ToDo:

* Highlights and Graphical Abstract
* Format in STAR methods style

1. **Reviewer #1: The authors investigate vocal emotion recognition (VER) performance in musicians with different levels of training and activity in different domains (instrumentalists and singers). They presumed to find no influences of either factor, which was confirmed. However, they found that VER correlated with measures of auditory sensitivity. They conclude that VER operates at a perceptual level (auditory sensitivity) and cognitive levels (musicality, musical activity) may play little role in this respect. The findings do contribute to the literature. Whether they really close a gap, as the authors claim, remains to be seen. I think it is a bold statement to say that a gap has been closed. It feels a bit overambitious to me. Nevertheless, an interdisciplinary community could benefit from learning what musicality can mean and what not.  
   Reviewer #2: The authors find that vocal emotion recognition does not vary between singers and instrumental musicians, or between amateurs and professionals. The finding is important because if music training is the causal factor in links between musicianship and vocal emotion recognition, one would expect differences in performance based on the type of musical background, particularly whether it focuses on the voice, and the extent of training/performing.  
   Reviewer #3: Thank you for the opportunity to review this interesting manuscript. Overall, I find it to be a well-written and well-justified study. The design builds on a previously published experiment and aims to test specific hypotheses regarding vocal emotion recognition across different musical subgroups. The study was preregistered with nine hypotheses that are tested for, and the target sample size (N=40 per group) was met or slightly exceeded. Recruitment procedures, inclusion/exclusion criteria, and compensation protocols were implemented as described. In addition, exploratory analyses were conducted, which is acceptable as long as they are clearly acknowledged as such. The study aligns with previous work, that musicians expertise in vocal emotion recognition is explained by auditory sensitivity, rather than musical training. The study extends a previous study comparing professionals and non-musicians, by including a group of amateurs. Comparisons are also made between singers and instrumentlists.**

*Response:* We thank all three reviewers for their effort and their careful evaluation of this manuscript. Below, we will respond to each point in detail.

1. **Reviewer #1: Humans operate at different, but highly intertwined levels, perceptual, cognitive, social. There is constant interaction between those. The authors conducted a study to suggest that VER is likely "programmed" to some degree in the nervous system. If it could be modified by learning, music is less a candidate than it might seem. This is a fair conclusion from the literature, to which the current study attempts to contribute. Nevertheless, participants respond in full consciousness, thus using their cognitive abilities to allow for inferences about their perceptual skills. So, in other words, there is no direct, albeit converging evidence to sustain the hypothesis of enhanced auditory sensitivity per se to explain individual differences in VER, irrespective of musical training and modality of training (singing vs. playing instruments). Please explain, why musicianship should play a role in such short utterances. I am aware that the "springboard" seems to be initial positive findings, but does that warrant continued investigations? Why should vocalists have an advantage? But the test amounts to show they do not have one. Why it is important to confirm this? Any indication in the literature expect the opposite? In other words is this an original hypothesis or an exploration?**

*Response:* ToDo

1. **The psychoacoustic transformation of stimuli materials is an interesting feature of the study. It suggests that no sensitivity can compensate for a reduction of stimulus information that leaves the participants merely with timbre differences. However, some emotions seem to be affected more or less strongly by morph type (interaction), which seems to make the picture more complicated. Indeed, the selection of emotions (p.10) appears not intuitive. The rationale to use happiness and pleasure (alongside fear and sadness) as distinct categories should be explained. Is there a circumplex-idea in the background? Could it be confirmed that the four emotions occupy four quadrants? Otherwise, there could be a confound arising from similarity and differences of underlying dimensions of the four emotions. - Please explain.**

*Response:* ToDo

1. **I wonder whether VER could be "boiled down" to the ability to recognize and differentiate pitch contours vocal utterances, with anything going up indicating arousal, and downward indicating relaxation to guess the correct emotion on the basis of contour alone seems rather plausible. So, in our daily interactions, we use this skill myriads of times. Why should even 10.000 hours of music training in 10 years (to reach expertise) change anything in this basic skill? So, why are the findings not trivial after all? Please explain, especially in the light of testing a Null hypothesis.**

*Response:* ToDo

1. **Reviewer #2: In general, the writing is very clear. My one main suggestion is to emphasize the big picture more. If individual differences in vocal emotion recognition are caused by an environmental factor (i.e., music training/performing in this instance), one would expect such differences to depend on the quality and quantity of the environmental intervention, yet none was found.**

*Response:* ToDo

1. **I also thought that the ms could be streamlined a bit more by not repeating details that were included in the authors' earlier publication (Nussbaum et al., 2024), but rather referring the reader to the previous article. The authors do this to some extent already. I'm wondering if they could do it more without making reading the earlier paper a prerequisite for understanding the present submission.**

*Response:* ToDo

1. **Reviewer #3: Major issues: Q1. Transparency and preregistration  
   The study claims to follow open science principles through preregistration. However, the preregistration was submitten on May 3, 2025, more than two years after the original study (Nussbaum, 2024), when some data had already been collected and analyzed. While the current study includes new data collection, the timing of the preregistration, shortly before manuscript submission, raises concerns about whether it qualifies as a true preregistration. Most importantly, this makes it unclear whether the hypotheses were formulated a priori or post hoc. Although I don’t consider this as an attempt to circumvent p-hacking, I believe these issues should be addressed explicitly if the benefits of preregistration and open science are to be fully realized.  
   This is my major concern and I would like the authors to elaborate on this, in the manuscript primarily, but of course also in their response.**

*Response:* ToDo

1. **Q2. Coding of PROMS Responses  
   The manuscript states:   
   "In alignment with the approach by Nussbaum et al. (2024), we recoded responses in the PROMS from 0 to 1 in 0.25 steps starting with the “definitely” correct option down two the “definitely” incorrect option (thus, “don’t know” was always coded with 0.5) and subtracted 0.5 from the final measure"  
   Should "down two" be "down to"? Is "definitely correct" coded as 0 or as 1? Why is 0.5 added and then subtracted for the “don't know option”. Please explain more clearly.**

*Response:* ToDo

1. **Q3. Justification of Priors  
   In Bayesian analysis, the choice of priors is critical. While “default setting of priors” are convenient and often appropriate, they may not reflect the best assumptions for a specific research question. The manuscript does not justify the choice of priors, which is particularly relevant given that Bayes factor outcomes are sensitive to prior assumptions. Without such justification, it is unclear whether the default priors aligns with the expected effect size or the theoretical framework of the study (which anticipates a medium effect, as mentioned in the power analysis). Please provide a rationale for the chosen priors.**

*Response:* ToDo

1. **Q4. Sensitivity Analysis  
   The manuscript does not report whether results are robust to different prior choices. This is a common but important omission in Bayesian reporting. I recommend a sensitivity analysis to demonstrate the reliability of the findings across varying prior assumptions.**

*Response:* ToDo

1. **Q5. Rationale for Emotional Categories  
   The rationale for selecting the emotional categories could be expanded. While the inclusion of two positive (Pleasure, Happiness) and two negative (Fear, Sadness) emotions provides a balanced valence structure, it is unclear whether the authors considered the temporal and psychological distinctions. For example, Pleasure is often immediate and biologically driven, whereas Happiness is more enduring and psychologically constructed. Similarly, Fear is typically short-lived and reactive, while Sadness may be more prolonged. Where these distinctions considered in the design? Could comparisons between short-term and long-term, or comparisons between positive and negative grounded emotions offer additional insights? And if so, as an exploratory analysis.**

*Response:* ToDo

1. **Q6. Inclusion of Big5 Personality Traits  
   In table 4, the BIG5 personality traits are reported, although they are not linked to any of the preregistered hypotheses. Was analysis conducted for exploratory purposes? If so, please clarify and consider presenting these results in a separate exploratory section to avoid confusion.**

*Response:* ToDo

1. **Q7. Terminology in Hypotheses H5-H7  
   Some terminology used in hypotheses H5-H7 is not introduced in the methods section. See “General-ME”, “Perception Subscale”, and “selfrated singing abilities”. For many readers, these terms only become clear when reviewing the results, particularly Table 4. Please define these terms earlier in the manuscript.**

*Response:* ToDo

1. **Q8. Hypothesis H9   
   I did not understand the rationale for hyptothesis H9. Please explain.**

*Response:* ToDo

1. **Q9. Definition of Singers (and Instrumentalists)  
   On page 9, singers are defined as individuals who “must not currently be instrumentalists in an orchestra or a band". Did you control for participants’ musical history, such as previous experience playing an instrument? If so, how?**

*Response:* ToDo

1. **Q10. Stimulus Validation  
   Were the stimuli validated? Figure 5 in Nussbaum (2024) shows confusion rates for different emotions, but perhaps there were specific stimuli that were consistently misclassified? Are misclassifications a result of poor interpretation or poor stimuli representation? For example, in Figure 3, is the poor performance in Timbre condition a result of poor stimuli quality or poor perception? Please elaborate.**

*Response:* ToDo

1. **Q11. Conclusion and causality  
   The conclusion that “the ability to recognize emotions in voices is not primarily determined by the type or amount of musical activity, but rather by individual differences in auditory sensitivity” is based on correlations in table 3 and table S10. First, are both tables including control for musical education? Or is it only controlled for in table S10. Please explain the differences. Second, “determined by” implies a causal relationship. Since this is not an experimental setup where individuals’ auditory sensitivity is manipulated, the causal claim has to be based upon theoretical assumptions (for example by motivating that no other potential causes occur).**

*Response:* ToDo

1. **Reviewer #1: The causal language should be reconsidered, even though it is taken on board from the literature, in part. This is a correlational study.**

*Response:* ToDo

1. **The number of hypotheses seems inappropriate to me, at least. As stated above, commenting on a correlation table is sufficient. The hypotheses 3-7 are not at the same level of the surrounding ones. That should be reflected in the writing.**

*Response:* ToDo

1. **Reviewer #2: The ms is written in APA style but some of the references diverge from APA (e.g., title case for article titles, abbreviated journal names).**

*Response:* ToDo

1. **São Luis Castro should be abbreviated Castro, S. L. in the reference list.**

*Response:* Thanks for noticing. We fixed this.

1. **p. 30 "...future research should incorporate..." Change "incorporate" to "consider"**

*Response:* Changed as suggested.

1. **Why/how were the four subtests from the PROMS chosen? These aren't the same as those that are included in the mini-PROMS.**

*Response:* This is true. We chose these specific subtests because we considered them most informative for our research. Because we are contrasting pitch (F0) and timbre in the voice, we included the subtests that we assumed to capture similar aspects in the musical domain (melody, pitch and timbre) and then added on rhythm task to capture sensitivity to temporal information as well. We included only four subtests to keep overall duration of the study within reasonable limits. We added a small clarification to the manuscript: “[...] comprised of the four subtests „Melody“, „Pitch“, „Timbre”, and „Rhythm“, which we considered most informative for the present research question.” (page 13)

1. **Baldé et al. (2025) now has volume, issue, and page numbers. It came out in the June issue of JEPHPP.**

*Response:* We added the missing information.

1. **Reviewer #3: Minor issues: Q12. Clarity in Reporting Simple Main Effects. The reporting of simple main effects following significant interactions is exemplary in format, however, there are some points that require clarification. See for example sentence: "…all pairwise comparisons |ts(77)| = 2.57, ps = .012, ds = 0.28 [0.06 0.49], except for Fear vs. Sadness (|t(87)| = 1.13, p = .261)." This begins with comparisons between F0 and Timbre (conditions), but ends with comparisons Fear vs Sadness (emotions). Please clarify and make sure all relevant comparisons are reported.**

*Response:* In this particular paragraph, we calculated the performance difference between the F0 and the timbre conditions and compared this difference between emotions. We carefully checked the sentence, and we think that this is described here correctly, but of course we are open to suggestions to make this clearer to avoid readers’ confusion.

1. **Q11 Epsilon HF: in table 2, explain epsilon{HF}, for example in a footnote. (Huynh-Feldt I assume).**

*Response:* We added the following note: “Note. **εHF =** Huynh–Feldt (HF) epsilon correction factor in case of violation of the sphericity assumption.” (page 18)

1. **Q12. Missing Legend in Table 3: Table 3 lacks a legend. Please add one to clarify the meaning of the columns and values.**

*Response:* Good point. We added a table caption “Spearman correlations between the PROMS and vocal emotion recognition performance” and more information in the legend: “VERAvg: VER performance averaged across all trials, Full-Morphs: VER in the Full Morph condition only, F0-Morphs: VER in the F0 Morph condition only,, Timbre-Morphs: VER in the Timbre Morph condition only, PROMSAvg: music perception performance averaged across all four subtests of the PROMS (Pitch, Melody, Timbre, and Rhythm).” (page 21)

1. **Q13. Terminology Consistency: The manuscript uses both “Vocal emotion recognition” and “vocal emotion perception”. Please ensure consistent terminology throughout the manuscript or clarify distinctions if several terms are intentionally used.**

*Response:* We have changed “vocal emotion perception” to “vocal emotion recognition” throughout.

1. **Q14. Comparisons between Professionals and Non-Musicians: In Table 4, some variables (Extraversion, Attention to Detail, Social Skills, Rhythm) show differences between professionals and non-musicians. These comparisons are not reported in the table. I suggest either including them where significant or or explicitly stating that such comparisons are reported elsewhere (Nussbaum, 2024?).**

*Response:* Indeed, we did not focus on these comparisons, because they are already reported and discussed in the previous publication. This is mentioned in the manuscript under 4.2 Method: “We focused our analysis on the comparison of amateurs with the other two groups, because the comparison of professional musicians and non-musicians is reported in Nussbaum et al. (2024).” To make this more transparent to the readers, we added a note to Table 4 as well: “*For a detailed description and discussion of the differences between professional musicians and non-musicians, please refer to the previous publication (Nussbaum et al., 2024).*” (page 24)

1. **Reviewer #1: The authors do not mention in the Abstract, which emotions were considered or whether a model representing dimensions of emotion recognition was used. Therefore, it is not obvious what they mean by emotion recognition upfront.**

*Response:* ToDo

1. **The authors use causal language at times, which is understandable due to the wide-spread use of it in the literature. However, in their own terms, they should rather consider to talk about associations, even when the literature they cite implies causality in contexts such as correlational studies. So, please avoid causal language.**

*Response:* ToDo

1. **The findings are relevant as they extend the current knowledge to assert that professional or amateur vocalists or instrumentalists do not differ in their VER performance, neither do personality, or music sophistication play any role. However, this may still not be too surprising in light of previous literature, although this conclusion can now be drawn on a planned study.   
   For example, https://doi.org/10.1037/emo0000770 found that "natural musical abilities" without training are sufficient to excel in VER, limiting the role of musical training, a study that the authors cite themselves. Note that the sample in that study also was substantial (N=169). This leaves the authors' claim of a substantial sample size as a reason to conduct this study with a bit of a sour taste, albeit the sample size was sufficient for their study. I do not really trust the claim as expressed in the first line of 5.3.**

*Response:* ToDo

1. **There is ample research by Schellenberg to suggest that musical talent or aptitude rather than musical practice and expertise predicts a range of aspects of auditory functioning, especially in the linguistic or speech domain** [**https://doi.org/10.1037/xlm0000798**](https://doi.org/10.1037/xlm0000798)**.**

*Response:* ToDo

1. **The authors report a software error (p.13), but offer no account for how that error could have affected the results.**

*Response:* ToDo

1. **I am struggling with the authors' decision to test 9 hypotheses. I believe that hypothesis-driven approaches are mandatory, but there is a line to be drawn between hypotheses based on previous findings in the literature and ad hoc hypotheses (working hypotheses). Therefore, I would rather like to see the basis for H3-H7 more clearly. Otherwise, a correlation table is sufficient with an associated description of implications. Also, considering an arbitrary N/hypothesis ratio, it turns out that 10 individuals on average account for one hypothesis. Given the massive load of data acquired in the 25 minutes of experimenting (fatigue-effects being ignored), I think we are approaching a grey area in experimental economy. The authors should perhaps run tests whether the quality of responses at the beginning and at the end of the 25-minute period is still the same.**

*Response:* ToDo

1. **Table 5 reports an emotion x morph type interaction. Did I miss a discussion of that effect, or follow-up analyses? Please explain.**

*Response:* ToDo