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Does music training enhance auditory and linguistic processing? A systematic review and meta-analysis of behavioral and brain evidence

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ABSTRACT

It is often claimed that music training improves auditory and linguistic skills. Results of individual studies are mixed, however, and most evidence is correlational, precluding inferences of causation. Here, we evaluated data from 62 longitudinal studies that examined whether music training programs affect behavioral and brain measures of auditory and linguistic processing ($N = 3928$). For the behavioral data, a multivariate meta-analysis revealed a small positive effect of music training on both auditory and linguistic measures, regardless of the type of assignment (random vs. non-random), training (instrumental vs. non-instrumental), and control group (active vs. passive). The trim-and-fill method provided suggestive evidence of publication bias, but meta-regression methods (PET-PEESE) did not. For the brain data, a narrative synthesis also documented benefits of music training, namely for measures of auditory processing and for measures of speech and prosody processing. Thus, the available literature provides evidence that music training produces small neurobehavioral enhancements in auditory and linguistic processing, although future studies are needed to confirm that such enhancements are not due to publication bias.

1. Introduction

Understanding how experience changes our brain and behavior is a fundamental question in cognitive neuroscience. This phenomenon is referred to as *plasticity*, and research on this topic often focus on individuals with training on specific domains, such as juggling (Draganski et al., 2004), spatial navigation (e.g., Woollett and Maguire, 2011), and bilingualism (e.g., Van de Putte et al., 2018). Over the past two decades, there has been a widespread interest in the idea that music training might be a useful framework for studying brain and behavioral plasticity (e.g., Herholz and Zatorre, 2012; Moreno and Bidelman, 2014; Münte et al., 2002; Wan and Schlaug, 2010). This idea remains contentious, though (Sala and Gobet, 2020; Swaminathan and Schellenberg, 2021).

Many correlational studies report differences between musicians and musically untrained individuals in brain structure and function (e.g., Bianchi et al., 2017; Gaser and Schlaug, 2003; Krause et al., 2010; Magne et al., 2006), and associations between music training and enhanced performance in abilities such as executive functioning (e.g., Zuk et al., 2014), speech-in-noise perception (e.g., Parbery-Clark et al., 2009), and emotional prosody recognition (e.g., Lima and Castro, 2011). It is typically presumed that the benefits are *caused* by musical experience (Schellenberg, 2020a), and therefore reflect plasticity, but correlational designs cannot exclude the possibility that the benefits are the cause rather than the consequence of training. This possibility is plausible because musically trained and untrained individuals differ in many ways in addition to training. Pre-existing cognitive, personality and socioeconomic factors might determine who takes music lessons (Schellenberg, 2020b), and twin studies show that genetic factors account for many aspects of musical behavior and achievement, including propensity for music practice, musical abilities, choice of musical instrument and genre, and associations between music practice and musical abilities (McPherson, 2016; Mosing et al., 2014; Mosing and Ullén, 2018; Ullén et al., 2016).

A growing number of studies implement longitudinal designs to address the issue of causality. Participants are assessed before and after a music training program, and compared to a control group that either

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does nothing – passive control (e.g., Hyde et al., 2009; James et al., 2020) – or takes part in a different form of training such as painting – active control (e.g., Martins et al., 2018; Moreno et al., 2009). Active control groups and random assignment to the groups allow for stronger inferences of causality (Schellenberg, 2020b). Active control groups minimize the possibility that music-related benefits stem from nonmusical aspects of the training (e.g., time spent in a learning environment), and random assignment minimizes self-selection effects (e.g., pre-existing motivational differences). Design features vary across studies, but a commonly asked question is whether music training produces *transfer* effects, i.e., has consequences that generalize beyond the trained skills. Due to potential theoretical and practical implications, there is particular excitement about the possibility that music promotes transfer of skills to substantially different nonmusical domains, such as mathematics, IQ, or language. Transfer to domains like these is called *far* transfer (Barnett and Ceci, 2002), and whether it exists is an ongoing debate (e.g., Bigand and Tillmann, 2022; Sala and Gobet, 2017a; Sala and Gobet, 2017b; Swaminathan and Schellenberg, 2021). Transfer to domains tightly related to music is called *near* transfer.

The processing of fine-grained acoustic features of sounds is a near transfer domain of music training (e.g., Bigand and Tillmann, 2022; Kraus and Chandrasekaran, 2010). Auditory skills are critical for music, and music training requires high precision in the processing of subtle acoustic differences, for instance in pitch or timing, which can be present in a range of sounds, from single-frequency tones to complex ones such as melodic or rhythmic patterns. There is evidence of cortical and subcortical plasticity in the auditory pathway (e.g., Herholz and Zatorre, 2012; Pantev and Herholz, 2011), and this plasticity can relate to improved auditory and musical abilities (e.g., Habibi et al., 2016; Hyde et al., 2009). In a study with children, however, Kragness and colleagues (2021) found that individual differences in music discrimination are stable over time, and when prior performance is held constant (measured five years earlier), the association between music training and music discrimination disappears. Even for near transfer domains, music training effects can therefore be weak.

Language is one of the far transfer domains most extensively examined in the music training literature. Many studies examine transfer to linguistic abilities including phonological awareness (e.g., Vidal et al., 2020), reading (e.g., Carioti et al., 2019), speech-in-noise perception (e.g., Hennessy et al., 2021), speech-in-quiet perception (e.g., Tierney et al., 2015), or prosody perception (e.g., Moreno et al., 2009). Although results from individual studies vary (e.g., Boebinger et al., 2015; Mehr et al., 2013), the mechanisms underlying associations between music and linguistic processing have been discussed. Both music and language are forms of human communication, rely on auditory learning and on a hierarchical organization of elements (e.g., from sounds/phonemes to melodies/sentences), and share auditory pathways (e.g., Peretz et al., 2015; Tervaniemi et al., 2022; Zatorre et al., 2002). According to the 'OPERA' hypothesis (Patel, 2011, 2012, 2014), music training induces plasticity in speech and language networks when five conditions are met: music engages sensory and cognitive networks that Overlap with those engaged by speech (e.g., encoding of periodicity; auditory working memory); music places higher demands on these networks than speech, requiring more Precision of processing; and musical activities occur in a context that involves positive Emotion, extensive Repetition, and focused Attention. In short, music training would enhance speech and language processing because it places higher demands on shared neural networks, elicits emotional rewards, and requires repetition and attention.

Several meta-analyses examine longitudinal evidence for music training effects, all focused on far transfer and behavioral measures (Cooper, 2020; Gordon et al., 2015; Román-Caballero et al., 2018, 2022; Sala and Gobet, 2017a, 2020; Vaughn, 2000). The emphasis is on general cognitive and academic skills, such as IQ and mathematics, and results reveal a small positive effect. The effect is heterogeneous across individual studies, however, and potentially related to the study design. For instance, Gordon et al. (2015) reviewed 13 studies ($N = 901$) assessing music training effects on phonological awareness and reading fluency. There was a small effect of training on phonological awareness ($d = 0.20$), which was larger when the training was longer. The effects on reading fluency were not significant. More recently, Cooper (2020) reviewed 21 studies ($N = 5612$) and found an overall significant effect of $g = 0.28$ for measures of verbal and nonverbal cognitive abilities. The effect remained significant for studies with active control groups, but only when they were conducted in a natural setting (e.g., a classroom). Another meta-analysis, by Sala and Gobet (2020), reviewed 54 studies ($N = 6984$) focusing on transfer to cognitive and academic skills, in an update of a previous meta-analysis on the same topic (Sala and Gobet, 2017a). The new analysis revealed a small significant effect of music training ($g = 0.18$), consistent with the previous one, but also heterogeneity across studies. The effect was observed for studies with passive control groups, but not for those with active control groups. Moreover, for the studies with passive control groups the effect was only found when assignment was not random. Thus, when design quality was optimal, including active control groups and random assignment, the benefits of music training were null. However, a reanalysis of Sala and Gobet's data indicated that randomization was not a robust moderator, and that there would be evidence for transfer if near-transfer effect sizes had been excluded in the control groups, as they were in the music groups (e.g., phonological awareness when the group received phonological training; Bigand and Tillmann, 2022). Sala and Gobet's findings were also not replicated in the meta-analysis by Román-Caballero et al. (2022), which revealed significant music training effects on children's cognitive and academic abilities, regardless of randomization and type of control group ($\bar{g}_{\Delta} = .26$; 32 studies, 34 independent samples, $N = 5998$). Only studies that involved learning how to play a complex instrument were included, though. It could be that a more demanding training produces larger effects, and that inconsistencies across meta-analyses result from not accounting for the type of music training. Whether music training enhances nonmusical abilities remains unclear, as does the role of study design features.

Two other aspects remain poorly explored. Despite the increasing number of studies of music training effects on brain structure and function, particularly regarding linguistic processing (e.g., Carpentier et al., 2016; Fleming et al., 2019; Hennessy et al., 2021), no systematic reviews have covered brain data. This will be crucial to understand behavior in the context of brain plasticity, and the neurobiological bases of associations between music and nonmusical domains. Moreover, because the primary focus has been on far transfer, meta-analytic evidence for near transfer remains unexplored, and this is crucial for a mechanistic understanding of plasticity and transfer effects. For example, existing hypotheses suggest that sharper auditory processing is required to explain far transfer from music to language (e.g., Besson et al., 2011; Goswami, 2011; Patel, 2014).

The present review and meta-analysis examines the neurobehavioral effects of music training in healthy individuals, focusing on auditory processing (near transfer) and linguistic processing (far transfer). Examining near transfer is necessary to inform theories of plasticity and transfer, and although previous meta-analyses explored far transfer to general cognitive abilities, a comprehensive analysis of effects on linguistic skills is lacking. Because language is extensively examined in music training studies, evaluating this domain will illuminate debates on far transfer, both from behavioral and brain perspectives. Sixty-two longitudinal studies were included, and we asked whether music training effects are observed at the behavioral and brain levels. Behavioral data were summarized through multivariate meta-analytic models and brain data through a narrative synthesis. In the meta-analysis, we also asked whether training effects depend on the outcome measure (auditory vs. linguistic skills), type of music training (instrumental vs. non-instrumental), participants' age, publication year, aspects of the study design (type of control group, randomization, risk of bias), aspects of the training programs (total months of training, hours per week), and baseline differences.

2. Methods

We followed the PRISMA guidelines for systematic reviews and meta-analyses (Liberati et al., 2009). The PRISMA checklist is presented in Table S1 (supplementary material), and Fig. 1 depicts a PRISMA flowchart. The protocol for this review was registered on PROSPERO (CRD42020201243).

2.1. Literature search

The first search was conducted in July 2019, using the Web of Science Core Collection, EBSCOhost, Scopus, and PubMed databases to identify longitudinal studies examining effects of music training on auditory and linguistic processing in healthy individuals. We used the query: "music training" OR "music practice" OR "music intervention" OR "music lesson* " OR "music instruction" OR "music program* " OR "music group". This query was adapted according to the specifications of each database (Table S2). By relying on several databases and on a broad query, we aimed to minimize search bias and avoid missing relevant studies, such as those that included linguistic and auditory processing outcomes but had a distinct primary focus (e.g., studies focused on IQ, Schellenberg, 2004; or mathematics, Holmes and Hallam, 2017). Two additional search rounds were conducted, in June 2020 and June 2021, to identify more recent eligible articles. Table S3 presents the total number of studies identified in each database and in each of the searching dates. We also screened the reference lists of the included studies and reviews on the topic to identify additional studies that might have not been captured by our search.

2.2. Selection criteria

Studies met the following criteria to be selected: written in English and published in a peer-reviewed journal; full-text available; sample of healthy individuals; longitudinal design; inclusion of a music training group and at least one control group (passive, active or both); and at least one measure of auditory and/or linguistic processing.

Reasons for exclusion: review articles; studies comparing professional musicians with untrained participants (i.e., correlational studies); lack of pre-training and/or post-training data; and studies with clinical populations (e.g., amusia; cochlear implant users).

Titles and abstracts were independently screened by two reviewers (L.N. and A.I.C.) for eligibility using *Rayyan* (Ouzzani et al., 2016). The same process was repeated for full-texts of all potentially eligible studies, where eligibility was assessed against inclusion criteria (reasons for exclusion are detailed in Table S4). Discrepancies were adjudicated by a third reviewer. We assessed inter-rater reliability (IRR) for the initial and full-text screening phases using Cohen's Kappa. IRR ranged from moderate (Cohen's K, 1st screening = 0.59) to substantial agreement (Cohen's K, 2nd screening = 0.73; Cohen's K, 3rd screening = 0.66) in the initial screenings. The IRR was almost perfect in the full-text screenings (Cohen's K, 1st screening = 0.85; Cohen's K, 2nd screening = 0.84; Cohen's K, 3rd screening = 0.85; Table S5; Landis and Koch, 1977).

2.3. Data extraction

The two reviewers who screened the studies for eligibility also independently extracted the following information from each study: authors, title, year, journal, participants' age, design and methodology (i.e., groups, randomization process, music training method [e.g., Suzuki], total months of training, and hours of training per week), type of measurement (i.e., auditory and/or linguistic), means and standard deviations for performance on each task per group (before and after training), and information to assess risk of bias, as specified below (Section 2.4). For studies that included brain outcomes, they additionally extracted information on the measure (e.g., EEG; MRI) and main findings.

When relevant data were missing, we contacted the authors by email

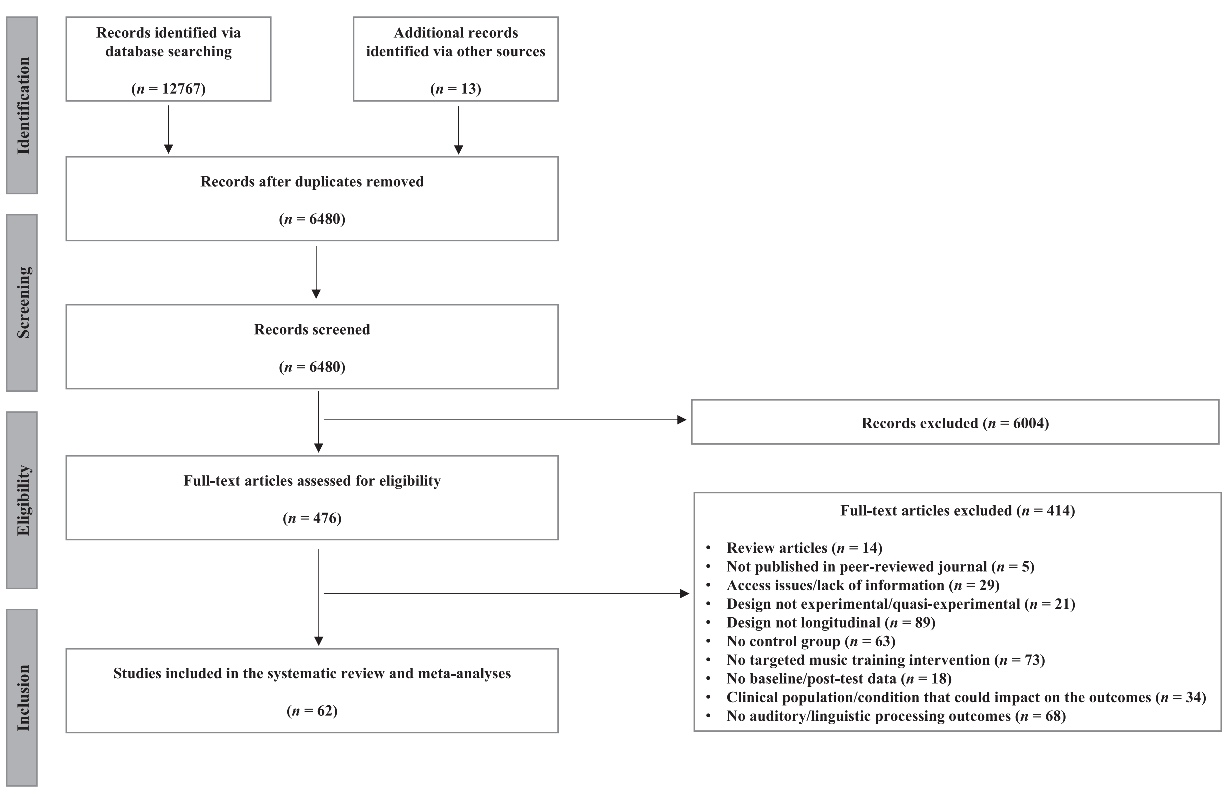

Fig. 1. Flowchart showing the process of selection of studies for the systematic review and meta-analysis according to the PRISMA guidelines.

($n = 24$). Eight replied and provided the requested data. In case they could not provide exact means and standard deviations but graphic information was available ($n = 4$), we estimated the values from the graphs using the software WebPlotDigitizer (Rohatgi, 2020). When the required data were neither available nor could be obtained from the authors, the study was either excluded ($n = 7$), or kept if it provided useful information (e.g., relevant data could be missing for behavioral measures, but not for brain measures; $n = 4$).

2.4. Quality assessment

We used the revised Cochrane Risk of Bias tool (RoB 2) to assess the risk of bias in each of the included studies (Higgins et al., 2011). We judged whether each study had a high risk of bias, low risk of bias, or some concerns regarding the following domains: randomization process, deviations from intended intervention, missing outcome data, measurement of the outcome, and selection of the reported results. The overall risk of bias of a given study was considered low if all the domains were rated as low risk, or if only one was rated as "some concerns" and the reviewers did not consider it worrisome. If the studies did not meet criteria for low risk, and no more than three domains were rated as "some concerns", the risk of bias was classified as "some concerns". The other studies were considered to have a high risk of bias. The risk of bias was assessed independently by two reviewers and any disparity was resolved by consensus. The evaluations were based on information provided in the article and in supplementary material. No study was discarded because of risk of bias.

2.5. Data synthesis

2.5.1. Meta-analysis of behavioral data

2.5.1.1. Calculation of effect sizes and respective variance. To estimate the effects of music training on behavioral measures, we used the formula proposed by Morris (2008) for standardized mean change difference: Hedges ́g (hereafter referred to as g_{Δ}). This allows not only to compare music training and control groups, but also to control for possible differences in the pre-training values. The formula is:

$$g_{\Delta} = J \times d \tag{1}$$

where

$$d = \frac{(M_{\text{post},m} - M_{\text{pre},m}) - (M_{\text{post},c} - M_{\text{pre},c})}{SD_{\text{pooled, pre}}} \tag{2}$$

The indices M_{post} and M_{pre} indicate the scores for different measurement times (e.g., pre- and post-training), for the music group (m) and control group (c). $SD_{\text{pooled, pre}}$ is the pooled standard deviation for the pre-training scores of both groups. The correction factor to achieve an unbiased estimator is defined as:

$$J = 1 - \frac{3}{4 \times (N_m + N_c) - 9} \tag{3}$$

The indices N_m and N_c are the number of participants in the music and control groups. Positive g_{Δ} indicates improvement from pre- to post-training in the music group compared to control group. The variance of g_{Δ} was calculated following the formula by Borenstein et al. (2009):

$$V_{g\Delta} = \left(\frac{N_m + N_c}{N_m \times N_c} + \frac{d^2}{2 \times (N_m + N_c)}\right) \times J^2 \tag{4}$$

We also calculated the traditional Hedges ́g only with pretest scores (hereafter referred to as g_{pre}), to compare the performance of music and control groups at baseline:

$$g_{\text{pre}} = J \times \frac{M_{\text{pre},m} - M_{\text{pre},c}}{SD_{\text{pooled, pre}}} \tag{5}$$

$$V_{g_{\text{pre}}} = \left(\frac{N_m + N_c}{N_m \times N_c} + \frac{g_{\text{pre}}^2}{2 \times (N_m + N_c)}\right) \times J^2 \tag{6}$$

2.5.1.2. Meta-analysis. The meta-analysis was conducted using the *"metafor"* package (version 2.0.0) from R (Viechtbauer, 2010). Because we frequently included more than one effect size coming from the same participants, a multilevel random-effects model was used to account for this dependency. Applying multivariate meta-analytic models can be challenging when the covariance structure is unknown and cannot be estimated based on previous literature, which was our case. To overcome this, we estimated the variance-covariance matrix from the data using the *"clubSandwich"* package from R (version 0.5.0).

2.5.1.3. Heterogeneity. Because studies differ in many respects, including experimental design, sample size, measures, and training schemes, it is likely that there is heterogeneity in the obtained effects (Xu et al., 2008). Statistical heterogeneity occurs when the true effects of the different studies show larger variation than expected due to random error or by chance. Assessing heterogeneity is therefore important for better evaluating the conclusions that can be drawn from a meta-analysis. We assessed between-studies heterogeneity using the Cochran's Q test (Kulinskaya and Dollinger, 2015) and the I^2 statistics (Higgins and Thompson, 2002; Higgins et al., 2003).

2.5.1.4. Influential studies and leave-one-out robustness analysis. We assessed the presence of influential studies by calculating Cook's distances. A conservative approach was adopted, considering as influential any study with a Cook's distance greater than three times the mean (Cook, 1977). To assess the robustness of our findings (i.e., to exclude the possibility that our results were driven by one specific study), we also repeated the meta-analysis excluding one study at a time.

2.5.1.5. Moderators. Meta-regression models were used to evaluate the potential influence of ten moderators on the behavioral outcomes:

(1) Domain of outcome measure: auditory or linguistic processing (dichotomous variable). This moderator tested whether the magnitude of transfer effects differed for near transfer (auditory processing) vs. far transfer (linguistic processing) domains.
(2) Type of training: instrumental or non-instrumental (dichotomous variable). This moderator accounted for the diversity of music training programs across studies, considering evidence that effects might be larger when the training involves learning how to play a complex musical instrument compared to other types of training (e.g., programs of music education such as Orff, listening programs, or computerized training of musical skills; Román-Caballero et al., 2022). We followed the same classification criteria as Román-Caballero et al. (2022).
(3) Baseline differences: measured as g_{pre} (continuous variable). This moderator asked whether between-group differences before training determined the magnitude of training effects. Previous studies raise the possibility that baseline differences determine the likelihood of taking music lessons (e.g., Swaminathan et al., 2017), and this could be a concern particularly for studies with non-randomized group assignment. Recent meta-analyses examined this moderator also to account for potential regression toward the mean in participants who had more extreme differences before training (Román-Caballero et al., 2022; Sala and Gobet, 2020).
(4) Publication year: published before 2000, between 2000 and 2009, or between 2010 and 2022. This variable was transformed into a categorical variable because the data was not uniformly distributed over time (95.16% of the studies were published after

2000). This moderator examined temporal trends in the magnitude of the reported effects.

(5) Age: mean age of the participants – less than 11 years old (children), between 11 and 17 years (adolescents), between 18 and 59 years (adults), and ≥ 60 years (older adults). Age was transformed into a categorical variable because the data was not uniformly distributed over the range of ages (70.97% of the sample are children). The age at which music training begins might influence the magnitude of the effects (e.g., White et al., 2013).

(6) Randomization: randomized or non-randomized group assignment (dichotomous variable). Random assignment is an important methodological aspect to establish causation, as it prevents self-selection effects, thereby minimizing the effects of potential pre-existing differences between groups (e.g., Ilari, 2020; Schellenberg, 2020a).

(7) Type of control group(s): active, i.e., another type of intervention (e.g., sports), or passive, i.e., no intervention (dichotomous variable). This moderator controlled for the possibility that the benefits of music training result from nonmusical aspects of the training.

(8) Duration of training: number of months (continuous variable). The length of music training has been associated with the level of proficiency achieved (e.g., Wilson et al., 2011).

(9) Hours of training per week (continuous variable). Similarly, the frequency of training can be associated with the magnitude of the effects (e.g., Kraus et al., 2014).

(10) Risk of bias: low risk, some concerns or high risk of bias (categorical variable). This moderator reflects the extent to which methodological flaws might have affected the results (Higgins et al., 2011).

2.5.1.6. Publication bias. In addition to the methods-related risk of bias, the risk of publication bias is an important issue to consider. If effects that are "significant" and large, or consistent with the authors' expectations, are more likely to be published than those that are null or inconclusive, inferences from individual studies and meta-analyses will be biased (e.g., Francis, 2012; Van Aert et al., 2019). Publication bias can lead to exaggerated average effect sizes, which might appear significant and important when there is no underlying 'true' effect. We assessed the potential presence of publication bias, and corrected for its consequences, using the trim-and-fill method and meta-regression methods, namely the precision-effect test and precision-effect estimate with standard errors (PET-PEESE; Stanley and Doucouliagos, 2014). Trim-and-fill is a non-parametric method used to estimate the number of studies missing from a meta-analysis due to suppression of most extreme results on one side of the funnel plot. If missing studies are detected, this method augments the observed data to increase the symmetry of the funnel plot (Duval and Tweedie, 2000). This approach assumes independence of effect sizes, and it is therefore not compatible with data like ours where effect sizes cluster around the study from which they originated. To account for dependence, we estimated aggregated effect sizes for each study by generating average estimates using the *agg* function from the *MAd* package in R. PET-PEESE tests for selective reporting and adjusts for small-study effects using a measure of precision as a covariate in the meta-analytic model (standard error of the effect size in the case of PET, and sampling variance in the case of PEESE). The procedure involves first testing whether the PET estimate is significant, using PEESE if it is or PET otherwise. The regression coefficient tests for publication bias, and the intercept of the model indicates the average effect size estimate from a study with zero sampling variance, taken as a 'bias-corrected' or true average effect.

The usual estimator of the sampling variance of the standardized mean differences includes the effect size itself in the formula. This is problematic when using PET-PEESE, as these test for the independence between d and Vg_{Δ}, and the fact that Vg_{Δ} is calculated from d generates an artefactual correlation among them. To overcome this, we followed Pustejovsky and Rodgers (2019) recommendation and modified the conventional variance formula so that it does not rely on the effect size for the estimation. As an alternative to d, we calculated h, whose variance does not involve the effect size:

$$h = \sqrt{2} \times \text{sign}(g_{\Delta}) \times \left[ln\left(|g_{\Delta}| + \sqrt{g_{\Delta}^2 + a^2}\right) - ln(a) \right] \tag{7}$$

where,

$$a = \sqrt{2 \times \frac{N_m + N_c}{N_m \times N_c} \times (N_m + N_c - 2)} \tag{8}$$

And the sampling variance of the estimate is calculated as:

$$V_h = \frac{1}{N_m + N_c - 2} \tag{9}$$

2.5.2. Brain outcomes (narrative synthesis)

Studies on brain outcomes would hardly allow for a quantitative synthesis because of their heterogeneity (e.g., functional versus structural outcomes; magnetic resonance imaging versus electrophysiological measures; task-based versus resting-state measures). We summarized these findings using narrative synthesis. Section 3.4., Table 3 and Fig. 4 summarize the characteristics of the brain studies and their main findings.

3. Results

3.1. Overview

Table 1 presents an overview of all included studies. We reviewed 62 studies, published between 1974 and 2022. Forty-four of them reported effects of music training on behavioral measures and 27 on brain measures (nine report both behavioral and brain findings). Nineteen studies reported effects on auditory processing, 34 on linguistic processing, and nine on both. Forty-four included a passive control group, 32 an active control group, and 14 included both. Sixteen studies had random assignment and 46 did not. Twenty-six studies had instrumental training programs, and 36 were non-instrumental.

The omnibus sample size was 3928 participants (M = 63.35 per study, SD = 53.16, range = 12–345). They were distributed across a range of ages: 3034 were children (M_{age} = 6.63 years, SD = 1.61, range = 3.60 – 10.30), 326 adolescents (M_{age} = 12.56, SD = 1.75, range = 10.80 – 14.69), 269 adults (M_{age} = 28.56, SD = 14.59, range = 20.90 – 58.29), and 331 older adults (M_{age} = 67.25, SD = 1.86, range = 63.50 – 68.45). From the total sample, 1845 participants were assigned to music training groups (M = 29.76 per study, SD = 27.07, range = 6 – 192), 1244 to passive control groups (M = 28.27 per study, SD = 18.03, range = 6 – 85), and 839 to active control groups (M = 26.22 per study, SD = 27.37, range = 6 – 153). The music training programs had a mean duration of 9.77 months (SD = 9.89, range = 0.66 – 48 months), and a mean frequency of 3.09 h per week (SD = 3.16, range = 0.50 – 15 h).

3.2. Quality assessment

Table S6 presents an overview of the studies' compliance with the Rob 2 criteria. Twenty-four studies had low risk of bias (38.71%), 18 raised some concerns (29.03%), and 20 had high risk of bias (32.26%). Thus, almost two-thirds of the studies (61.29%) had risk of bias. This was primarily because of the randomization process, a methodological concern for most studies. Forty-seven studies raised some concerns (29) or high risk of bias (18) regarding randomization, and only 15 had low risk.

Table 1
Overview of the studies included in the systematic review and meta-analysis (N = 62). Abbreviations: EEG – Electroencephalography; MEG – Magnetoencephalography; MRI - Magnetic Resonance Imaging; NR – not reported.

Study	Characteristics								Behavioral Measures		Brain Measures		
	N	Mean Age (years)	Groups (*n* per group)	Random Assignment	Training Duration (months)	Hours of Training (per week)	Type of Music Training	Instrumental Training*	Auditory	Language	MRI	EEG	MEG
Tervaniemi et al. (2022) (Cereb. Cortex)	85	9.3	Music (29) Language (38) Passive Control (18)	No	5.8	1.7	Kodály music theory and solfeggio					✓	
Hennessy et al. (2021) (Aging)	41	58.3	Music (18) Passive Control (23)	Yes	2.8	2	Group choir singing		✓	✓		✓	
Wiener and Bradley (2020) (Lang. Teach. Res.)	20	20.9	Music (10) Language (10)	No	1.8	3.5	Computer-based program (identifying structural elements of music, e.g., chords)		✓	✓			
Habibi et al. (2020) (Brain Struct. Funct.)	23	7	Music (12) Passive Control (11)	No	48	NR	Ensemble and group performances (string instruments)	✓			✓		
James et al. (2020) (Front. Neurosci.)	63	10.2	Music (31) Passive Control (32)	Yes	24	1.5	Orchestra in class (string instruments)	✓	✓				
Li et al. (2020) (IEEE Trans. Neural Syst. Rehabil. Eng.)	56	23.2	Music (29) Passive Control (27)	Yes	5.5	4.75	Piano training	✓			✓		
Vidal et al. (2020) (Appl. Psycholinguist. Psycho)	44	3.6	Music (23) Visual Arts (21)	Yes	6.9	0.75	Mixed music activities (e.g., joint singing and rhythm exercises)			✓			
Dubinsky et al. (2019) (Front. Neurosci.)	63	67.6	Music (34) Passive Control (29)	No	2.3	3	Choir singing (pitch and vocal training)		✓	✓		✓	
Bugos (2019) (Front. Integr. Neurosci.)	135	68.4	Music (49) Music (38) Passive Control (48)	No	3.7	3.75	Piano training; Percussion training	✓		✓			
Fleming et al. (2019) (Brain Cogn.)	33	67.8	Music (12) Video Games (8) Passive Control (13)	No	6	2.5	Piano training	✓			✓		
Zendel et al. (2019) (Neurobiol. Aging)	34	68.0	Music (13) Video Games (8) Passive Control (13)	Yes	6	2.5	Piano training r	✓		✓		✓	
Carioti et al. (2019) (Front. Psychol.)	74	11.4	Music (30) Passive Control (44)	No	12	4	Ensembles and individualized training (instrument of their choice)	✓		✓			
MacCutcheon et al. (2020) (Front. Psychol.)	41	6.3	Music (26) Sports (15)	No	8.7	0.75	Kodály and Orff			✓			
Cohrdes et al. (2019) (Psychol. Music)	202	5.4	Music (67) Language (68)	No	6	1.5	Fundamental music competencies (e.g., tonal discrimination)		✓				

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Table 1 (*continued*)

Study	Characteristics								Behavioral Measures		Brain Measures		
	N	Mean Age (years)	Groups (*n* per group)	Random Assignment	Training Duration (months)	Hours of Training (per week)	Type of Music Training	Instrumental Training*	Auditory	Language	MRI	EEG	MEG
			Passive Control (67)										
Li et al. (2019) (Brain Struct. Funct.)	56	23.2	Music (29) Passive Control (27)	Yes	5.5	4.75	Piano training	✓			✓		
Alain et al. (2019) (Front. Neurosci.)	53	68.2	Music (17) Visual Arts (19) Passive Control (17)	No	3	3	Mixed music activities and basic music theory (e.g., body percussion)			✓		✓	
Rose et al. (2019) (Psychol. Music)	38	7.8	Music (19) Passive Control (19)	No	12	3.33	Individual instrumental playing	✓	✓	✓			
Patscheke et al. (2019) (Psychol. Music)	40	5.5	Music-Pitch (13) Music-Rhythm (13) Sports (14)	Yes	3.68	1	Pitch training; Rhythm training			✓			
Jaschke et al. (2018) (Front. Neurosci.)	146	6.4	Music + (38) Music (42) Visual Arts (29) Passive Control (37)	No	30	1.5	Theoretical and active instrumental lessons			✓			
See and Ibbotson (2018) (Int. J. Educ. Res.)	56	4.5	Music (28) Passive Control (28)	Yes	2.3	1	Kodály approach			✓			
D'Souza & Wiseheart, 2018 (Arch. Sci. Psychol.)	75	7.8	Music (24) Dance (26) Passive Control (25)	No	0.7	10	Mixed music activities and instruments	✓		✓			
Nan et al. (2018) (Proc. Natl. Acad. Sci. U.S.A.)	74	4.6	Music (30) Reading (28) Passive Control (16)	No	6	2.25	Piano training	✓	✓	✓		✓	
Li et al. (2018) (Hum. Brain Mapp.)	56	23.2	Music (29) Passive Control (27)	Yes	5.5	4.75	Piano training	✓			✓		
Habibi et al. (2018) (Cereb. Cortex)	47	6.9	Music (15) Sports (15) Passive Control (17)	No	24	6.5	Ensemble and group performances (string instruments)	✓			✓		
Degé and Schwarzer (2018) (Music Sci.)	30	10.8	Music (13) Passive Control (17)	No	12	3	Mixed music activities and school choir/ orchestra		✓				
Guo et al. (2018) (Front. Psychol.)	40	7.5	Music (20) Passive Control (20)	No	1.4	0.83	Keyboard harmonica instruction	✓		✓			
Fujioka and Ross (2017) (Eur. J. Neurosci.)	14	63.5	Music (7) Passive Control (7)	No	1.1	3	Piano training	✓					✓
	59	5.5		No	12	0.5	Rhythmic instruction			✓			

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Table 1 (*continued*)

Study	Characteristics								Behavioral Measures		Brain Measures		
	N	Mean Age (years)	Groups (*n* per group)	Random Assignment	Training Duration (months)	Hours of Training (per week)	Type of Music Training	Instrumental Training*	Auditory	Language	MRI	EEG	MEG
Holmes and Hallam (2017) (London Rev. Educ.)	61	4.5	Music (29) Passive Control (30) Music (31) Passive Control (30)	No	12	0.5	Rhythmic instruction			✓			
Habibi et al. (2016) (Dev. Cogn. Neurosci.)	37	6.9	Music (13) Sports (11) Passive Control (13)	No	24	6.5	Ensemble and group performance (string instruments)	✓				✓	
Carpentier et al. (2016) (J. Cogn. Neurosci.)	30	5.6	Music (14) French (36)	No	0.7	10	Computer-based program (rhythm, pitch, melody, voice, and basic musical concepts)					✓	
Janus et al. (2016) (J. Exp. Child. Psychol.)	57	5.5	Music (29) French (28)	No	0.7	15	Computer-based program (rhythm, pitch, melody, voice, and basic musical concepts)			✓			
Ilari et al. (2016) (Front. Psychol.)	50	6.8	Music (23) Passive Control (27)	No	12	7	Ensemble practice and group performances (e.g.,violin, choir), musicianship, theory skills	✓	✓				
Schellenberg et al. (2015) (PLoS One)	84	8.7	Music (38) Passive Control (46)	No	10	0.67	Kodály method – ukulele in the classroom	✓		✓			
Tierney et al. (2015) (Proc. Natl. Acad. Sci. U.S.A.)	40	14.7	Music (19) Fitness (21)	No	36	2.67	Learning to play in a large ensemble (e.g., percussion, trumpet)	✓		✓		✓	
Moreno et al. (2015) (Child Dev.)	36	5.6	Music (18) French (18)	No	0.7	10	Computer-based program (rhythm, pitch, melody, voice, and basic musical concepts)					✓	
Rautenberg (2015) (J. Res. Read)	159	7.8	Music (33) Visual Arts (41) Passive Control (85)	No	8	NR	Gordon's learning theory of music (rhythmic and tonal skills training, auditory discrimination of timbre and sound intensity)			✓			
Slater et al. (2015) (Behav. Brain Res.)	38	8.2	Music (19) Passive Control (19)	Yes	24	2	Harmony Project (introductory musicianship class and instrumental classes)	✓		✓			
Slater et al. (2014) (PLoS One)	42	8.3	Music (23) Passive Control (19)	No	12	4.5	Harmony Project (introductory musicianship class and instrumental classes)	✓		✓			
Chobert et al. (2014) (Cereb. Cortex)	24	8.3	Music (12) Painting (12)	No	12	1.13	Kodály and Orff methodologies			✓		✓	
Kraus et al. (2014) (J. Neurosci.)	44	8.3	Music (26) Passive Control (18)	Yes	12	3	Fundamental skills and group instrumental instruction (strings, woodwinds, brass winds)	✓				✓	
Roden et al. (2014) (Appl. Cogn. Pscyhol.)	345	7.9	Music (192) Natural Science (153)	No	18	0.75	Lessons of an instrument of their choice	✓	✓				
Kaviani et al. (2014) (Cogn. Process.)	60	5.5	Music (30) Passive Control (30)	No	2.8	1.25	Orff method (singing, chanting rhymes, clapping, playing and keeping a beat)			✓			
Mehr et al. (2013) (PLoS One)	29	4.8	Music (15) Visual Arts (14)	Yes	1.5	0.75	Kindermusik, Orff method, Music Together			✓			

(*continued on next page*)

Table 1 (*continued*)

Study	Characteristics								Behavioral Measures		Brain Measures		
	N	Mean Age (years)	Groups (*n* per group)	Random Assignment	Training Duration (months)	Hours of Training (per week)	Type of Music Training	Instrumental Training*	Auditory	Language	MRI	EEG	MEG
	45	4.7	Music (23) Passive Control (22)	Yes	1.5	0.75	Kindermusik, Orff method, Music Together			✓			
François et al. (2013) (Cereb. Cortex)	24	8	Music (12) Painting (12)	No	12	1.13	Kodály and Orff methodologies					✓	
Rabinowitch et al. (2013) (Psychol. Music)	52	10.3	Music (23) Games (8) Passive Control (21)	Yes	9	1	Musical group interaction (musical tasks in the form of pre-arranged musical games)			✓			
Tierney et al. (2013) (Front. Psychol.)	43	14.7	Music (21) Fitness (22)	No	24	3	Band/Choral class (e.g., sight reading, singing, and playing technique)					✓	
Rickard et al. (2012) (Int. J. Music. Educ.)	111	12.7	Music (47) Drama (37) Art (27)	No	6.5	1	Playing and learn about different instruments (improvisation and composition)			✓			
Bugos and Jacobs (2012) (Res. Stud. Music. Educ.)	28	11.2	Music (15) Passive Control (13)	No	4	NR	Create music while learning compositional and stylistic concepts			✓			
Moreno et al. (2011a) (Psychol. Sci.)	48	5.3	Music (24) Visual Arts (24)	No	0.7	10	Computer-based program (rhythm, pitch, melody, voice, and basic musical concepts)			✓			
Moreno et al. (2011b) (Music Percept.)	60	5.3	Music (30) Visual Arts (30)	No	0.7	10	Computer-based program (rhythm, pitch, melody, voice, and basic musical concepts)			✓			
Herdener et al. (2010) (J. Neurosci.)	40	22.4	Music (19) Passive Control (21)	No	7.5	3	Aural skills training				✓		
Moreno et al. (2009) (Cereb. Cortex)	32	8.4	Music (16) Painting (16)	No	6	2.5	Kodály, Orff and Wuytack methodologies		✓	✓		✓	
Hyde et al. (2009) (J. Neurosci.)	31	6.1	Music (15) Passive Control (16)	No	15	0.5	Individual keyboard lessons	✓			✓		
Piro and Ortiz (2009) (Psychol. Music)	103	6.5	Music (46) Passive Control (57)	No	36	1.42	Piano training	✓		✓			
Shahin et al. (2008) (Neuroimage)	12	4.7	Music (6) Passive Control (6)	No	12	NR	Suzuki method					✓	
Fujioka et al. (2006) (Brain)	12	5.5	Music (6) Passive Control (6)	No	12	NR	Suzuki method		✓				✓
Moreno and Besson (2006) (Psychophysiol.)	20	8.5	Music (10) Painting (10)	No	1.8	1.33	Pitch discrimination (e.g., learning the different notes of the scale, musical intervals)					✓	
Gromko (2005) (J. Res. Music. Educ.)	103	5.5	Music (43) Passive Control (60)	No	4	0.5	Bruner's method (e.g., singing, body percussion)			✓			
Schellenberg (2004) (Psychol. Sci.)	132	6	Music (30) Music (32) Drama (34) Passive Control (36)	Yes	8.3	0.79	Keyboard lessons; Kodály voice lessons	✓		✓			
	42	5		No	4	NR	Suzuki method			✓			

(*continued on next page*)

Table 1 (*continued*)

Study	Characteristics								Behavioral Measures		Brain Measures		
	N	Mean Age (years)	Groups (*n* per group)	Random Assignment	Training Duration (months)	Hours of Training (per week)	Type of Music Training	Instrumental Training*	Auditory	Language	MRI	EEG	MEG
Orsmond and Miller (1999) (Psychol. Music)			Music (21) Passive Control (21)										
Flohr (1981) (J. Res. Music. Educ.)	156	5.3	Music (29) Passive Control (127)	Yes	3	0.83	Mixed music activities (e.g., improvisation, playing percussion instruments)		✓				
Young (1974) (J. Res. Music. Educ.)	64	5.5	Music (32) Passive Control (32)	No	2	1	Music activities (musical concepts and songs)		✓				
	64	5.5	Music (32) Passive Control (32)	No	2	1	Music activities (musical concepts and songs)		✓				

* This classification of training programs as instrumental or non-instrumental followed the criteria by Román-Caballero et al. (2022).

3.3. Meta-analysis of behavioral data

3.3.1. Overview

The 44 studies with behavioral measures contributed 161 effect sizes, based on an omnibus sample size of 3241 participants (music groups = 1529; passive control groups = 1029; active control groups = 683). Table 2 shows the distribution of individual studies and number of effect sizes across auditory and linguistic processing domains, as well as across more specific subdomains. Subdomain categories were defined by assigning different tasks to a particular auditory or linguistic skill (e.g., word discrimination and speech-in-noise perception both in the category of speech discrimination). The categories "general auditory discrimination" and "general linguistic skills" refer to studies in which the measures do not discriminate between different types of skills (e.g., rhythm and pitch discrimination; see tables S7 and S8 for details about the tasks).

3.3.2. Meta-analysis

We found a significant positive effect of music training on auditory and linguistic processing ($\bar{g}_{\Delta} = 0.31$, 95% CI [0.15; 0.47], $p < .001$; see tables S7 and S8 for individual effect sizes).

3.3.3. Heterogeneity

There was evidence for a significant high amount of heterogeneity ($I^2 = 76.69\%$, $Q(160) = 697.05$, $p < .001$), i.e., 76.69% of the between-studies variability in effect sizes was due to true heterogeneity rather than chance (Higgins et al., 2003).

3.3.4. Leave-one-out robustness analysis and influential studies

The positive effect of music training was not driven by specific studies, as it was replicated in all leave-one-out sensitivity analyses ($\bar{g}_{\Delta}$ range = 0.25–0.33; *ps* < .001). We detected two studies with Cook's distance more than three times the mean, though: Jaschke et al. (2018), $\bar{g}_{\Delta} = 2.41$; and Piro and Ortiz (2009), $\bar{g}_{\Delta} = 1.30$. The main model was repeated without these studies and the effect of music training remained significant ($\bar{g}_{\Delta} = 0.22$, 95% CI [0.10; 0.34], $p < .001$). Removing these outliers also reduced heterogeneity ($I^2 = 57.97\%$, $Q(154) = 441.36$, $p < .001$.They were therefore removed from the subsequent analyses.

3.3.5. Baseline differences

To examine whether there were differences between the music and control groups prior to training, we conducted a meta-analysis of g_{pre}. There were no group differences ($\bar{g}_{pre} = 0.01$, 95% CI [−0.07; 0.09], $p = .808$), including when the analyses considered separately studies with random assignment ($\bar{g}_{pre} = -0.09$, 95% CI [−0.24; 0.05], $p = .173$) and non-random assignment ($\bar{g}_{pre} = 0.05$, 95% CI [−0.05; 0.15], $p = .298$). These findings confirmed that randomization was successful, and highlighted that non-random assignment is not necessarily related to advantages in the music groups before training.

Table 2
Number of studies and effect sizes within each domain of outcome measure.

Domain of Outcome	Studies (*n*)	Effect Sizes (*n*)
Auditory Processing	**15**	**34**
Rhythm Discrimination	6	8
Pitch Discrimination	10	18
Timbre Discrimination	1	2
General Auditory Discrimination	5	6
Linguistic Processing	**36**	**127**
Phonological Awareness	7	11
Speech Discrimination	9	19
Reading	7	20
Verbal Fluency	8	17
General Linguistic Skills	20	60

3.3.6. Moderators

Most moderators did not explain a significant amount of variance in the effect sizes, namely domain of outcome measure (auditory vs. linguistic processing), type of training (instrumental vs. non-instrumental), year of publication, randomization (randomized vs. nonrandomized group assignment), type of control group (passive vs. active), duration of training (months), hours of training per week, age, and risk of bias (*ps* $> .145$; see Table S9 for statistical details).

The only significant moderator was baseline differences: the larger the baseline difference between groups, the smaller the observed effect of training ($F[1,40] = 15.61$; $\bar{g}_\Delta = -0.87$, 95% CI [−1.31;−0.42], $p < .001$). After accounting for this moderator, heterogeneity was slightly reduced, $I^2 = 48.73\%$, $Q(153) = 322.04$, $p < .001$. The moderating effect of baseline differences survived corrections for multiple comparisons considering the number of moderators (Bonferroni-corrected $p = .003$. see Fig. 2 for a meta-analytic scatter plot.

3.3.7. Publication bias

The trim-and-fill method with the *L0* estimator did not detect any missing studies. But when the same analysis was performed with the *R0* estimator, we found evidence in favour of eight missing studies on the left side of the funnel plot (see Fig. 3), a finding compatible with the presence of publication bias. After including these missing studies in a univariate model on the aggregated effect sizes to estimate a corrected effect of music training, the effect was much smaller and became non-significant ($\bar{g}_\Delta = 0.09$, 95% CI [−0.06; 0.24], $p = .221$). Regarding the PET-PEESE correction, the regression coefficient was not significant neither for the standard error in the PET meta-regression ($SE = 0.74$, $p = .280$), nor for the sampling variance in the PEESE meta-regression ($Vh = 1.53$, $p = .223$). Similar findings were obtained in separate analyses for auditory and linguistic processing (auditory processing, PET, $SE = 0.60$, $p = .629$, PEESE, $Vh = 2.65$, $p = .465$; linguistic processing, PET, $SE = 0.76$, $p = .318$; PEESE, $Vh = 2.37$, $p = .366$). In short, trim-and-fill is suggestive of the presence of publication bias, but PET-PEESE methods are not.

3.4. Synthesis of brain data

3.4.1. Overview

Table 3 and Fig. 4 present an overview of the studies including measures of brain structure and/or activity in relation to auditory and linguistic processing. The omnibus sample size is 1059 participants (music groups = 481; passive control groups = 318; active control groups = 260). Out of the 27 identified studies, 18 investigated effects of music training on auditory processing and 15 on linguistic processing (six studies focused on both). Seventeen used electroencephalography (EEG), eight magnetic resonance imaging (MRI), and two magnetoencephalography (MEG). Most evidence comes from children ($n = 15$; adolescents, $n = 2$; adults, $n = 5$; older adults, $n = 5$). Twelve studies included a passive control group, eight an active control group, and seven included both.

3.4.2. Auditory processing

Studies of music training effects on auditory processing have focused on instrumental and pure tone perception ($n = 11$), and on melody and/or rhythm perception ($n = 3$). EEG was the technique used more often ($n = 11$), followed by MEG ($n = 2$) and fMRI ($n = 1$). Instrumental and pure tone perception was examined in eight EEG, one fMRI and two MEG studies, and all asked participants to passively listen to streams of tones (e.g., piano, violin, or pure tones). Seven of these studies used oddball tasks, which examine participants' responses to deviant tones (e.g., A#), presented rarely among more frequent standard tones (e.g., A). The remaining four studies presented a stream of tones but without deviants. Melody and rhythm perception were examined in three EEG studies. One examined participants' responses to deviant melodies using an oddball task (Tervaniemi et al., 2022), and the remaining two asked participants to make same/different judgments on pairs of musical stimuli (Habibi et al., 2016; Moreno et al., 2009). Our synthesis also included six MRI studies that had no task or stimuli but focused on auditory systems and/or their connectivity. Four of them examined music training effects on structural aspects of auditory systems, including connectivity (Li et al., 2018), and cortical thickness and volume (Habibi et al., 2018, 2020; Hyde et al., 2009). Three focused on functional connectivity of auditory (Li et al., 2019, 2020) and auditory-motor networks (Li et al., 2018; this study included both sMRI

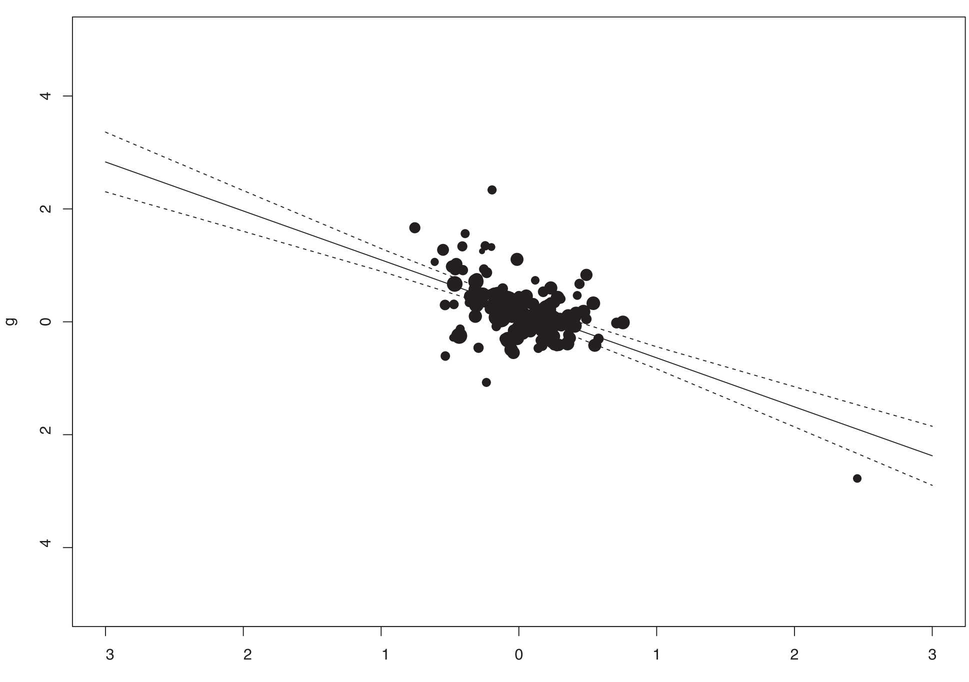

Fig. 2. Meta-analytic scatter plot showing the effect sizes of the included studies in the y-axis (Hedges' g) plotted against the predictor in the x-axis (baseline differences between groups, measured as the Hedges' g with the pretest scores). Larger baseline differences between groups led to smaller music training effects in auditory and linguistic processing. Each dot represents an effect size. The bold line corresponds to the regression line of the meta-regression model, and the dashed lines show the 95% confidence interval bounds (note: the moderating effect of baseline differences remains significant when the extreme value observed in the scatter plot is removed from the analysis).

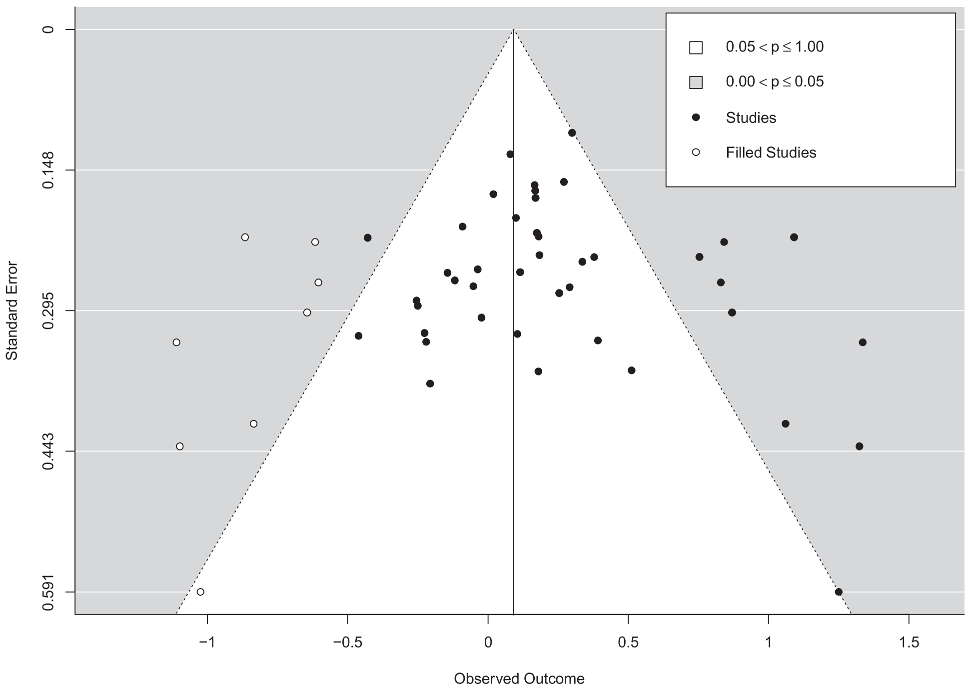

Fig. 3. Funnel plot with trim-and-fill of the aggregate effects of the studies. The y-axis represents the standard error of the aggregate effects, and the x-axis represents the magnitude of the effects (observed outcome). The vertical line represents the estimated common effect, and the black dots represent the aggregate effects of the studies included in the meta-analysis. The white dots represent eight missing studies imputed by the trim-and-fill using the *R0* estimator. The contour lines mark different standard levels of statistical significance (95% confidence interval).

and fMRI). One MRI study also examined associations between the volume of auditory areas and behavioral performance in a melody/rhythm discrimination task (Hyde et al., 2009).

Most studies on auditory processing were conducted with children ($n = 11$; adults, $n = 5$; older adults, $n = 2$), and compared music training groups with passive control ($n = 11$) and/or active control groups ($n = 7$). Moreover, most studies have not used random assignment of participants ($n = 14$), and an equal number of studies had instrumental and non-instrumental training programs ($n = 9$ for each). Sixteen out of 18 studies (88.89%) reported some significant benefit of music training on auditory processing (see Fig. 4). This was observed across age groups, regardless of the type of control group, use of random assignment, and type of training program. It was often the case, however, that the benefits were limited to some of the included measures ($n = 11$ out of 16, 68.75%). For example, in an EEG study with children, Moreno et al. (2009) found significant effects in the amplitude of N300 in response to weak incongruities in melodies (small pitch variations), but not in response to strong incongruities (large pitch variations). The two studies that did not find significant effects of music training were sMRI studies focused on children's cortical thickness and volume of auditory cortices (Habibi et al., 2018, 2020).

3.4.3. Linguistic processing

Studies of music training on linguistic processing have focused on speech perception, both in typical/quiet conditions ($n = 9$) and in noise ($n = 4$), as well as on speech prosody perception ($n = 2$). EEG was the technique used in all studies, except for the fMRI study of speech-in-noise perception by Fleming et al. (2019). In the studies examining speech perception in quiet, participants were asked to passively listen to streams of spoken stimuli, which consisted of vowels (e.g., Alain et al., 2019), words (Nan et al., 2018), or syllables (e.g., Kraus et al., 2014), for instance. Five of these studies have used an oddball task, and the remaining four did not include deviant stimuli. There was only one study that included an active task, asking participants to make familiarity judgments (familiar vs. unfamiliar) on pseudowords, which could be new to them or previously presented in a familiarization phase (François et al., 2013). The studies that examined speech-in-noise perception also varied in the type of stimuli (e.g., syllables, Hennessy et al., 2021; sentences, Fleming et al., 2019) and task. One study used passive listening (Tierney et al., 2013), while the remaining three included active tasks. For example, Zendel et al. (2019) asked participants to repeat words aloud. The two studies that examined prosody perception focused on the detection of pitch violations inserted at the end of spoken sentences (e.g., the fundamental frequency of the last word was increased by 120%). Specifically, children were asked to decide whether the last word seemed normal or strange (Moreno and Besson, 2006; Moreno et al., 2009).

Most studies on linguistic processing were conducted with children ($n = 8$; adolescents, $n = 2$; adults, $n = 1$; older adults, $n = 4$), and compared music training groups with passive control ($n = 3$) and/or active control groups ($n = 12$). Moreover, most studies have not used random assignment of participants ($n = 12$) and had non-instrumental training programs ($n = 10$). Twelve out of 15 studies (80%) reported some significant benefit of music training on linguistic processing. The effects were observed across age groups, regardless of the type of control group, use of random assignment, and type of training program. Nonetheless, these benefits were also often limited to some of the included measures ($n = 8$ out of 12, 66.67%). For instance, Hennessy et al. (2021) found significant effects for adults' N1 amplitude during passive listening to speech-in-noise, but not for the active speech-in-noise task (participants were asked to press a button when they could hear a target syllable). Moreover, three studies reported null results (e.g., cortical processing changes in older adults during the perception of deviant vowels; Alain et al., 2019).

3.4.4. Summary

The reviewed studies provide initial evidence that music training changes brain responses to auditory and linguistic stimuli, and the structure and functional dynamics of auditory systems. The benefits appear to be similar across age groups, but most evidence comes from children (55.56%), and therefore conclusions for the other groups remain tentative or non-existent. For example, no studies examined auditory processing in adolescents, and there was only one study examining linguistic processing in adults. Benefits seem to be observed slightly more often for auditory compared to linguistic processing (88.89% vs. 80% of the studies, respectively), but the type of control group did not make a difference (the percentage of studies reporting at least some positive effects of music training was 84.21% in the case of

Table 3

Overview of the studies included in the systematic review and narrative synthesis of music training effects on brain measures of auditory and linguistic processing ($N = 27$). The main findings are reported for statistically significant results ($p < .05$) comparing music training with control group(s). Abbreviations: AEF – Auditory Evoked Magnetic Field; DTI – diffusion tensor imaging; ERD – event-related desynchronization; ERP – event-related potential; ERS – event-related synchronization; FFR – frequency following response; fMRI – functional magnetic resonance imaging; GBA – gamma-band activity; LDN – late discriminative negativity; MMN – mismatch negativity; pMMR – mismatch positivity; sMRI - structural magnetic resonance imaging; ↑ – increased/enhanced/larger; ↓ – decreased/smaller).

Study	*N*	Mean Age (years)	Groups (*n* per group)	Random Assignment	Instrumental Training	Primary Focus	Me Measure (s)	Task	Is There a Benefit of Music Training (vs. Control)?
Tervaniemi et al. (2022) (Cereb. Cortex)	85	9.3	Music (29) Language (38) Passive Control (18)	No	No	Processing of auditory novelty	EEG - ERP	Oddball paradigm (multi-feature with tones and melodies)	↑ MMN amplitude during tone frequency deviants (but not for tone location, duration and intensity deviants) No significant effects for P3a amplitude (multi-feature with tones & melodies)
Hennessy et al. (2021) (Aging)	41	58.3	Music (18) Passive Control (23)	Yes	No	Speech-in-noise discrimination	EEG - ERP	Oddball paradigm (pure tones) and speech-in-noise perception (active & passive task with syllables)	↓ N1 latency in the active speech-in-noise discrimination task (but not for the passive speech-in-noise and oddball tasks) ↑ N1 amplitude in the passive speech-in-noise discrimination task (but not for the active speech-in-noise task) ↑ N1 amplitude for standard trials in the oddball task (but not distractor trials) No significant effects for P1, P2 and P3-like amplitude and latency (active & passive speech-in-noise tasks; oddball task)
Alain et al. (2019) (Front. Neurosci.)	53	68.2	Music (17) Visual Arts (19) Passive Control (17)	No	No	Processing of auditory novelty	EEG – ERP	Oddball paradigm (piano tones & vowels)	↑ N1 and P2 amplitude for the piano tones, as compared to the passive control group (but not as compared to visual arts group) No significant effects for vowels No significant effects in the MMN (piano tones & vowels)
Dubinsky et al. (2019) (Front. Neurosci.)	63	67.7	Music (34) Passive Control (29)	No	No	Speech perception	EEG - FFR	Passive perception of syllables	No significant effects (FFR strength at fundamental frequency)
Zendel et al. (2019) (Neurobiol. Aging)	34	68.0	Music (13) Video Games (8) Passive Control (13)	Yes	Yes	Speech-in-noise discrimination	EEG – ERP	Speech-in-noise perception (active & passive tasks with words)	↑ N1 amplitude during passive listening to words (but not for the active task) No significant effects for N1 latency ↑ Positive-going electrical brain activity during word repetition (but not for the passive task) ↑ Negative-going activity (700–1000 ms) during passive listening
Nan et al. (2018) (Proc. Natl. Acad. Sci. U.S.A.)	74	4.6	Music (30) Reading (28) Passive Control (16)	No	Yes	Processing of auditory novelty	EEG – ERP	Oddball paradigm (piano tones & words)	↑ pMMR for both words and piano tones Word discrimination based on consonants correlated with ↑ pMMR for piano tones (but not vowels) No significant effects in the MMN and LDN (piano tones & words)
Carpentier et al. (2016) (J. Cogn. Neurosci.)	30	5.6	Music (14) French (36)	No	No	Processing of auditory novelty	EEG – ERP	Oddball paradigm (piano tones & vowels)	↑ Multiscale entropy for piano tones and vowels No significant effects for power spectrum density (piano tones & vowels)
Habibi et al. (2016) (Dev. Cogn. Neurosci.)	37	6.9	Music (13) Sports (11) Passive Control (13)	No	Yes	Processing of tones and auditory discrimination	EEG – ERP	Passive perception of tones (violin, piano & pure) and melody/rhythm discrimination	↓ P1 amplitude during passive listening to piano tones (but not violin and pure tones) ↑ P3 amplitude in response to detected melody deviations, as compared to the passive control (but not as compared to sports group) No significant effects in the P2 and N2 amplitude
Moreno et al. (2015) (Child Dev.)	36	5.6	Music (18) French (18)	No	No	Processing of auditory novelty	EEG – ERP	Oddball paradigm (piano tones & vowels)	↑ LDN amplitude to piano tones ↓ LDN amplitude to vowels

(*continued on next page*)

Table 3 (*continued*)

Study	*N*	Mean Age (years)	Groups (*n* per group)	Random Assignment	Instrumental Training	Primary Focus	Me Measure (s)	Task	Is There a Benefit of Music Training (vs. Control)?
									No significant effects in the MMN (piano tones & vowels)
Tierney et al. (2015) (Proc. Natl. Acad. Sci. U.S.A.)	40	14.7	Music (19) Fitness (21)	No	Yes	Speech perception	EEG – ERP	Passive perception of speech (syllables)	↑ Response consistency across trials No significant effects in cortical onset response (N1 – P1 amplitude)
Chobert et al. (2014) (Cereb. Cortex)	24	8.3	Music (12) Painting (12)	No	No	Processing of auditory novelty	EEG – ERP	Oddball paradigm (syllables)	↑ MMN amplitude to duration and voice onset time of deviant syllables No significant effects for syllabic frequency
Kraus et al. (2014) (J. Neurosci.)	44	8.3	Music (26) Passive Control (18)	Yes	Yes	Speech perception	EEG – Time Frequency	Passive perception of contrastive speech (syllables)	↑ Neurophysiological distinction of contrastive syllables More hours of music training predicted larger improvements in neurophysiological function
François et al. (2013) (Cereb. Cortex)	24	8	Music (12) Painting (12)	No	No	Speech segmentation abilities	EEG – ERP	Speech discrimination (pseudowords)	↑ ERP difference between familiar and unfamiliar pseudowords (familiarity Effect in the 450–550 ms latency window)
Tierney et al. (2013) (Front. Psychol.)	43	14.7	Music (21) Fitness (22)	No	No	Speech-in-noise perception	EEG – Time Frequency	Passive perception of speech-in-noise (syllables)	↓ Neural transmission delay between stimulus presentation and the neural response
Moreno et al. (2009) (Cereb. Cortex)	32	8.4	Music (16) Painting (16)	No	No	Pitch discrimination in music and speech prosody	EEG – ERP	Melody and speech discrimination (sentences)	↑ N300 amplitude to weak incongruities in melodies (small pitch variations) ↑ Amplitude of a long-lasting positivity to weak incongruities in sentences (small pitch variations) ↓ Positivity to strong incongruities in sentences (large pitch variations) No significant effects for strong incongruities in melodies (large pitch variations) No significant effects for congruous melodies and sentences
Shahin et al. (2008) (NeuroImage)	12	4.7	Music (6) Passive Control (6)	No	No	Timbre-specific oscillatory gamma band activity	EEG – GBA	Passive perception of tones (piano, violin & pure)	↑ Induced GBA for piano and violin tones (as compared to pure tones) No significant effects on evoked GBA
Moreno and Besson (2006) (Psychophysiol.)	20	8.5	Music (10) Painting (10)	No	No	Pitch discrimination in speech prosody	EEG – ERP	Speech discrimination (sentences)	↓ Amplitude of a late positive component in response to strong incongruities in sentences (large pitch variations) No significant effects for weak incongruities in sentences (small pitch variations) No significant effects for congruous sentences
Habibi et al. (2020) (Brain Struct. Funct.)	23	7	Music (12) Passive Control (11)	No	Yes	Cortical thickness of Auditory Cortices	sMRI	–	No significant changes in cortical thickness
Li et al. (2020) (IEEE Trans. Neural Syst. Rehabil. Eng.)	56	23.2	Music (29) Passive Control (27)	Yes	Yes	Dynamic integration of functional systems	Resting-state fMRI	–	↑ flexible integration of primary functional systems, including the auditory system
Fleming et al. (2019) (Brain Cogn.)	33	67.8	Music (12) Video Games (8) Passive Control (13)	No	Yes	Speech-in-noise discrimination	fMRI	Speech-in-noise discrimination (sentences)	↑ Responses to speech in left middle frontal gyrus and right medial frontal gyrus, left supramarginal gyri and right superior/middle temporal gyrus ↑ Responses to speech (left middle frontal and supramarginal gyri) were correlated with better speech-in-noise perception
Li et al. (2019) (Brain Struct. Funct.)	56	23.2	Music (29) Passive Control (27)	Yes	Yes	Modularity in functional brain networks	Resting-state fMRI	–	↑ Flexibility and intersystem connections of the auditory system
	47	6.9		No	Yes		sMRI	–	

(*continued on next page*)

Table 3 (*continued*)

Study	*N*	Mean Age (years)	Groups (*n* per group)	Random Assignment	Instrumental Training	Primary Focus	Me Measure (s)	Task	Is There a Benefit of Music Training (vs. Control)?
Habibi et al. (2018) (Cereb. Cortex)			Music (15) Sports (15) Passive Control (17)			Cortical thickness and volume of auditory cortices			No significant effects (volume and cortical thickness)
Li et al. (2018) (Hum. Brain Mapp.)	56	23.2	Music (29) Passive Control (27)	Yes	Yes	Functional and structural connectivity within and between auditory and sensorimotor regions	Resting-state fMRI & DTI	–	No significant changes in connectivity within auditory regions ↑ Functional and structural connectivity between auditory and motor regions
Herdener et al. (2010) (J. Neurosci.)	40	22.4	Music (19) Passive Control (21)	No	No	Processing of auditory novelty in the hippocampus	fMRI	Oddball paradigm (tones)	↑ Activity in the left anterior hippocampus in response to temporal novelty in tones (stimulus onset asynchrony with different degrees of deviance)
Hyde et al. (2009) (J. Neurosci.)	31	6.1	Music (15) Passive Control (16)	No	Yes	Brain structure and auditory skills	sMRI	–	↑ Volume in the right primary auditory area (Heschl's gyrus) ↑ Volume in the right auditory area related to improvements on a melodic/rhythm discrimination test
Fujioka and Ross (2017) (Eur. J. Neurosci.)	14	63.5	Music (7) Passive Control (7)	No	Yes	Timing processing abilities	MEG - AEF	Passive perception of tones (metronome beats)	↑ Change of beat-induced beta modulation in the right auditory cortex (ERD & ERS)
Fujioka et al. (2006) (Brain)	12	5.5	Music (6) Passive Control (6)	No	No	Processing of tones and noise	MEG - AEF	Passive perception of tones (violin) and noise burst	↑ N250 latency peak in response to the violin tone ↑ N250 amplitude in the left hemisphere to the violin tone No significant effects for noise burst

passive control groups, and 86.67% in the case of active control groups). Although random assignment did not seem to make a difference in the observed benefits (all studies using random assignment reported at least some positive effects), most studies did not have random assignment (77.78%). The role of randomization therefore remains an open question. Additionally, the number of studies with instrumental and non-instrumental training was relatively balanced (48.15% vs. 51.85%, respectively), and the percentage of studies that reported at least some positive effects was high in both cases (92.31% for instrumental training, and 85.71% for non-instrumental training).

Although the percentage of studies reporting positive effects was high, in many of them the effects were restricted to some of the measures or conditions (auditory domain: 68.75%, linguistic domain: 66.67%), and six studies reported null results. For both auditory and linguistic processing, the effects seem roughly similar across the covered subdomains.

4. Discussion

We examined evidence for behavioral and brain effects of music training on auditory and linguistic processing. For the behavioral data, a multivariate meta-analysis revealed a small benefit of music training ($\overline{g}_\Delta = 0.31$), which remained significant after the exclusion of outliers ($\overline{g}_\Delta = 0.22$). The effect was observed regardless of the domain (auditory vs. linguistic), type of music training (instrumental vs. non-instrumental), type of control group (active vs. passive), or strategy of assignment to the groups (random vs. non-random). We found no overall differences between the music and control groups at baseline, but variation in the magnitude of baseline differences moderated music training effects: the larger the differences prior to training, the smaller the improvements. Moreover, meta-regression methods provided no evidence of publication bias (PET-PEESE), but trim-and-fill did, and the music training effect became non-significant after bias correction using this method. For the brain data, a narrative synthesis also provided evidence for a positive effect of music training, both for auditory and linguistic processing. In many of the included studies, effects were restricted to some of the included measures or conditions, though. Thus, the available literature provides evidence that music training causes small improvements in auditory and linguistic processing, but future studies will need to confirm that effect size estimates are not being inflated by publication bias.

4.1. Behavioral data

Previous meta-analyses examined far transfer effects of music training (e.g., Cooper, 2020; Román-Caballero et al., 2022; Sala and Gobet, 2020) but, to our knowledge, none has focused on near transfer. Empirical studies also show that there is more interest in far compared to near transfer: in our meta-analysis, 36 studies examined linguistic skills, and only 15 examined auditory skills. Perhaps near transfer effects are taken for granted and thought to require less attention, but examining them is central considering recent evidence that they might be weak or non-existent (Kragness et al., 2021; Schellenberg, 2020c). Moreover, if transfer from music to linguistic processing results from sharper auditory processing (e.g., Besson et al., 2011; Goswami, 2011; Patel, 2014), we need to establish that music training changes auditory skills. We provide meta-analytic evidence that music training can enhance aspects such as rhythm, pitch, and timbre discrimination. The fact that the study design did not play a role suggests that the benefits are unlikely to result from self-selection or nonmusical aspects of the training. Furthermore, we did not find differences between music and control groups at baseline, even when conducting separate analyses for studies with random vs. non-random assignment, which reinforces the idea that the benefits are unlikely to reflect self-selection. Benefits in auditory abilities are consistent with the notion that the auditory system is altered by music training (e.g., Herholz and Zatorre, 2012), and with correlational

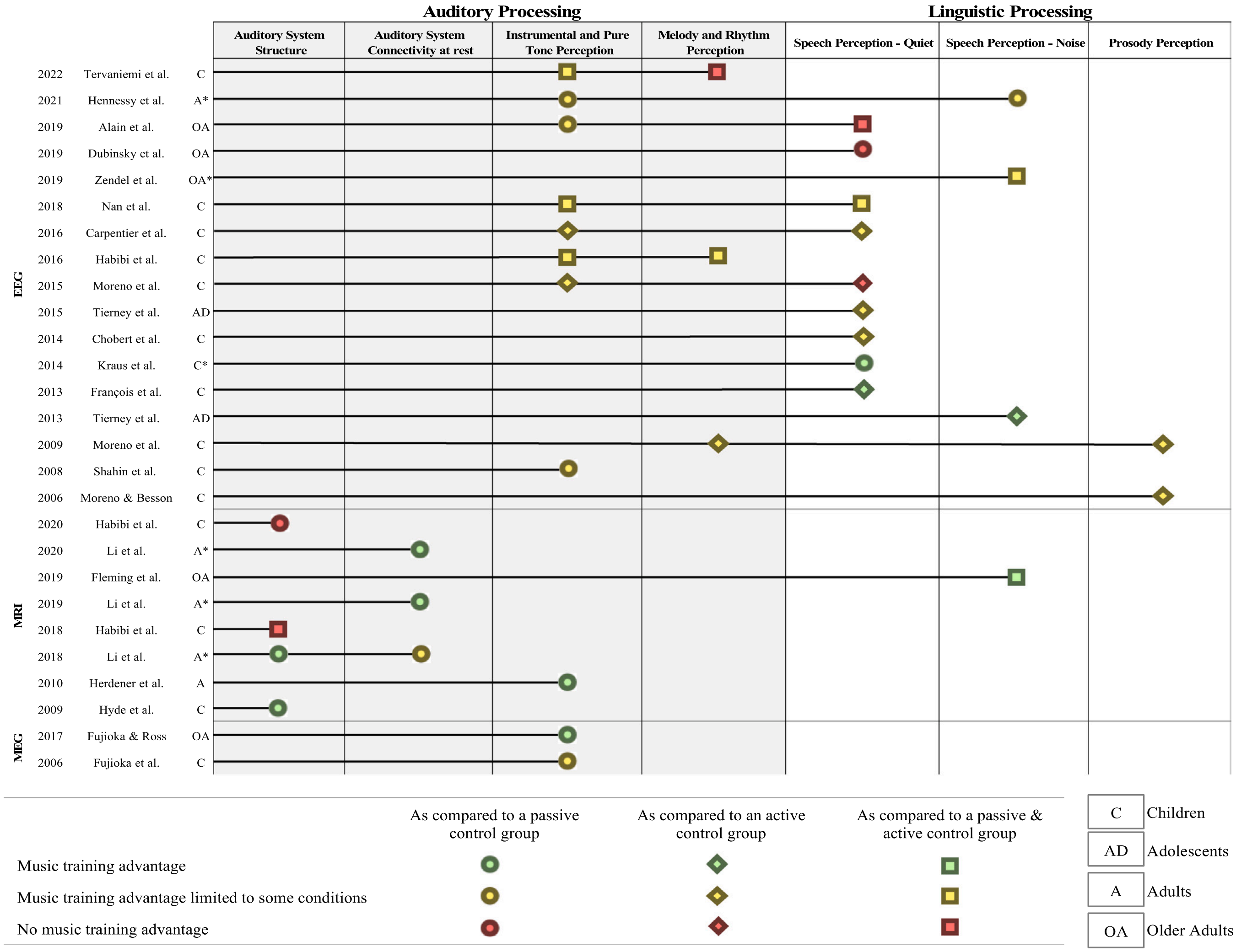

Fig. 4. Synthesis of the studies examining music training effects on brain measures of auditory and linguistic processing. The studies are organized according to domain (auditory or linguistic processing) and technique (EEG, MRI, or MEG). Green symbols indicate that the study reported an advantage of music training over passive and/or active control group(s); yellow ones indicate that the advantage was limited to some conditions (e.g., reduction of cortical thickness but not cortical volume); and red ones indicate that no advantage of music training was found. Circles indicate that the control group was passive, rhombuses that it was active; and squares that the study had passive and active control groups. “C” indicates studies with children, “AD” with adolescents, “A” with adults, and “OA” with older adults. The asterisks indicate that assignment to the groups was random.

evidence of advantages in these abilities in musicians (e.g., Rammsayer and Altenmüller, 2006; Schellenberg and Moreno, 2010; Tervaniemi et al., 2005).

Along with general cognitive abilities such as IQ, language is the most studied domain of far transfer from music training, and the one that attracts more theorizing (e.g., Besson et al., 2011; Patel, 2014). Previous meta-analyses covered language-related outcomes (e.g., Gordon et al., 2015; Román-Caballero et al., 2018; Sala and Gobet, 2020), but because their scope was broader, a comprehensive analysis of different aspects of linguistic processing was missing. Moreover, meta-analytic findings are mixed. For instance, Gordon et al. (2015) found significant benefits for phonological awareness in children, but not for reading fluency. Román-Caballero et al. (2018) found significant benefits for phonological verbal fluency in older adults, but not for semantic verbal fluency. Three meta-analyses found small-to-moderate benefits for general cognitive and academic outcomes in children, including aspects of verbal abilities such as vocabulary and phonological processing (Cooper, 2020; Román-Caballero et al., 2022; Sala and Gobet, 2020). Here we conducted the most extensive review of longitudinal data on music training and linguistic abilities, covering studies from all age groups, and found that the benefits are significant and similar across a range of domains, including phonological awareness, speech discrimination, reading, verbal fluency, and general linguistic skills (e.g., verbal IQ). These benefits were comparable to those observed for auditory abilities, and are also unlikely to reflect self-selection effects or nonmusical aspects of the training. That random assignment and type of control group did not play a role is consistent with recent meta-analyses on far transfer (Bigand and Tillmann, 2022; Román-Caballero et al., 2022; but see Sala and Gobet, 2020). More work will be needed to reconcile the benefits observed in longitudinal data with the pattern that emerges from correlational data. Many correlational studies report advantages of musicians in linguistic abilities, such as prosody perception (Lima and Castro, 2011; Marques et al., 2007), but these advantages are not always replicated (e.g., Trimmer and Cuddy, 2008), and the pattern of results for abilities such as speech-in-noise perception is mixed (Boebinger et al., 2015; Kaplan et al., 2021; Madsen et al., 2019; Parbery-Clark et al., 2009). Because correlation does not imply causation, but causation implies correlation, future studies need to uncover the sources of variability in the literature. Crucially, by documenting experience-dependent effects, we do not mean to overlook pre-existing factors. Recent evidence indicates that music training is not necessary to account for enhancements in linguistic abilities: musically untrained individuals with good musical abilities show a more efficient neural encoding of speech (Mankel and Bidelman, 2018), enhanced performance in tasks of speech perception (Swaminathan and Schellenberg, 2017, 2020), and better emotion recognition in speech prosody (Correia et al., 2020), mirroring the benefits observed in musicians. Both nature and nurture seem to account for associations between music and nonmusical domains.

The amount of heterogeneity in effect sizes across studies was high (76.38%), in line with meta-analyses based on pre-post intervention effect sizes (Cuijpers et al., 2017). In previous meta-analyses of music training effects, I^2 values ranged from 34% (Cooper, 2020) to 96% (Román-Caballero et al., 2018). However, the high levels of unexplained heterogeneity here were partially explained by influential effect sizes, as indicated by Cook's distance values. After removing two influential studies, heterogeneity remained significant but decreased (57.75%). The sources of the remaining variability are unclear. Although we considered ten moderators, only the baseline difference between groups was significant. The larger the differences at baseline, the smaller the effect of music training. This moderator accounted for 9.24% of the heterogeneity, which decreased from 57.75% to 48.51%. A moderating role of baseline differences has also been found by Román-Caballero et al. (2022) and Sala and Gobet (2020). Participants with lower abilities before training could have more room for improvement, or there might be regression toward the mean when samples differ markedly at baseline. The potential role of baseline performance levels in how much participants benefit from music training is an interesting avenue for future research.

Recent work suggests that the type of music training (instrumental vs. non-instrumental) could account for discrepancies across studies (Román-Caballero et al., 2022), but that was not observed here. Instrumental and non-instrumental training programs seem to have comparable effects in auditory and linguistic processing. Future studies could ask whether the putative advantage of instrumental training is more salient for transfer domains less reliant on auditory skills – auditory skills (which are important for auditory but also for many language tasks) are typically an important focus of both instrumental and non-instrumental training programs. Other characteristics of the training could also be a source of variability (e.g., individual vs. group training; vocal vs. instrumental training), and the same applies to the tasks and stimuli used to assess transfer.

4.2. Brain data

The present work provides the first systematic synthesis of electrophysiological and neuroimaging data on how music training shapes auditory and linguistic processing. The fact that most studies reported positive effects in at least some of the conditions (88.89% for auditory processing, 80% for linguistic processing) suggests that the observed behavioral benefits can be traced to plastic changes in brain structure and function. Most evidence comes from EEG studies with children (e.g., Carpentier et al., 2016; Moreno et al., 2015), but the number of those using MRI has been increasing (e.g., Habibi et al., 2020; Li et al., 2020).

Consistent with the behavioral data, EEG studies provide evidence that music training can shape several aspects of cortical auditory processing, including those related to instrumental and pure tone perception, and melody and rhythm perception. Positive effects are observed regardless of whether the control groups were passive or active. Tervaniemi et al. (2022), for example, found that music training led to higher MMN amplitude during passive listening to tone frequency deviants in an oddball paradigm. These findings arguably reflect an effect of music training at relatively automatic stages of auditory processing, but task-based studies indicate that effects can be seen at more controlled stages of processing too. Using a melody discrimination task, Moreno et al. (2009) found that music training was associated with a higher N300 amplitude during the perception of small pitch variations in melodies. MRI studies suggest that, in addition to effects on brain responses to auditory stimuli, music training can change the morphology, structural connectivity, and intrinsic functional connectivity of auditory systems. For instance, Hyde et al. (2009) found that music training increased cortical volume in the right primary auditory region in children, and Li et al. (2018) found enhanced structural connectivity between auditory and motor regions in adults. Li et al. (2019) also found that music training enhanced flexibility and intersystem connectivity of the auditory system. Moreover, a literature review suggests that music training might counteract age-related changes in auditory perception and cognition that manifest in late adulthood (Alain et al., 2014). Thus, there is evidence for music training effects on auditory processing at the levels of behavior and brain structure and function.

Our review also highlights that most neuroscientific evidence for music training effects on linguistic processing comes from studies on spoken language perception in quiet (60% of the studies). These studies have often used a passive listening approach. For example, Chobert et al. (2014) found that music training increased the MMN amplitude during passive listening to deviant syllables, and Nan et al. (2018) found increased pMMR amplitude during the perception of words (oddball paradigms). Although fewer, there are also studies that reported promising results for speech-in-noise perception and prosody perception. Zendel et al. (2019) found that music training increased N1 amplitude during speech-in-noise perception, and enhanced a positive-going electrical brain activity during word repetition. Furthermore, Moreno et al. (2009) found that music training was associated with increased amplitude of a long-lasting positivity in response to small pitch variations in sentences. Not only these findings are consistent with those obtained in the meta-analysis of behavioral data, but they are also in line with the notion that music and speech share neurocognitive pathways (e.g., Peretz et al., 2015; Zatorre et al., 2002). A potential explanation for the effects is that music training demands high precision on these shared pathways, leading to neurobehavioral plastic changes that also result in benefits for speech (Patel, 2014).

Both for auditory and for linguistic processing, positive effects of music training were often limited to some of the measures and/or conditions included in the studies. This might reflect true specificity of the effects, but it also raises concerns regarding potential false positives, particularly when no corrections for multiple comparisons are implemented. The small number of participants in many of the published studies adds to these concerns ($n < 20$ in the music training groups for 18 of the 27 identified studies, 66.67%), precluding definitive conclusions before the findings are replicated in larger samples. More well-powered studies, along with stricter statistics and more explicit hypotheses (regarding which measures are expected to improve and which ones are not), will shed light on the observed variability across studies. For example, in studies with children, while Moreno et al. (2009) reported that music training increased the amplitude of a long-lasting positivity in response to small pitch variations in sentences, Moreno and Besson (2006) found no effects using the same task on a different sample. This variability might additionally relate to the characteristics of music training programs, stimuli and tasks, which remain poorly explored. Moreover, because most available evidence is based on children, future work will be crucial to determine whether similar findings are observed for older participants. Finally, we were unable to perform a quantitative meta-analysis of the brain data because of the small number of studies and heterogeneity in the outcome measures. But, as the number of existing studies grows, it will be important to revisit these findings quantitatively.

4.3. Clinical implications and future directions

By documenting positive effects of music training, the present review suggests that musical activities could be an effective, safe, and comfortable tool to improve auditory and linguistic skills. These skills are crucial for everyday communication and social interactions (e.g.,

Table 4
Identified concerns and suggestions for future longitudinal studies on music training effects.

Concerns	Suggestions
Variability and lack of detailed information about the training programs	• Reporting details about the amount of training, including total duration, number of sessions per week, and whether participants are encouraged to practice at home or not • Providing a rationale and detailed description of the contents of training programs, including the covered skills and how they will be trained • Being explicit about the mechanistic links between the trained skills and the expected transfer effects • Linking the hypotheses to the specific features of training as much as possible
Evidence mostly limited to children	• Focusing on other age groups to determine whether the effects are age-dependent or more general across the life span
Small sample sizes	• Including larger samples, ideally informed by power analyses • Optimizing the reliability and validity of the measures (e.g., by using validated measures and/or running pilot studies)
Suboptimal designs	• Allocating participants randomly to the groups • Including active control groups • Controlling for confounding variables such as personality, cognitive abilities and socioeconomic status
Selective reporting and emphasis on findings favoring the music group	• Preregistering the studies, specifying details such as the full list of measures, hypotheses and plans for analyses • Reporting null results and consider them when discussing significant ones • Distinguishing between confirmatory and exploratory analyses • Data sharing

Neves, Martins et al., 2021; Parbery-Clark et al., 2011), and they are impaired in conditions such as dyslexia, specific language impairment, and hearing impairment treated with cochlear implantation. We note that the benefits of training were small, though, raising questions regarding their practical significance. There are some studies directly examining whether music training improves auditory and linguistic processing in clinical conditions (e.g., Cheng et al., 2018; Frey et al., 2019; Fuller et al., 2018), but this research is in its infancy and shares some of the problems observed in the music training literature, including small sample sizes, non-random assignment, and lack of active control groups. Additionally, although musical activities can have a unique motivational component, learning to play a musical instrument requires effort and time. It remains unclear whether shorter and focused interventions targeting specific auditory and linguistic impairments could be more efficient than music training. This would not mean that there is no value in engaging in musical activities. Music is fundamentally linked with positive emotions, mood regulation, and social bonding, and these are arguably the primary motives for the ubiquity of musical behaviors (e.g., Koelsch, 2014; Tarr et al., 2014).

We have also identified several limitations in the existing literature on music training that will need to be addressed in future work, as we summarize in Table 4. Improving aspects such as sample size, design quality, and unbiased reporting of findings will be crucial to reach firmer conclusions regarding near and far transfer effects. Publication bias is a particularly important issue. Meta-regression methods showed no evidence of bias, but the trim-and-fill method suggested that we cannot be sure that the music training effects truly exist beyond the reach of selective reporting of positive findings. To further complicate things, the available bias-correction methods have limitations, which might under- or over-correct meta-analytic estimates for biases (e.g., Stanley, 2017). In any event, future longitudinal studies on music training should adopt strategies to counteract publication bias, such as preregistration (see Table 4).

4.4. Conclusions

The present review provides evidence that music training has a small positive effect on auditory and linguistic processing. A multivariate meta-analysis showed that the benefits can be observed across a range of behavioral tasks, and a narrative synthesis of neuroscientific studies showed that they can also be observed at the level of brain function and structure. A causal role of music training can be inferred because we focused exclusively on longitudinal evidence, the effects were observed regardless of whether the assignment to the groups was random or not, and there were no differences between the music and control groups before training. These findings are suggestive of both near and far transfer, and have implications for debates on plasticity and on the use of music as an intervention tool in educational and clinical contexts. Because current evidence is often based on small samples, further well-powered studies are needed to establish the reliability of the findings. We have also obtained some evidence that publication bias might be inflating the true effect of music training, an issue that should be considered in future work.

Conflict of interest

None declared.

Data Availability

All the data and R code for the analyses are available in: https://osf.io/vz7mj/?view_only=dff52dfa43914ff0b1752707bf0cc41b.

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Appendix A. Supporting information

Supplementary data associated with this article can be found in the online version at doi:10.1016/j.neubiorev.2022.104777.

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