), $p < .001$.

We sought to recruit as many children as possible. Post hoc power

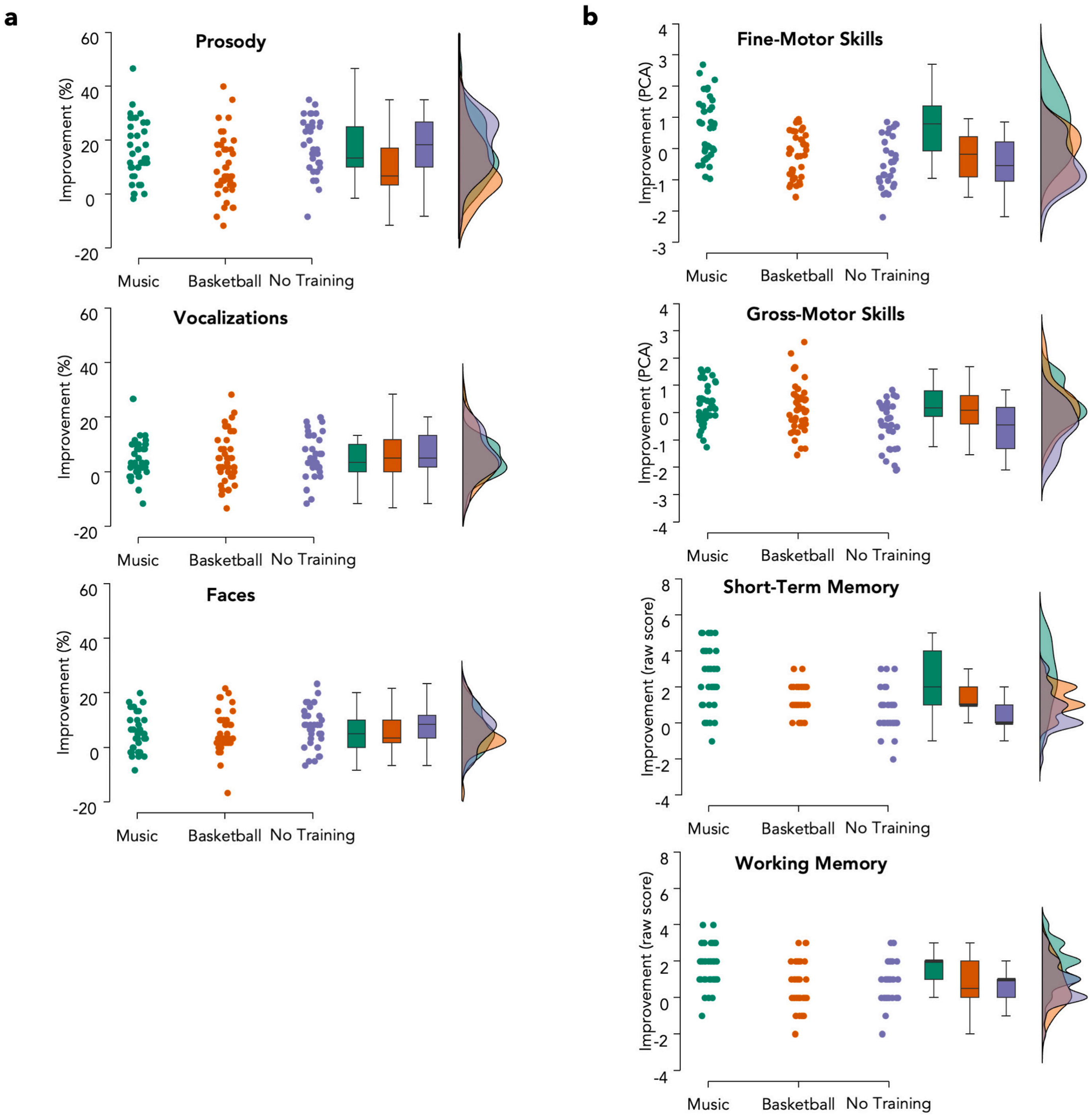

Fig. 2. Improvement Over Time for the Three Groups of Children in Study 1 in (a) Emotion Recognition and (b) Motor Skills, Short-Term Memory, and Working Memory. *Note.* Improvement corresponds to difference scores (T2 – T1). PCA = latent variable extracted using principal components analysis.

analysis confirmed that we had sufficient power (80%) to detect partial correlations of .2 or greater between music training and emotion recognition, with four other variables held constant.[3]

5.2. Procedure

For the children from Study 1, we considered T1 scores. The 82 newly recruited children completed the three emotion-recognition tasks and the tests of musical ability (MBEMA), nonverbal reasoning (Raven's), STM (Digit Span Forward), and WM (Digit Span Backward). To shorten the testing session, we omitted tests of rhythm copying, executive functions, motor ability, and social behavior. One child's Raven's score was missing.

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[3] G*Power settings: Test family = *t*-test; Statistical test = linear multiple regression: Fixed model, single regression coefficient; and Type of power analysis = Post hoc: Compute achieved power – given α, sample size, and effect size. Input parameters: two tails, effect size $f2 = 0.0417$ (Partial $R^2 = 0.04$, partial $R = 0.2$), α err prob. = 0.05, Total sample size = 192, and number of predictors = 5.

6. Results and discussion

We derived a principal component for musical ability from the three MBEMA subtests, which explained 67.6% of the variance and correlated highly with the individual variables ($rs \geq .784$).

Descriptive statistics and comparisons between musically trained and untrained children on the emotion-recognition tasks are provided in Table 3 and Fig. 3 (Table S7 reports simple associations among all variables). The data provided strong evidence that musically trained children performed better on the prosody task (Fig. 3a). For vocalizations and faces, however, the data provided substantial support for the null hypotheses: evidence of no association between music training and emotion recognition. After controlling for SES, other activities, musical ability, nonverbal reasoning, STM, and WM, music training no longer predicted emotion recognition from prosody, $r = .045$, $p = .544$, $BF_{10} = .321$ (Fig. 3b), with Bayesian evidence indicating substantial support for the null hypothesis. Similar null results were evident when we examined individual emotions separately (Table S8).

In contrast, after accounting for music training and all other variables, musical *ability* predicted emotion recognition from prosody $r = .299$, $p < .001$, $BF_{10} > 100$, and faces, $r = .211$, $p = .004$, $BF_{10} = 13.4$ (Fig. 3c, d), but not from vocalizations, $r = .117$, $p = .113$, $BF_{10} = 1.18$. For prosody, musical ability was associated with all emotions but fear, while for faces the association was only evident for fear (Table S8). Exploratory analyses of individual tests of musical ability revealed that both Melody, $r = .197$, $p = .007$, $BF_{10} = 8.00$, and Memory, $r = .319$, $p < .001$, $BF_{10} > 100$, predicted prosody performance, but Rhythm did not, $r = .137$, $p = .064$, $BF_{10} = 1.44$. Memory also predicted performance for faces, $r = .264$, $p < .001$, $BF_{10} > 100$, and vocalizations, $r = .172$, $p = .020$, $BF_{10} = 4.09$, but recognizing emotions from faces or vocalizations was not associated with Melody (faces: $r = .066$, $p = .337$, $BF_{10} = .532$; vocalizations: $r = .026$, $p = .770$, $BF_{10} = .425$) or Rhythm (faces: $r = .124$, $p = .095$, $BF_{10} = 1.27$; vocalizations: $r = .057$, $p = .446$, $BF_{10} = .523$).

We next examined which variables eliminated the association between music training and prosody. We ran a separate model for music training and each predictor variable, examining the effect of training on prosody while controlling for the predictor and age. Controlling for SES alone removed the advantage for musically trained children, $r = .126$, $p = .082$, $BF_{10} = .918$. The same finding was observed after controlling for musical ability, $r = .067$, $p = .360$, $BF_{10} = .259$, or STM, $r = .116$, $p = .112$, $BF_{10} = .699$. The association between music training and prosody remained after controlling for nonverbal reasoning, $r = .238$, $p < .001$, $BF_{10} = 36.4$, or other activities, $r = .179$, $p = .014$, $BF_{10} = 4.01$. When WM was held constant, $r = .167$, $p = .021$, $BF_{10} = 2.78$, the association with music training was weak. Note that after accounting for individual differences in musical ability, the data provided substantial support for the null hypothesis, indicating no partial association between music training and prosody, presumably because of shared variance between musical ability and music training, $r = .239$, $p = .001$, $BF_{10} = 34.1$ (age, SES, other activities, nonverbal ability, STM, and WM held constant). Although this finding was evident for the latent variable of musical ability, when individual tests were held constant, the data did not provide substantial support for or against an association between music training and prosody (Melody: $r = .131$, $p = .071$, $BF_{10} = .972$; Rhythm: $r = .132$, $p = .069$, $BF_{10} = .978$; Memory: $r = .132$, $p = .070$, $BF_{10} = .846$).

As shown in Table 3, groups comparisons on our cognitive measures revealed decisive evidence for superior STM and WM among trained compared to untrained children, although both groups performed similarly for nonverbal reasoning. The partial association between music training and STM remained evident even after we controlled for SES, other activities, and musical ability, $r = .233$, $p = .001$, $BF_{10} = 30.2$, as it did for WM, $r = .174$, $p = .017$, $BF_{10} = 3.35$, with the data providing very strong and substantial evidence, respectively.

7. General discussion

Does music training enhance children's emotion recognition? In Study 1, 2 years of music training did not lead to greater improvements in recognizing emotions from voices or faces compared to control groups. A marginal difference emerged for prosody but was deemed anecdotal according to Bayesian analyses. In any case, the difference stemmed from smaller improvements in the basketball group; the music and no-training groups had similar gains. In Study 2, music training correlated positively with recognizing emotions from speech prosody, but the association disappeared after adjusting for confounding variables such as SES. By contrast, musical *ability* predicted emotion recognition for both prosody and faces independently of music training and other variables.

Previous research linked music training to enhanced emotion recognition in adults (Martins et al., 2021; Nussbaum & Schweinberger, 2021). Our results extend this association to children, but they also challenge the assumption that training plays a causal role. Study 1 had a larger sample and longer training than comparable studies (e.g., Neves et al., 2022; Román-Caballero et al., 2022; Sala & Gobet, 2020), yet effects on emotion recognition were null across auditory and visual modalities, even though we combined the two control groups and controlled for baseline performance to maximize statistical power. Results were similarly null when we focused on participants with low baseline emotion-recognition scores, or on those showing larger improvements in STM and WM.

The basketball group showed the smallest improvement in prosody recognition, even compared to no training, which explains the marginal advantage of music training over the combined controls. A similar pattern emerged for musical ability. Although negative transfer is rare in music-training studies, including those with active and passive control groups (e.g., Nan et al., 2018; Nie et al., 2022), it has been reported in working-memory training (Ni et al., 2023) and cognitive control (Yanaoka et al., 2024). Perhaps the emphasis on rapid sensorimotor responses in basketball training predisposed children to prioritize speed during testing, which might have conferred advantages in tasks

Table 3
Descriptive Statistics for Musically Trained and Untrained Children in Study 2 (Unadjusted Ms and SDs) and p-Values, Effect Sizes (r), and Bayes Factors (BF_{10}) for Group Comparisons (Age Held Constant).

Variable	Trained		Untrained		p	r	BF_{10}
	M	SD	M	SD			
Prosody	71.90	11.94	59.50	14.95	.003	.212	11.9
Vocalizations	85.93	7.15	83.40	7.79	.912	.008	.218
Faces	83.47	8.02	79.46	8.93	.429	.058	.284
Musical Ability	1.14	0.73	−0.26	0.86	<.001	.353	>100
Nonverbal Reasoning	21.17	3.24	22.68	4.69	.064	−.134	1.14
Short-Term Memory	7.33	1.43	5.68	1.15	<.001	.379	>100
Working Memory	4.56	1.61	2.96	1.05	<.001	.294	>100

Note. Negative values for r indicate that the trained group had a *lower* mean compared to the untrained group. The analyses of musical ability used a latent variable derived from the three MBEMA subtests.

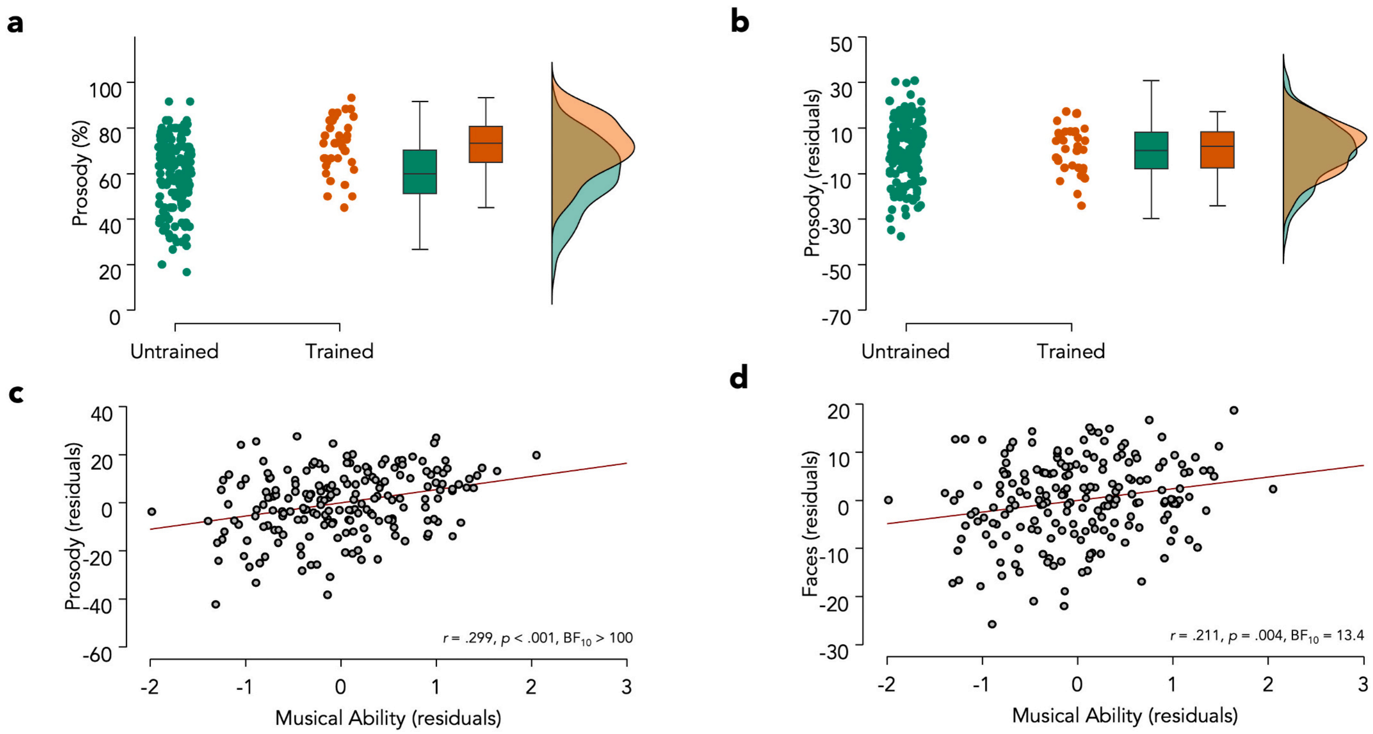

Fig. 3. Performance of Musically Untrained and Trained Children on the Prosody Task in Study 2 (a) Before and (b) After Adjusting for Other Variables. Associations Between Musical Ability and Emotion Recognition in (c) Prosody and (d) Faces After Adjusting for Other Variables. Note: (b), emotion recognition was adjusted for age, SES, other activities, musical ability, nonverbal reasoning, STM, and WM. In (c) and (d), musical ability and emotion recognition were adjusted for age, music training, SES, other activities, nonverbal reasoning, STM, and WM.

requiring quick responses (e.g., motor tasks), but disadvantages in tasks requiring careful perceptual processing, such as the emotion-recognition and music-related tasks. Future research could test this hypothesis. In any event, substantial evidence for no differences between the music and no-training groups highlights that music training did not confer benefits on prosody recognition.

Our null findings are consistent with results from previous small-scale longitudinal studies of emotion recognition among individuals with cochlear implants (Chari et al., 2020; Fuller et al., 2018; Good et al., 2017), but contrasting findings have also been reported. For example, Mualem and Lavidor (2015) found that 4 weeks of music training led to greater improvements than art training on a task that required participants to recognize emotions from prosody. The small sample ($n = 12$ per group) and explicit focus on improving emotion recognition in the music-training program raise doubts, however, about the reliability and generalizability of their findings.

Thompson et al. (2004) reported that children who received 1 year of keyboard lessons were better than children who had singing lessons or no lessons at discriminating anger from fear in prosody. The sample was again small ($n \leq 13$ per group), and the advantage did not extend to other emotions, namely happiness and sadness. Moreover, the benefits of keyboard lessons were not specific to music—drama lessons showed similar benefits—and sampling bias may have influenced the results, because only a minority of the children who started the training was tested. Indeed, if links between music training and emotion recognition are epiphenomenal, as our findings suggest, underpowered studies would be more likely to produce inconsistent or difficult-to-interpret results.

In Study 1, the relatively large sample size and long duration of training make it unlikely that the null results were Type II errors. Although the training was interrupted twice because of Covid-19, we found no significant differences between groups in how these disruptions affected the children. Moreover, developmental improvements were observed across all measures, with group-specific improvements for several of them.

In Study 2, the association between music training and recognizing emotions from prosody disappeared after adjusting for SES alone, as well as after adjusting for musical ability or STM, but it remained evident after adjusting for nonverbal reasoning, WM, or other activities. These findings in children parallel those from previous studies of adults, when the association between music training and emotion recognition in prosody persisted after accounting for nonverbal reasoning (Lima & Castro, 2011), but disappeared after adjusting for STM and musical abilities (Correia et al., 2022). Because both STM and music perception rely on auditory cognition, individual differences in auditory skills may explain musicians' advantages in recognizing prosodic emotions. This indirect association could also help to explain previous failures to find a link between music training and emotion recognition (e.g., Park et al., 2015; Trimmer & Cuddy, 2008). Our suggestion of a role for basic auditory skills does not rule out potential training effects—music training could improve emotion recognition in prosody through improvements in auditory processing (e.g., Patel, 2014). The role of SES is more consistent with reverse causality, however. Children from higher-SES backgrounds may be more likely to take music lessons (Corrigall et al., 2013) and to recognize emotions accurately.

Unlike music training, musical ability was a strong predictor of emotion recognition. Children with higher levels of musical ability performed better at recognizing emotions in both prosody and faces, with the strongest associations observed for prosody. This pattern was consistent across children with (Study 2) or without (Study 1) music training. At the opposite end of the spectrum, individuals with amusia, who have atypically low musical abilities, also have difficulty recognizing emotions from prosody and faces (Lima et al., 2016; Thompson et al., 2012), although their processing of musical emotions can be less affected (Gosselin et al., 2015; Peretz et al., 1998).

The positive association between musicality and emotion recognition provides empirical support for the proposal that shared processing mechanisms across domains explain the close link between music and social cognition (e.g., Clark et al., 2015). Neuroimaging findings from adults (Escoffier et al., 2013; Park et al., 2015; Van't Hooft et al., 2021) indicate that these mechanisms may extend beyond auditory processing. It remains uncertain, however, whether the link between musical ability and emotion recognition is general or auditory-specific. For example, adults' musical abilities are associated with recognizing emotions from voices but not from faces (Correia et al., 2022). In the current study of children, the association with faces was significant only for fear, perhaps because age played a moderating role.

Considered jointly, our longitudinal and correlational findings suggest that preexisting musical abilities, possibly along with other factors such as SES, account for observed associations between music training and emotion recognition. Musical abilities have a well-established genetic component (Wesseldijk et al., 2023), and children who are naturally more musical are more likely to take music lessons (Kragness et al., 2021). These children may also perform better on emotion-recognition tasks. Although it is possible that music training enhances this association, as Mankel and Bidelman (2018) proposed for speech-in-noise perception, the findings from Study 1 provided no support for a causal effect. Moreover, a recent meta-analysis concluded that musical abilities are better than music training at predicting prosody perception, whether prosody's function is linguistic or emotional (Jansen et al., 2023).

For tests of musical ability, initial analyses in Study 1 suggested that music training may cause small improvements. Follow-up results revealed, however, that the observed difference was driven by unexpectedly smaller improvements in the basketball group; the music training group improved similarly to the no-training group. A null association between music training and musical ability might seem counterintuitive, yet our music intervention was not designed to improve the specific skills measured by the ability tests, such that any improvement would reflect some form of transfer. In one previous study of children, 6 months of music training led to greater improvement in rhythm abilities compared to controls, but the groups did not differ at post-test (Martins et al., 2023). In another study, self-selected music training over a 5-year period did not predict improvements on same/different music-discrimination tasks (Kragness et al., 2021). If self-selected training does not improve musical ability, such improvement would be even less likely when motivation is lower due to random assignment, as in the current study. Overall, the strong association between music training and musical ability observed in Study 2 is consistent with the idea that musical ability predicts training.

For gross-motor skills, improvements in Study 1 were greater for the music compared to the no-training group, yet the music and basketball groups were similarly advantaged, presumably a consequence of participating in organized physical activities. A music-specific advantage emerged for fine-motor skills, replicating findings from an earlier study that showed a similar fine-motor advantage for music training over basketball or no training (Martins et al., 2018). The Orff-based teaching method likely contributed to this effect, both in the current and in the previous study (Martins et al., 2018), because of its emphasis on motor control and performance, which required children to practice precise movements involved in playing percussion instruments. Future studies could examine whether other types of training, such as singing, lead to similar results.

For nonverbal reasoning, executive functions, and broader socio-emotional functioning, there was no evidence that music training improved performance, in line with the view that far transfer is rare (Gobet & Sala, 2023; Sala & Gobet, 2020; Schellenberg & Lima, 2024), but in contrast to meta-analyses that reported positive effects of music training (Bigand & Tillmann, 2022; Jamey et al., 2024; Román-Caballero et al., 2022). Robust improvements were nevertheless observed for STM and WM. Music training is often associated positively with memory in general (Talamini et al., 2017), but such associations do not imply causation. With some exceptions (e.g., Nie et al., 2022), previous longitudinal studies with children reported null effects of music training compared to active control groups on auditory STM or WM (D'Souza & Wiseheart, 2018; Kosokabe et al., 2021; Nan et al., 2018). Particularly strong associations between musical ability and auditory STM and WM (Swaminathan & Schellenberg, 2018, 2020) may stem, at least in part, from task similarity.

Although we did not predict that music training would improve STM and WM, the robustness of the findings is difficult to attribute to random error. In our test battery, Digit Span—used to measure both STM and WM—was the only test with direct, face-to-face interaction between experimenter and child. The experimenters presented stimuli by reading numbers aloud, which children repeated orally. Because the experimenters were aware of group assignment, we cannot exclude the possibility that expectancy effects influenced the results. This aspect of the procedure may also explain why the music group showed larger increases in memory for digits but not for tunes from the MBEMA, which were presented via headphones. Alternatively, improved auditory memory in the music group may represent a real case of transfer. Auditory STM and WM were integral to many activities in the music-training program, such as imitating rhythmic and melodic patterns. Over time, repeated practice could have generalized to nonmusical tasks (e.g., remembering digits). In any event, links between music training and STM or WM are an active area of research across different labs (e.g., Grassi et al., 2023), which will eventually clarify the nature of the associations.

The present studies have several limitations. Although questionnaires completed by the teachers suggested that the impact of Covid-19 lockdowns was similar across groups, we cannot rule out the possibility that the interruptions affected the results. For example, unexpected findings, such as the smaller improvements in prosody and musical ability observed in the basketball group, might have differed had the training proceeded for the full planned duration. Test-retest correlations between scores at T1 and T2 were relatively low for some measures, including executive functions and faces (Table S3). Modest test-rest reliability is common among children (e.g., Van der Ven et al., 2013), however, and training effects appeared to be unrelated to these correlations. For example, a robust effect was observed for WM despite relatively low reliability ($r = .389$). In any event, future longitudinal research on music training could examine effects of measurement noise over time. Additionally, as with most research on transfer, we did not directly measure skills targeted by the interventions, and our assessment of musical abilities could have been more comprehensive by including production-based tasks such as finger tapping and sensorimotor synchronization (Dalla Bella et al., 2017). While group-specific improvements in STM, WM, and motor skills point to the intervention's efficacy, future studies could identify the specific skills trained by the programs and assess how they change over time. Such targeted assessments would further validate the intervention and help to identify the components responsible for any observed transfer effects.

Future studies could also compare different types of music training to examine generalizability. In Study 1, we used Orff-based training, whereas in Study 2 and virtually all cross-sectional comparisons, music training varied across individuals, precluding direct comparisons between pedagogical approaches. Moreover, we tested children's ability to recognize emotions with stimuli produced by adults, exclusively by female adults in the case of prosody. Future research could use a more diverse range of stimuli, including those produced by children. At present, the available data show that school-age children recognize adult expressions with above-chance accuracy (e.g., Amorim et al., 2021; Correia et al., 2019), and that factors such as the speaker's age or sex have minor effects on recognition (Amorim et al., 2021).

In summary, our findings suggest that preexisting factors, rather than plasticity and far transfer, are the main explanation for observed associations between music training and emotion recognition. This conclusion, combined with the lack of training effects for several other measures, is consistent with the general rarity of far transfer (Gobet & Sala, 2023) and well-established genetic influences on musical abilities and behaviors (Wesseldijk et al., 2023). At the same time, improvements in fine-motor skills and auditory STM and WM highlight the possibility that transfer may occur in specific domains and contexts. To address ongoing controversies about potential training effects, we recommend further longitudinal studies, combined with a systematic examination of the nonmusical correlates of musical abilities. In the meantime, preexisting associations between music and nonmusical abilities represent the simplest explanation for the many advantages exhibited by musicians.

CRediT authorship contribution statement

Leonor Neves: Writing – original draft, Methodology, Investigation, Data curation. **Marta Martins:** Writing – original draft, Project administration, Methodology, Investigation, Data curation. **Ana Isabel Correia:** Writing – review & editing, Investigation, Data curation. **São Luís Castro:** Writing – review & editing, Project administration, Funding acquisition. **E. Glenn Schellenberg:** Writing – review & editing, Writing – original draft, Supervision, Formal analysis. **César F. Lima:** Writing – review & editing, Writing – original draft, Supervision, Resources, Project administration, Funding acquisition, Conceptualization.

Data availability

We have shared the link to our data

Acknowledgments

This work was funded by the Portuguese Foundation for Science and Technology (FCT) through a PhD studentship awarded to L.N. (SFRH/BD/135604/2018), a Scientific Employment Stimulus grant awarded to E.G.S. (CEECIND/03266/2018), Centro de Psicologia da Universidade do Porto (CPUP UIBD/00050/2020), and a project grant awarded to C.F. L. (PTDC/PSI-GER/28274/2017), co-funded by the European Regional Development Fund (ERDF) through the Lisbon Regional Operational Program (LISBOA-01-0145-FEDER-028274) and the Operational Program for Competitiveness and Internationalization (POCI-01-0145-FEDER-028274).

We thank the school administrators, teachers, parents, and all the children who took part in the studies.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.cognition.2025.106102.

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