Response 1

Major comments

1) In the last paragraph of the introduction, the authors set the stage for what the paper will present in the following way (italics are a direct quote of parts of the last paragraph of the introduction): „This study uses an 11-year climatology (2007–2017) of thermal atmospheric stratification below 10 km above ground level (AGL) over Namibia and the adjacent Atlantic Ocean, combining ERA5 reanalysis model data with GPS-RO observations from the COSMIC mission to evaluate the regional atmospheric stratification's spatial and temporal variability. It seeks to understand the impact of stratus clouds on the formation of the stratocumulus cloud deck and to investigate the relationship between the strength of atmospheric stability, as identified by the data, and the lower tropospheric stability (LTS), which has been shown to correlate with cloud fraction in the region." I have highlighted two statements that I will comment on:

Blue: This is the only mention of stratus clouds in the entire paper - there is no context given for this statement in the introduction, there is no analysis to this end in the paper, and no concluding statement.

Red: This is the only time that LTS is mentioned in the entire manuscript. It is never calculated, there is no analysis regarding this, and there is no concluding statement.

These statements are now being revised and carried along throughout the manuscript. Please refer to sections L29-35 and 3.3-3.5. Also, the last paragraph of the introduction has been modified as:

L62 "... to evaluate the regional atmospheric ..." >> *"to evaluate the regional ... Benguela low level jet intensity." Now in L69-75*

2) Lack of references: In some (not all) parts of the manuscript, there is speculation on the attribution of patterns to physical processes. In many cases, there is no evidence presented for these speculations, and often there are no references provided (see minor comments).

3) Section 3.1:

1) Questions about the data: GPS-RO: no data available below 500 meters, however, from the plots it looks like only the lowest 25hPa are not shown - this should correspond to about 200 meters? Please elaborate further.

GPS-RO data is available from 200 m and above, especially over the ocean, accounting for about 10% of the total profiles. Regions from the coastline inward into

the interior are mainly elevated 500 m and above the mean sea level. L104 and L133-136, which are meant to reflect this description, have been corrected appropriately.

2) L130: I agree with the authors that the data sets agree remarkably well everywhere in many atmospheric levels. However, this is not the case between 800 and 1000 hPa. This is clearly relevant as these are the pressure levels where the inversion typically sits to cap the boundary layer. I am not convinced by the strong correlation provided in L137 (r > 0.95), as this is clearly driven by the (very) good agreement of the two data sets above the capping inversion of the PBL. In my opinion, it would be much more meaningful to show correlations of the estimated inversion height/depth/strength between the two data sets. Also, I am questioning why the authors decided to use monthly mean data for the analysis. Using monthly mean data should cause biases in the estimated inversion characteristics, as this mixes situations with and without an inversion being present (e.g. leading to an inversion strength that is biased low).

The R-value given in the text represents the relationship from the surface to 200 hPa. Notably, the strong correlation is driven by the good agreement at higher altitudes, and this value decreases as the height approaches the surface. Hence, the R-values at 800 hPa (~ 2 km) and 900 hPa (~ 1 km) from the surface are 0.71 and 0.64, respectively, using the daily profile. The table below (now available in the main text) shows the correlation statistics of the two datasets at various levels relative to the surface. Also, plots of the corresponding correlations are attached as a supplementary figure (see Fig. B2).

L137 >> "... the temperature profiles ERA5 Vs GPS ... depending on the profiles range from the surface. Table 1." Now in L189

Profile ranges from surface (hPa)	*GPS Vs ERA5 R-value*
> 600	*> 0.90*
600	*0.89*
700	*0.80*
800	*0.71*
900	*0.64*

3) In my opinion, it would be helpful to show the actual distributions of the inversion characteristics height/strength/depth from the two data sets and correlate these, in the best case for daily data.

The temperature profiles for the datasets are now zoomed-in on to show changes between 700 hPa and the surface. Also, an additional figure (i.e., Fig 3) is included to illustrate relationships in inversion strength/depth/top height between the datasets.

4) Section 3.2

1) L158: SB inversion is defined as an inversion between 0 and 50 m AGL, but how are these detected when the data has a vertical resolution of 100m (GPS) or at least 200m (ERA5)? It would be better if the authors clearly defined SB by criteria that can be represented with their data.

The definition for SB inversions is modified to represent inversions extending from the surface.

L158 >> "... identified: surface-based (SB) or radiative inversions... form above the surface." Now L226

2) L160: The results described here are hard to see in the figures, as always, the full profiles are shown. It would be much easier to follow if e.g. the authors just showed the seasonality/diurnal cycle and spatial gradients of the inversion characteristics in one dedicated plot each.

To ensure clarity, figures in the entire work have been adjusted to include a zoom-in insertion of plots from 700 hPa to the surface. This is applicable to the figure associated with L160.

3) L181: The authors are using reanalysis data at the levels 1000, 975 and 950 hPa to detect inversions, and argue that none are detected inland - how should they considering these pressure levels are subsurface there?

Figure corresponding to L181 description is now corrected to cover the vertical region where low-level inversions commonly occur (i.e., between 800 hPa and the surface). Hence, inversion over the land is accounted for in the revised figure. Please refer to Fig. 6

5) Section 3.3

1) In principal, connecting the inversion characteristics to aerosols and clouds is relevant. However, I believe the data selection to be a poor choice. CATS profiles are shown, which are available at 60m resolution, but are shown at 1km vertical resolution. In the top of each overpass panel, the monthly average temperature profile from reanalysis is shown (pixelated and not readable), which is then described as a „collocation". Again, the choice to use monthly mean data for the reanalysis seems to limit the information that one can derive, for individual cases. Regarding the CATS profiles, how can 1km vertical resolution be useful, when often times, the distance between the aerosol and cloud (and therefore inversion) layers in the SEA much less than that (Rajapakshe et al. 2017, Gupta et al. 2021)?

Truly, the CATs dataset is available in 600 m vertical resolution but for the sake of viewing aerosol dispersion both above and below the inversion top, we displayed the vertical profile up to 20 km. However, this has now changed in the revised figures similar to vertical profiles displayed in other figures. CATS figures are now displayed up to 7 km above the surface. Also, each plot is now based on a daily thermal profile from ERA5 and includes the inversion top height for the nearest time. (Fig. 11)

2) CATS seems to frequently classify aerosols as volcanic, which does not seem reasonable in this region, however, this is neither discussed, nor is there any uncertainty/quality discussion on the data.

This trend is owing to an error in the code line for the CATS plot, and it is now resolved in the revised figures. A wrong mapping of the colours linked to the different aerosol types gave rise to this issue.

6) Section 3.4

1) L300-303: This is an interesting point to make, and indeed the seasonality between the SB inversion seems to agree with the occurrence of fog (directly at the coast: The paper by Nagel is for Swakopmund). This topic justifies a closer look, though, as other locations in the Namib feature a different fog seasonality (that is related to the EL inversions), peaking in spring and summer (Lancaster et al. 1984, Spirig et al. 2019, Andersen et al. 2019).

Truly interesting, we included some discussions to reflect the process in section 3.6 (L400) and some other parts of the paper. In summary, fog formation at the coast and inland seems to depend on the characteristics of the inversion base height aside from the elevation differences and air advection.

2) Fig. 9: It is unclear what precisely is shown here, what is meant with average cloud cover over the region (which region?), and which locations are used for the inversion characteristics?

Fig. 9 is meant to show the monthly characteristics of cloud cover and temperature vertical profiles over 20S and 8-19E. This figure uses low-level CF as a proxy to stratus/stratocumulus cloud fraction and aims to illustrate the temporal effect of inversion cap on low-level, mid-level and high-level clouds.

3) Looking at the scatter plot, I am surprised to see that the two variables are still fairly strongly correlated (r = 0.68 and 0.59). In particular inversion strength and cloud cover seem to be anti-correlated when removing the data points that do not feature an inversion. Is this true, and how many data points are this?

According to our result, this is true and consistent with findings from other studies (e.g., Wood and Bretherton, 2006; Cutler et al., 2022). Basically, the inversion intensity is only one out of multiple conditions linked to low cloud formation.

Therefore, the correlation values only reflect contributions of these features to cloud fraction. Meanwhile, a new figure based on daily data is now provided.

7) Structure of the manuscript: I was confused about the sectioning of combining results and discussion in Sec. 3, then in each of the sections 3.1 to 3.4 results are described and discussed (so far ok), but then Sec. 3.5 is named Discussion. This entire section is speculative, and provides very little references to other literature.

Section 3.5 titled Discussion is removed from the revised manuscript.

Minor points:

• L18: I am surprised to see the authors state that Namibia partially lies in the mid-latitudes when its southernmost point is < 29°S

L18 >> "*Geographically positioned in the subtropics, Namibia's mean atmospheric ...*" Now L19

• L28-45: This part of the introduction is not easy to understand and follow and needs to be restructured.

L28-45 >> "Atmospheric stability, characterised ... inhibits vertical exchanges between the BBA layer and clouds." Now L29 - 52

• L80: The vertical resolution of the ERA5 levels data needs to be described.

This statement has been revised as below

L80 >> "This work mainly utilises ... ." Now L88

• L105-106: „The spatial and temporal distribution of valid profiles across the study area is given in Fig. 1." This figure seems to be missing from the manuscript.

Figure is now provided as Fig. B1 and sentence modified as below

L105-106 >> "The spatial and temporal distribution of valid profiles across the study area is given in Fig. A1." Now L133-134

• L108: I guess this refers to the GPS data, as the reanalysis has a vertical resolution of 25hPa?

Yes, this description is for the GPS dataset.

• L120: Information on the vertical resolution of the data set is missing but relevant.

The dataset vertical resolution provided and L120 modified as below;

L120 >> "This work further uses the 0.6 x 5 km (vertical X horizontal) resolution CATS Level 2 version-3 aerosol ..." Now in L144

• Fig. 1: The bar scale of the map seems to be wrong (at 23°S a 1°x 1° box has length scales of around 100km, but the 5° coastal margin corresponds to less than 400km with the bar scale)

Fig. 1 now corrected.

• L133 The authors mention EL inversion and SB inversion here, however, it is not described how these are defined and the term SB has not been established. I see that it is discussed further down in section 3.2, but it would need earlier explanation at the very least.

EL and SB inversion are now defined. L183-187

However, the modelled data slightly differ at the point of inversion detection, resulting in varying values of the inversion depth and strength between it and the GPS, especially for EL inversion. These differences partly result from the contrast between the two data vertical resolutions. Nonetheless, and in most cases, the inversion base height (hib) is formed at about the same altitude in both SB and EL inversion datasets. Notably, most GPS measurements do not cover heights below 200 m above ground, which could affect the effective determination of SB inversion. As stated earlier in the previous section under data description, only data points above this height are considered for GPS analysis. In contrast, profiles of heights below 200 m are constantly shown for ERA5. Irrespective of these discrepancies, the temperature profiles ERA5 Vs GPS demonstrated good agreement with strong correlations (i.e., $R > 0.95$) across the study period. Nevertheless, this correlation value reduces at elevations nearer to the surface. In the case of the temperature profiles between the surface and 3 km above, the relationship demonstrates $R > 0.8$ and $R > 0.7$ for altitude 2 km above.

• L138/139 The uncertainty numbers provided are not clear - what are the units here?

The units are in degrees Celsius.

L138/139 >> "... ERA5 modelled data relative to the GPS observation is ±0.014 °C, ..."

>> "... is significantly higher (i.e., ±0.11 and ±0.17 °C, respectively) than at high ..." Now in L192-195

• L164-168: For the described causal links neither results nor references are provided.

Results regarding the causal links now presented in section 2.3

• L185: unit missing, also in the related figure.

The quantity is based on percentage, hence provided in the revised manuscript. Also, the statements in L185-194 are now revised to reflect the percentage frequency estimation between the surface and pressure height 800 hPa. Please refer to L249

• L188-193: References missing

Entire section revised.

• L194: Unit missing

Unit provided in revised version.

• L195: „Like observed across the maritime environs“ please correct this

L195 >> Like observed across the maritime environments, …“ Now in LXXX

• L204-212: References missing

Entire section revised.

• L230: Reference missing, in my opinion, most studies point to fog being advective

Sentence revise (L285)

• L234/235: The „other associated factors“ seem purely speculative, and no analysis regarding these is presented.

References provided and related analysis provided in section 2.3

• L242-252: References missing

References provided. Now in L353-359

<u>Other corrections/clarifications</u>

L104 “Between 2007 and 2017, 60 % of the profiles were usable from 100 m agl and another 20 % from 200 m agl.” **>> *“Between 2007 and 2017, 60 % of the profiles were usable from 600 m agl and above, of which over 10 % were from 200 m agl.” Now in L132***

L120-122 “The classification scheme identifies … vertical levels in the work.” **>> *“The classification scheme … specific vertical region.” Now in L149***

New section 2.3 (Synoptic weather pattern over Namibia) added

L127 “Only the locations within longitudes 8, 10, and 13° E were available for the GPS plots during this study.” >> ***“Only the locations … shown for ERA5 where applicable.” L176***

L129 “ From the plots, the ERA5 temperature profile demonstrates strong agreement with the GPS observation during the study period.” >> ***“From the plots, … evaluated depth and strength. L181***

New section added (Section 3.3: Atmospheric (AS) and lower tropospheric stability (LTS) characteristics of the lower strata)

New section added (Section 3.4: LLJ characteristics of the strata)

Section 3.4 (Interrelation between the vertical structure and stratocumulus deck form over the Atlantic Ocean) revised, now Section 3.6

References

Cutler, L., Brunke, M. A., & Zeng, X. (2022). Re-evaluation of low cloud amount relationships with lower-tropospheric stability and estimated inversion strength. Geophysical Research Letters, 49, e2022GL098137. https://doi.org/10.1029/2022GL098137

Wood, R., & Bretherton, C. S. (2006). On the Relationship between Stratiform Low Cloud Cover and Lower-Tropospheric Stability. Journal of Climate, 19(24), 6425-6432. https://doi.org/10.1175/JCLI3988.1