

Ocean Sci. Discuss., referee comment RC1
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Comment on os-2021-101

Anonymous Referee #1

Referee comment on "On the use of acoustic data to characterise the thermohaline stratification in a tropical ocean" by Ramilla Vieira Assunção et al., Ocean Sci. Discuss., <https://doi.org/10.5194/os-2021-101-RC1>, 2021

- Brief summary

The authors investigate whether active acoustic data can be used to characterize the thermohaline stratification in an oceanic oligotrophic area. For this, they use CTD profiles and scientific echosounder data from two surveys, comprising different stratification levels and day and night acoustic records. The thermohaline structure is characterized using pre-established algorithms on CTD data. Then, the acoustic profiles recorded after each CTD stations are compared with the hydrographic ones to try to infer the feasibility of estimating the latter from the former by using three different approaches: (1) a cumulative backscattering threshold, (2) by matching a particular backscattering gradient value and (3) by crossing the thermohaline and acoustic profiles with wavelet techniques. After some analysis, the authors conclude that the task is not feasible in this area using any of the applied methods.

- General comment

The manuscript addresses an important question: trying to use the high resolution and continuous acoustic recordings to infer the thermohaline structure of the seawater column. If successful, this would be an important finding, as it would provide a way to interpolate between the discrete individual CTD stations or even avoid performing them completely, which would save a lot of survey time. Although the methods applied (especially the last one) are potentially useful for the task, the authors come out with a negative answer on the question, which hampers the interest of the findings. But in my opinion the manuscript seems not mature enough for publication in its current form because of various reasons that I try to detail below: (1) The manuscript feels careless, more like a report than a paper. (2) There is a lack in summarization of the results, consisting mostly in a chaotic

series of anecdotal examples. (3) The ms lacks also any proper testing of the hypothesis. And, finally, (4) the lack of positive results seems not sufficiently justified. For these reasons, I recommend rejecting its publication.

- **Major comments.**

- **Careless manuscript.** The manuscript feels careless, badly structured, with insufficiently described methodology (the variables of some equations not defined, the actual algorithms to estimate thermohaline structure from acoustics not described), making it very hard to follow and understand. In addition, the quality of the figures is very poor (some are unreadable), and the legends of the figures are incomplete and disorganized. For example, you must reach Figure 8 to understand some features of Figure 2. It looks much more like a quickly written report than a paper.
- **Lack of summarization.** The authors don't try to summarize the different structures of the different areas or seasons. And they don't try to summarize the acoustic profiles in the same areas either. Instead of summary representations, in most cases, the figures represent mere anecdotal examples. The result is rather chaotic and confusing, especially given the wide range of different scenarios (different seasons, years, periods and frequencies) that they present.
- **Lack of proper testing of the hypothesis.** There is no real attempt to predict thermohaline structure from the acoustic data, and, as such, there is no statistical analysis determining the performance of each method. Instead, the authors just plot some examples of acoustic data concurrently with some thermohaline profiles and the location of their main features. From these, the authors extract some patterns of vertical distribution of acoustic energy in relation to the main thermohaline features. According to the result of this qualitative comparison, they seem to refuse estimating the main thermohaline structure features. In other words, the ms seems to deliver, rather than an analysis, a preliminary visualization of the data, previous to the analysis itself.
- **Conclusions not sufficiently justified.** Not only the paper obtains a negative result: It seems that much more could have been done to try to estimate the thermohaline structures' features. Perhaps I could agree with the authors that the illustrative examples shown "suggest" (they can't prove it, as there are no statistical tests) that the attempted methods don't seem to be able to predict thermohaline features *individually*. But what about combining their capacities using, say, machine learning techniques? Or what about trying to interpolate the main features between CTD casts? Given the effort of the paper I don't think that we can conclude that "This result prevents for a fine-scale representation of the upper-layer turbulence from acoustic data" as they state in the abstract.

- Minor comments

- Lines 123-124: I suppose that the LTD is calculated as the first depth that fulfils that increase of temperature is 0.1 °C or higher. In addition, I assume the authors didn't apply derivatives on the signal, and they instead calculated something like: $\text{Temp}(z+1) - \text{Temp}(z) \geq 0.1$
- Lines 129-132. I don't see the point of calculating MLD, a depth that sometimes is located in the middle of the mixed layer and sometimes at its edge. I think that with UTD and LTD, the thermocline would be sufficiently described. Maybe I'm wrong, but then the authors should justify it
- Lines 133-134. The BLT is not mentioned anywhere in the rest of the manuscript.
- Lines 140-144. The explanation of the cumulative method is vague and disorganized (partly described in the introduction). It is not clear what is the particular algorithm the authors want to apply to determine LTD from the cumulative backscattering.
- Lines 147-149. it's not surprising that gradient methods are used to find (by definition) gradients.
- Lines 150- 159. is the CWT applied for CTD profiles on a single profile each time? Then, how is the scale obtained? How is the CWT applied for Sv profiles: is it applied to a single (averaged) profile or to the 100 pings?
- Figure 2. It took a while here to figure out that the MLD, UTD and LTD are all obtained from CTDs and not from acoustics using different methods.
- Lines 160-168. A lot of missing information in the figure legend:
 - what is the color code of (d)?
 - what are the arrows in (d)?
 - what is the y axis in (d)?
 - What is the solid black line surrounding the yellow areas in (d)?
- Figures 3 and 6. Unreadable annotations.