

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2021-668

Anonymous Referee #2

Referee comment on "Atmospheric stratification over Namibia and the southeast Atlantic Ocean" by Danitza Klopper et al., Atmos. Chem. Phys. Discuss.,
<https://doi.org/10.5194/acp-2021-668-RC2>, 2021

Review for "Atmospheric stratification over Namibia and the southeast Atlantic Ocean" by Klopper et al.

This paper used retrievals from global positioning system radio occultation (GPS-RO) and radiosonde profiles to describe the characteristics of atmospheric stratification over Namibia and in the nearby regions.

While the research topic may be relevant to the broader understanding and characterization of the atmospheric boundary layer over the southeast Atlantic and southern Africa region, the overall presentation of the analysis in this paper is poor. For the most part, I also find the paper difficult to read! As such, I believe the authors may need to substantially rewrite the paper to make it easier for readers to understand.

The difficulty of understanding this paper occurs at several levels, and because of that, I find myself reading each sentence and paragraph more than I would like to admit. For example, a lot of the paragraphs are not well connected structurally, which makes it difficult to understand either the point of the paragraph or its relevance in the context of the section it belongs to. In addition, it is difficult to see the big-picture relevance of this analysis, whether in the introduction or anywhere else in the paper. The authors must ask (and answer the question): Why should the reader care about this work?

Another point to consider is that the results section presents values that sometimes have larger uncertainties than the average values themselves. Such large relative uncertainties render the results useless for practical purposes. For example, the authors find an error of $-0.30 \pm 1.30^\circ\text{C}$ for temperature from GPS-RO when compared to the radiosonde below 7 km amsl. The same is true for cases above 7km or for the scatter plot in Fig. 3 (790 ± 990 m).

Almost all the figures lack detailed information that can let the reader better understand exactly what they are meant to convey. In addition, the authors showed seasonally averaged figures of standard deviations. Some of these figures are not discussed in the text.

Finally, the authors used several different methods to calculate the inversion. However, while discussing them in the results section, the authors sometimes did not clearly specify

which of the three methods they referred to.

Other Comments:

- Line 8-9: Why? Why is there "a limited understanding of the spatial and temporal variability in vertically stratified atmospheric layers over Namibia and the southeast Atlantic"? Please be more specific.
- Line 18-21: This sentence says that the two profiles have a "good agreement" and then says that one profile underestimates the other. It is either one or the other. I suggest the authors remove the "good agreement part", and rewrite the entire sentence for better clarity.
- Line 14: minimum gradient or minimum vertical gradient?
- Line 24: What does it mean to "found correlations in the character"? That statement needs to be clarified.
- Line 33-57: It is difficult for me to understand the point of this introduction. I will suggest that the authors rewrite it, paying close attention to telling the readers exactly why they should care about this study.
- Line 78: What a priori information? This place needs appropriate references.
- Line 83: "several times a day"? What time? You could provide a temporal interval.
- Line 90: "The Abel inversion algorithm was applied...." By who? Additionally, the whole sentence should be rewritten for clarity.
- Line 81 - 86: These lines mentioned "data" several times, without clearly specifying what data. Is this what the instrument measure? What exactly is it? What are the "raw data" separate from the "atmPrf" dataset that the authors mentioned?
- Line 135-136: "... were on average 100 m higher in the autumn and 100 m lower in the spring" than what?
- Section 4: There are several places where the words like "define" or "definition" were used to signify the calculation of the boundary layer height. For example, in line 149, the authors stated that "...was performed based on four different definitions of BLH...". I supposed the authors meant the four different methods used to calculate BLH. Would you please rewrite this section to reflect the right language that can better improve the clarity?
- Line 187 & Fig. 2: The text mentioned that the temperature profiles from GPS-RO are taken for the same day as the radiosonde. Is this an average over the entire day or the one with measurement time closest to the soundings? Please clarify.
- Line 200: Delete "where".
- Line 221: Change "high cloud fractions" to "high fractions"
- Line 220-224: The authors should examine (and possibly include in the supplementary document) the cloud distribution/variability for the periods that are compared.
- Line 224-227: Given the large discrepancies and the fact that the authors have no explanation for the 6 data points, I don't believe they can make this conclusion based on the exclusion of those "bad" 6 points
- 4: The authors should include a figure of the climatology that uses all the years in the supplementary document. That would allow readers to place the result of 2015 within a climatological context with all the uncertainties. Is this what is shown in Table S-1? If so, that is not clear in the main text and caption!
- Line 238: "equivalent" or similar? Also, the BLH values are not similar, the BLH for VPT is about twice that of surface-based inversion. However, the monthly variation or monthly consistency is similar. The authors should rephrase this sentence.
- Line 252-254: Comparing all data and not a subset of the data sounds like a more "sensible comparison" to me.
- Line 254: Do you mean the monthly variability? Fig. 6 shows monthly variability, not inter-annual variability. I also notice this in another part of the text. Please change all of them accordingly.

- Line 254-255: how different would this estimate/assessment be if all data were included?
- Line 266: Given that you talked about the difference between land and ocean, I wonder if this statement refers to zonal gradient and not "meridional" gradient.
- All Figures and Tables (main text and supplementary): Please all figure captions should be full and complete -- Meaning that it should include all the information used to make the plot (time range, date name, and so on), regardless of whether that information has been stated in the text or not. Also, all acronyms/abbreviations should be defined, irrespective of whether it has been used elsewhere or not.
- Line 491-493: This attribution was stated as speculation within the text. To make this type of conclusion requires more than speculation. I will suggest the authors better clarify their statement or remove it altogether.
- Line 505-508: Again, I don't see where this analysis was laid out in the result section of this paper. If the authors are making speculation, they either have to back his by previous studies that have made this conclusion or clearly state that they are speculating.