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Response to comments on "Atmospheric stratification over Namibia and the southeast Atlantic Ocean" by Danitza Klopper et al.

The authors would like to thank the two anonymous referees for taking great efforts and time to read and provide very useful and constructive criticism on the manuscript. Both referees commented on the poor structure and flow in the paper (and the need for improvements), and the need to clearly outline the scientific contribution of this work. Both referees also agreed that if the comments would be fully addressed and the paper re-worked, it could make relevant contributions to our broader understanding of atmospheric stratification over the southeast Atlantic and Namibia. We are extremely grateful for the extra eyes and inputs and have made extensive efforts to address the comments from each referee in detail (starting with referee #1 followed by those of referee #2). Each comment is listed below along with our *accompanying responses* below that. Sections inserted into the paper are given in blue.

Comments from both authors motivated for;

- The restructuring of the paper for improved flow and ease of understanding.
- Clear communication of the scientific contribution.
- Improved connection between the satellite and the radiosonde portions of the analysis.
- Improved connection between the different BLH definitions just using the radiosonde data.
- Re-evaluation of the calculation of superrefracted signals, which was changed from initially being determined from a smoothed gradient over a sliding window, to rather identifying the individual points and removing those profiles.
- A change of the area plots (showing BLH and inversions across latitude and longitude) to zonally averaged plots where the regional variability across diurnal and seasonal scales is easier to identify and compare.
- The results and discussion sections were combined to present our findings alongside the relevant literature and in the context of the broader meteorological picture.

After incorporating the responses from both referees, the manuscript was certainly improved and we hope that it is now acceptable for publication in ACP.

Referee #2: specific comments

1. 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 ± 1.30°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 ± 90 m).

The high standard deviations are indicative of skewed data, which yes, might be better described using other statistical summaries, but not in all cases. In consideration of this, we included additional frequency distributions (Fig. S.2. and S.5.) which helped to illustrate this, and we updated the text at ***3.2. Boundary layer heights > 3.2.1. Seasonal variability*** to include, *"...and the frequency distributions are given in Figure S.2. In general, there is a gradual increase in primary hBP over the ocean away from the coastal margin, and an abrupt increase in primary hBP from the coastal margin over land. Figure S.2. illustrates the high variability of BLH over land, and the preferential formation of lower BLH over the ocean and coastal margin.*" and at ***3.3. Temperature inversions > 3.3.1. Seasonal frequency, height, strength, and depth***, we added "*Cold marine surfaces further enhance regional atmospheric stability, where seasonal variability is minimal and inversions frequently form at low-levels along the coastal margin between 500 and 750 m (Fig. S.5.), and over the ocean between 500 and 1250 m (Fig. S.5.), decreasing in height towards lower latitudes (Fig. 7 and Tab. S.1.).*"

2. 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. 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.

Thank you for this. All figure and table captions were updated accordingly.

3. 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.

Only one method was used to calculate the inversions, and three were used to calculate the BLH, for which clear, concise abbreviations have been defined and used in the text to remove any uncertainty in this regard.

Additional comments

4. 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.

For clarity, this was updated in the introduction as follows, "Above the boundary layer, the spatial and temporal features of temperature inversions over the region have not been extensively studied using high spatial and temporal resolution data, despite their importance for the elevated transport of aerosols over the west coast of Namibia."

5. 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.

The entire section was re-written and placed in the context of other author's findings. We also explored the reasons for these differences in context.

6. Line 14: minimum gradient or minimum vertical gradient? **AND** Line 24: What does it mean to "found correlations in the character"? That statement needs to be clarified.

The abstract was re-written for clarity. The only other mention of "minimum gradient" was changed to "minimum vertical gradient".

7. 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.

The introduction was re-written as suggested with a greater focus on the importance of the research and finished off with the intended contribution of this study.

8. Line 78: What a priori information? This place needs appropriate references.

*To clarify, we updated the **section on 2.1. Region of investigation** to include, "This region along the coastal desert represents a transition zone between the cold Benguela current and arid Namibia (Preston-Whyte, Diab and Tyson, 1977; Cosijn and Tyson; 1996; Garstang, et al. 1996; Tyson and D`Abreton, 1998; Ao et al., 2012)" along with the relevant references.*

9. Line 83: "several times a day"? What time? You could provide a temporal interval.

*The supplementary materials include **Figure S.1**: Frequency of COSMIC GPS-RO measurements by month and year made over the ocean, coastal margin and land. Additionally, the number of measurements made across 4 four 6-hour intervals over the region are given in **Tables S.4, S.5, S.6**.*

10. Line 90: "The Abel inversion algorithm was applied…." By who? Additionally, the whole sentence should be rewritten for clarity.

*Following the suggestions and in the interest of improved structure, the entirety of **section 2 (Data and Methods)** has been reworked to clarify the pre-processing already done on the dataset by ECMWF and that post-processing performed for the article (**Section 2.2.**), and definitions used in data analyses (**Section 2.3**).*

11. 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?

*The **Data collection and processing** (now section **2.2**) has been updated to better explain this, thank you.*

12. 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?

These sections were re-worked as suggested and the use of "definition" was changed to "method" where appropriate.

13. 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.

The text was updated to specify "...co-located GPS profiles measured within 2 hours and 200 km of the radiosonde release from Walvis Bay…"

14. Line 200: Delete "where".
AND Line 221: Change "high cloud fractions" to "high fractions"
AND Line 135-136: "… were on average 100 m higher in the autumn and 100 m lower in the spring" than what?

These sections of text were significantly reworked and language edits were also made throughout considering reviewer comments and to ensure clarity in the text.

15. Line 220-224: The authors should examine (and possibly include in the supplementary document) the cloud distribution/variability for the periods that are compared.

Thank you for this suggestion. The supplementary materials were updated to include Figure S.4. Seasonally averaged cloud fractions for daytime measurements made between 2007 and 2017 (1° x 1° grid).

16. 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

Thank you for this comment, you are correct. We have included all the co-located pairs and instead just discussed different groupings, i.e. those that we more closely related in distance or time.

17. 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.

The entire section was re-written and hopes to provide clarity on these issues.

18. Line 252-254: Comparing all data and not a subset of the data sounds like a more "sensible comparison" to me.

Thank you for this suggestion. After consideration, all available months of data (2014-2016) were included on the condition that there were more than 10 measurements available for that month.

19. 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.

You are correct, and the text was updated throughout for clarity and consistency.

20. Line 254-255: how different would this estimate/assessment be if all data were included?

Considering that the methods for calculating BLH were re-evaluated and a more regionally relevant set of methods were chosen, all analyses were re-done and results presented in such a way to consider the comments above.

21. 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.

Meridional gradient is across lines of longitude, i.e. from ocean to land, so meridional is the correct use here.

22. 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.

The bigger differences between temperature near the surface has been shown (by other authors) to be due to the higher humidity, as in our case as well.

23. 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 this by previous studies that have made this conclusion or clearly state that they are speculating.

We trust the improved organisation and discussion addressed this issue, thank you.

References

Ao, C.O., Waliser, D.E., Chan, S.K., Li, J.L., Tian, B., Xie, F. and Mannucci, A.J.: Planetary boundary layer heights from GPS radio occultation refractivity and humidity profiles, J. Geophys. Res., 117(16), 1–18, doi:10.1029/2012JD017598, 2012.