

Date: 09 May 2022
To: "Jont Allen" jontallen@ieee.org
cc: lorenzi@ens.fr
From: "Journal of the Association for Research in Otolaryngology" jaro.editor@gmail.com
Subject: Decision on your Manuscript #JARO-D-22-00023

Dear Dr. Allen:

Thank you for submitting your manuscript, "Analysis of phone-errors in Reading Disabled children", to the Journal of the Association for Research in Otolaryngology (JARO). I am sorry to inform you that your manuscript is not accepted for publication. The reviewers and the handling editor have provided detailed comments on your manuscript, which we hope you find helpful as you proceed with your investigations.

Thank you for your interest in JARO.
Sincerely yours,

Paul B. Manis, Ph.D.
Editor
Journal of the Association for Research in Otolaryngology

Comments for the Author:

Dear colleagues,

Two reviewers with sound knowledge in reading impairment and speech perception have read your manuscript. Although both agreed that the topic is interesting, they felt that your manuscript presents a number of flaws and inaccuracies that make it unsuitable for publication in JARO. For instance, the huge literature on reading difficulties is not correctly covered. I agree with this comment. Both reviewers also pointed out that the study is based on a small dataset and recommend to include more participants and analyze further the differences between participants. One reviewer also noted that the manuscript fails to present a clear, relevant and original research question. Based on these negative reviews, I am afraid that I cannot accept your manuscript for publication in its present form. I am sorry to reach a negative decision given the quality of the speech tasks used in this study and the original methods developed by your group to analyse confusion matrices. However, the general and specific concerns raised by the two reviewers are quite serious and would require substantial changes (eg, including more participants) that go beyond a "major revision". I hope their many comments and suggestions will help you improve this study and pursue this important and in-depth investigation of phonetic processing in RDs.
Kind regards,
Christian Lorenzi

Reviewer #1: This is a very interesting and stimulating piece of research. However, although that the idea is very worthwhile, a number of major remarks and a list of other comments can be made

Major remarks

This study is based on an original idea.

However, a major shortcoming is that the authors do not quantify possible differences enough, and do not specify and develop scientific measures to argument with figures.

The argumentation about effects of difference stops with a "having a look at" the confusion matrices of RD and RC. However, it is hard to notice any clear significant differences beyond the fact that some cells in the confusion matrices have larger numbers. The authors should analyze further and try to develop metrics to specify differences. The differences between RD and RC are not clear. So, although I very positively evaluate this line of thinking for dyslexia research, I think the future of this report and manuscript is either analyzing further and developing metrics that can be used for dyslexia research from the existing (small) dataset or either considerably extending the dataset with more subjects and dig further in the differences between the different RD readers. The multi-dimensional aspect of dyslexia is presently very important in dyslexia research.

It would have been very positive if the authors take one more step, develop metrics and measures that quantify the amount of confusions to allow direct comparison across subjects. Maybe the features calculated from the confusion matrices as used for information transmission analyses can be applied, cfr work of Miller and Nicely 1955 with prior assumptions about the dimensions. New features might have to be defined for these reading deficiency investigations, or based on sequential information transfer analyses (Wang and Bilger 1973) which also require predetermined categories. Or to submit the data to multidimensional scaling analyses where no predetermined selection of categories is required. For the latter the datasets might be limited.

Also, to get an idea of the precision of the results the authors should check test-retest differences, or split dataset in half and analyze both semi-sets separately to establish a measure of precision. This measure can provide us with a correct interpretation of the differences observed.

Although that the research idea is very worthwhile, some claims in the manuscript can absolutely not be made and I would like to stimulate the authors to proceed and develop metrics to quantify "a level of dyslexia"

The claim that no distinct patterns can be revealed (the idiosyncratic nature of RD) cannot be made based on these data.

P7: "nine children aged 8 to 11 years" this is a very broad age range for such a very small sample. A lot about reading and learning is taking place at this age.

P15L421-423: "not reveal distinct patterns" this cannot be proven with such small samples

P15L429: "little overlap" quantify with figures

P16L433: proof with numbers?

P19L502-504: "only weak correlations" "... correlations ... found"

P19L493-495: this is a very abrupt transition! Please make more readable

P19L499-501: this is a strange argumentation, especially because it is all about only 9 subjects.

P20L530-531: this is jumping very far. This cannot be proven by the data!

Explanations: In several parts of the manuscript explanations in the text are missing (and do not use figure and table captions to explain). Give more information related to Table 4 bottom (explain what this is)

P19L493-495: this is a very abrupt transition! Please make more readable

The literature review in the "previous research" section is rather selective to a "no effect" but several references are missing where a statistically significant effect, though small, is demonstrated (see refs below).

P4L98-100: how do these 2 research questions relate to the 4 research questions on P7L207-214? Make more clear what the hypotheses or research questions are and structure the other sections in a similar way.

Confusion matrices: why not present the confusion matrices with the same ranking/order of phones, to allow more easy comparison across matrices? why 23 items in Table 5, 6 and 25 in Table 8? What about the vowel confusions?

SCO: No SCO data are reported or discussed. Why not keep it simple for the readers and do not mention at all (or only 1 line) SCO?

P8: please add argumentation why was not chosen for speech-in-noise perception? Several studies have shown already a small but significant and consistent effect? (see refs below)

Other comments

Overall: I do not think it is allowed to use the first names of the subjects because it might give clue of identity, breaking the privacy of the subjects.

P1: Abstract: "15-20% of grade school students have RD" this is very high in comparison with international prevalence figures!

P3: "we do know that its not a dysfunctional brain" wat is meant? Everyone considers that dyslexia is neurologically determined (and as such related to functioning of the brain)

P3: figure 1: In this figure "text" is used for 2 different modi. At "decoding" it is about written tekst, at "encoding" it is about spoken text. I do not know that it is correct to put it in this way in the same figure

P5: the authors do not mention a number of studies of speech intelligibility demonstrating speech perception deficits (see refs below)

P5L133: recent? (10 years ago)

P5-6: 3 sections "Results". Rewrite and restructure, can be made more clear

P6L193: Lu 2018: is not available for readers, refer to published material or mention where can be accessed.

P8L239: I presume HL instead of SPL?

P8L267 "chosen by the subject" and L272: "remove ... loudness irregularities": how was this done? Specify

P9 and abstract: 1500 trials in 2 week period and average trial time is about 10 seconds (Table caption 1). This adds up to only about 25minutes testing in 2 weeks time. That means that large session variabilities can be present? This might as well be done in 1 or 2 sessions with some child-friendly guidance? Why 2 weeks?

P9L286-292: the MaxEnt paragraph is not clear, please explain

P10: figure 2 and caption is not clear enough: where are the "subjects on the left"?, "better than average performance than the average RC normal"?, explain/define "n" and "probability of Error"

P13 table 4 bottom: please explain what this figure shows?

P14 Figure 3: use same X and Y axes for all 4 figures or both C and both V figures?

P15L404: "a larger spread"??

P16fig4: use same X and Y axes to allow more easy comparison

P16L435: "defined to modeled the average ..." rephrase sentence

Refs

Van Hirtum et al. A Bridge over Troubled Listening: Improving Speech-in-Noise Perception by Children with Dyslexia. JARO 2021

Vandermosten et al. Adults with dyslexia are impaired in categorizing speech and nonspeech sounds on the basis of temporal cues, PNAS 2010

Boets et al. Auditory processing, speech perception and phonological ability in pre-school children at high-risk for dyslexia: a longitudinal study of the auditory temporal processing theory, *Neuropsychologia* 2007

Boets et al. Preschool impairments in auditory processing and speech perception uniquely predict future reading problems. *Research in Developmental Disabilities* 2011

Reviewer #2: What causes the phonological difficulties of children with dyslexia remains hotly debated. Here, a study was designed to investigate phonological awareness in children with reading difficulties. Although the topic is interesting, the manuscript presents a number of flaws, errors and/or inaccuracies that make it unsuitable for publication. Here are my main concerns about this work:

- The very definition of "reading difficulties" is disconnected from the established literature on the topic. The manuscript refers to "children (who) never learn to read", in a proportion of "more than 15%" (line 89) or "15-20%" (line 22), without "dysfunctional brain". The manuscript also makes a "strong parallel ... with hearing impairment". None of these claims are adequately substantiated by references, and all of them are, in fact, wrong. See for example Lyon, Shaywitz & Shaywitz, 2003; or Vellutino, Fletcher & Snowling, 2004 for exhaustive descriptions of the reading deficit. Note that references must come from peer-reviewed scientific articles, not from the general press (Hanford, 2018). In short, children with reading difficulties can read but are prone and slow, the prevalence is typically estimated under 10% (Wagner et al., 2020), the deficit has a neurobiological origin - reading difficulties cannot be attributed to a sensory deficit such as hearing impairment. In addition to the general inaccuracies about reading difficulties, the manuscript omits a large portion of the recent literature on speech perception in noise. This literature converges to show intact speech perception in optimal, quiet environments; but consistent deficits in noisy environments (Ziegler et al., 2009 cited in the manuscript; for a review see Calcutt et al., 2018).
- Following these imprecisions/errors in the literature review, the manuscript fails to present a clear, relevant and original research question. Some questions are posed (lines 207-214), but they are unrelated to the literature review and are not accompanied by operational hypotheses. What seems to emerge as the research question is whether children with reading difficulties make more/different phonological confusions than typically developing children.
- Is this truly the research question, the results do not present any statistical analyses that would allow to answer the question. The result section only provides a qualitative description of the data collected.
- In addition to these major concerns, a number of methodological issues need to be addressed before the manuscript is considered for publication: the number of participants varies throughout the manuscript (line 390: "ten"; line 227: "nine"; line 17: "more than ten"). The n~10 participants with reading difficulties have been compared to n~6 age-matched controls. A power analysis would be relevant to confirm that the study is not under-powered. Another concern is whether the study was approved by an ethical committee. Please confirm that the ethical committee agreed that the first name and reading status of the children (eg see Table 2) is revealed in the manuscript - this seems unnecessary and I would advise to anonymise the data. Other methodological points to clarify are: audiometry is typically measured in dB HL, not dB SPL (line 239); how was nonverbal IQ measured? what are CI/CF (line 264); please define "maximum entropy" in the context of the material created for the study.

References

- Calcutt et al. (2018). The "rowdy classroom problem" in children with dyslexia. In: "reading a dyslexia": literacy studies.
- Lyon, Shaywitz & Shaywitz (2003). A definition of dyslexia. *Annals of Dyslexia*, 53.
- Vellutino, Fletcher, & Snowling (2004). Specific reading disability (dyslexia): what have we learned in the past four decades? *Journal of child psychology & psychiatry*, 45(1).
- Wagner et al. (2020). The prevalence of dyslexia: A new approach to its estimation. *Journal of learning disabilities*, 53(5).

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