Author Responses to Reviewer Comments

Third Round of Peer Review

We greatly appreciate the constructive comments from the reviewer and the editor. We have carefully considered all the comments and have provided detailed and thoughtful responses to each concern.

Major revision

Editor comments on Report #1 dated 2024-05-5

The issue raised by the Reviewer needs to be properly addressed in an updated version.

Reviewer Comments from Report Dated 2024-05-05

I am happy to see a much-improved manuscript. However, there are still some issues that needs to be addressed before I can recommend publication.

We appreciate the reviewer's comments on the interpolation issue and its potential impact on the scientific results of the paper.

The authors would like to reemphasize that the major results of this paper focus on the high-resolution radiosonde data. The interpolation method issue raised by the reviewer does not affect the radiosonde data analysis, but may affect some of the ERA5 data analysis as the reviewer pointed out. We have clarified the point that some results related to ERA5 data analysis could be affected by the interpolation issue which we hope addresses the reviewer's concerns in addition to further examination included in our responses. Please see below for our detailed responses.

1) Figure 2d (and its implications) is still a major problem in my opinion. The authors have now made clear that they use quadratic interpolation (now written in the text), and that the calculation of the derivative also uses quadratic interpolation (in their answer to me). This means that the second derivative becomes constant in intervals (and is equal to the first derivative of the gradient). From their explanation, I think I can understand why this procedure can result in both short (about 20 m) and longer (about the distance between model levels) intervals with constant first derivatives of the gradient, which appear as the sawtooth-like features. In the text the authors now write: "Note that the weak gradients seen above the minimum in the ERA5 refractivity gradient (Fig. 2d) are a result of the vertical derivative being calculated from the interpolated ERA5 refractivity profile and do not appear for larger interpolation intervals suggesting that the non-linearity of the ERA5 vertical grid at this height affects the vertical gradient." I don't understand why the non-linearity of the ERA5 vertical grid gets the blame, I think it comes from the interpolation method of using only quadratic interpolation (cubic interpolation would have been a better choice). In any case, the sawtooth-like features in Figure 2d are not only the weak ones above the minimum gradient. The minimum gradient itself has a short interval right above (about 20 m), and a longer interval right below (about 150 m) with

constant first derivatives. As I see it, this is a sawtooth-like feature right at the minimum gradient.

a. **We apologize for the confusion in our previous response. The "non-linearity of the ERA5 vertical grid" refers to the uneven vertical sampling intervals of the ERA5 data, i.e., much larger sampling interval at higher altitude, which is contrary to the relatively constant sampling interval (~ 8 m) in radiosonde data.**
b. **Note that the raw vertical sampling interval of ERA5 refractivity profile of ~ 160-200m between 2 km and 3 km. The refractivity profile is interpolated into 10 m intervals with three different interpolation methods (*linear, quadratic, and cubic spline*). Figure RS1 illustrates the original ERA5 refractivity profile (used in Figure 2 of the manuscript) and the interpolated profiles using each method (a and c), as well as the resulting refractivity gradient profiles (b and d). All three interpolation schemes lead to identical refractivity profiles. The "sawtooth-like features" in the gradient plot above and below the peak refractivity gradient at ~2.1 km are also evident in both linear and cubic-spline interpolation methods, in addition to the quadratic interpolation method used in this study. Detailed differences are more explicitly seen in the enlarged figure (right panels of Fig. RS1).**
c. **On the other hand, Figure RS2 demonstrates the same three interpolation methods applied on the colocated MAGIC radiosonde refractivity profile (native vertical sampling of ~8 m). The quadratic interpolation is almost identical to cubic-spline interpolation. This confirms that the high-resolution radiosonde data is not sensitive to the selection of the high-order interpolation schemes (quadratic or cubic).**
d. **Therefore, such fine "sawtooth-like features" are the results of the interpolation when the interpolation interval is much smaller than the vertical sampling interval of the original data.**
e. **Based on these results, the authors believe that a full reassessment of the data with a new interpolation method will not improve the results of this study.**

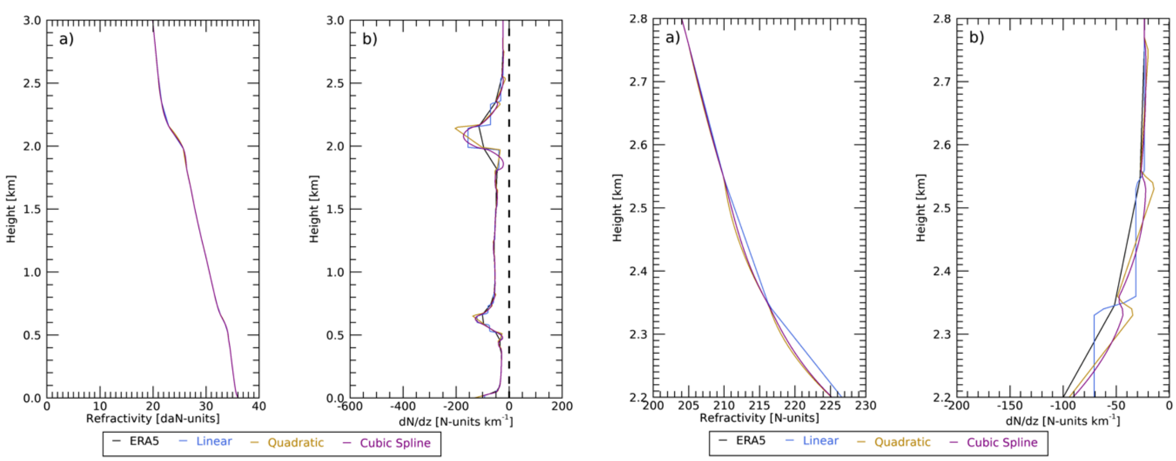

Figure RS1. (Left 2 panels): Comparison of ERA5 (a) refractivity and (b) vertical refractivity gradient profiles from Figure 2 in the original manuscript. Note the ERA5 has ~160-200 m vertical sampling interval between 2 km and 3 km. In both panels, raw ERA5 data (solid black) and 10 m interpolated profiles using linear (blue), quadratic (gold) and cubic spline (purple) interpolation schemes in IDL. (Right 2 panels): the enlarged portion of the left panels from 2.2 km to 2.8 km, to illustrate the gradient difference for three interpolation schemes.

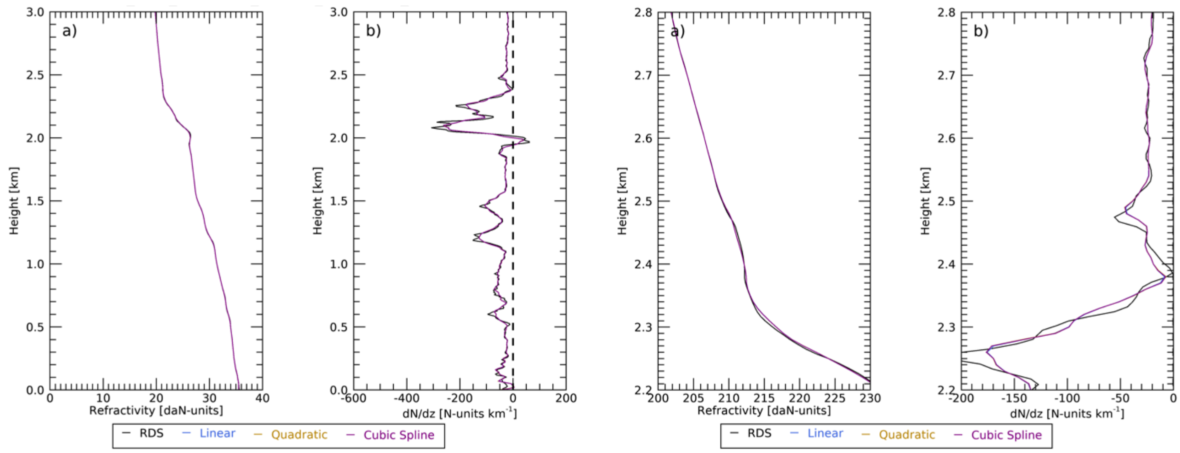

Figure RS2. Same as Figure RS1 above, but for the colocated MAGIC radiosonde (RDS) (a) refractivity and (b) refractivity gradient profile. Note the radiosonde profile has the raw vertical sampling interval of ~ 8 meters from surface up to ~30 km.

2) Further the authors write: "These features of approximately 15 N-units/km magnitude are only noticed in the plotting and do not impact the results of the study, as only the moisture-induced minimum gradient values are large enough in magnitude to exceed the minimum gradient threshold." I don't understand why the authors say that the features are only noticed in the figure. I assume that the figure shows the data, so the sawtooth-like features are in the data (due to the quadratic interpolation), not only in the figure. Maybe they mean that these minor gradients are not affecting the estimates of minimum gradient and height. However, the largest sawtooth-like feature right at the minimum gradient is more than 200 N-units/km. It may be that quadratic interpolation do not affect the estimate of the magnitude and height of the minimum gradient much, but that at least is unclear.
    a. **Sorry for the confusion. Yes, we believe that the quadratic interpolation does not affect the estimate of the magnitude and height of the minimum gradient, as the sawtooth feature is higher order fine structure in the gradient profile.**
3) On the other hand, I do think the method (creating these sawtooth-like features) may affect the estimate of the ducting thickness (see below). I strongly suggest that the study is done with a better interpolation method. If this is not done, then at least the authors should write in the text and in the conclusion that the results may be affected by the interpolation method and by how the gradients are calculated. Now that I think I understand how the sawtooth-like features appear, I am even more concerned that the results in Figure 6 and elsewhere are affected. It seems clear to me that the narrow (about 20 m) intervals of constant first derivatives near the minimum gradients are artificial, and a result of the interpolation method, and that this can affect the estimated ducting

thickness. In their answer the authors write: "As an example, an ERA5 profile that is not interpolated can have a minimum gradient value of -160 N-units/km and if the raw profile resolution at that point is 50 m, one could argue the ducting layer thickness is 50 m. However, a 50 m ducting layer thickness could be just as artificial as the reviewer is claiming for a 10 m thickness.". I disagree. The 50 m ducting layer thickness (if so estimated) would be what ERA5 suggests. It may not be the actual value of the ducting layer thickness in the real atmosphere, but I think that is irrelevant here. Due to the coarser resolution ERA5 profile, the 50 m thickness (if so estimated) would be the right number. Thicknesses of only 10 m, if they are a result of the interpolation method, would be wrong. Again, I urge the authors to repeat the study with a better (higher order) interpolation method, or clearly write that the results may be affected by the interpolation method and by how the gradients are calculated.

a. **The authors understand the reasoning for the reviewer's strong opinion on the interpolation method. However, changing the interpolation method would not remove the presence of the "saw-tooth like features" as seen in Figures RS1 and RS2. As stated previously, the issue arises from calculating the gradient of a profile that was interpolated to a higher resolution than the raw data. To remove the possibility of this occurrence, the resolution of the interpolated profile would have to be decreased to the coarsest vertical resolution from the ERA5 data in the lowest 3 km, which would be approximately 200 m. If this were the case, then there would be no point to using the radiosonde data as it would need to be interpolated to the same resolution for a reasonable comparison.**
b. **We agree with the reviewer that quadratic interpolation could introduce higher-order fine structure in the gradient plot right next to the sharp gradient (e.g., the PBL height). However, we believe that the PBL height detection is not affected.**
c. **As stated in the response for reviewer comment #2, such fine "sawtooth-like features" are the results of the interpolation when the interpolation interval is much smaller than the original data vertical sampling (e.g., ERA5). The authors believe that a full reassessment of the study with a new interpolation method will not improve the results of this study.**
d. **The authors believe the research is sound and the findings are of note, especially since the MAGIC data are not subject to the shortcomings of the interpolated ERA5 data. The authors believe that the methods of analysis used on the ERA5 data in this paper will also provide an example of the differences when high resolution model data are interpolated and compared to high resolution radiosonde data. We feel that a new iteration of the study with updated methods of both interpolation and calculation of the gradient would not offer the solution the reviewer suggests; however, we have no issue with giving full disclosure of the methods used and acknowledgment that the current methods may impact some of the results for the ERA5 data**

i. **Section 2.3, lines 174-180, "It should be noted that the weak "saw tooth-like" gradients seen above the minimum in the ERA5 refractivity gradient (Fig. 2d) are a result of the vertical derivative being calculated from the interpolated ERA5 refractivity profile. When interpolating the relatively coarse vertical resolution ERA5 profile (up to 200 m in the lowest 3 km) into 10 m vertical sampling, the higher-order interpolation could lead to fine structure in the first order derivative. However, these minor gradients do not affect the estimates of minimum gradient and associated heights."**

ii. **Section 4, lines 447-449, "Further, the ERA5 results may be affected by the interpolation resolution and gradient calculation. Both warrant a more comprehensive study in the future."**

e. **Thank you for the comment acknowledging that the author's intention to state that the minimum gradient used to identify the PBLH is not affected by these weak gradients was unclear. The reviewer's sentence stating this explicitly has been added at lines 177-178.**

4) The authors followed my suggestion and removed section 3.3.3. Because of that, they also removed equation 2, which means that the sharpness parameter is no longer defined. However, the sharpness parameter is still discussed and shown in figures (Fig. 4c). Maybe equation 2 just needs to be reintroduced. Please revise.

a. **Thank you for pointing this out. The paragraph that introduces the sharpness parameter has been reintroduced (lines 138-143) as follows:**

i. **"To assess the robustness of the PBLH detection with the gradient method, Ao et al. (2012) introduced the sharpness parameter ($\widetilde{N}'$) to measure the relative magnitude of the minimum gradient, which is defined as the ratio of the minimum vertical refractivity gradient (N'_{min}) to the root mean square (N'_{RMS}) of the refractivity gradient profile from surface to 5 km as follows:**

$$\widetilde{N}' \equiv -\frac{N'_{min}}{N'_{RMS}}, \qquad \textbf{(2)"}$$

5) line 162: 'N-units/10' is still not the right unit. I believe it should be '10 x N-units' or deca N-units (just like km = 1000 m; when using km instead of m it reduces the numbers on the axes by a factor of 1000). However, here units for the other variables are not specified, so why for refractivity? - one could just say (N) here. Please see my comments on earlier revisions about the N-unit issue in Figure 2 and revise.

a. **The units on the x-axis of Figure 2 have been revised according to the reviewer's suggestion and the text has been modified accordingly.**

6) Although the authors have now removed the reference to Table 1 when discussing Fig. 9, the numbers in Table 1 are still the same and thus not consistent with Fig 9. I don't understand that. Why are the numbers for the peak N-bias in Table 1 different from the peak N-bias in Fig. 9? If the numbers are supposed to be different (in which case I misunderstood something), then please explain in the text why they are different. Otherwise please revise either Table 1 or Fig. 9 (and the numbers in the text).

a. **The numbers in Table 1 are intended to accompany Figure 8. Clarification has been added to line 351-352 with the following text.**
    i. **"Table 1 lists detailed statistics of the peak *N*-bias values at each bin for both radiosonde and ERA5 data seen in Fig. 8".**
b. **The numbers in Table 1 are included in the discussion of Figure 8 (lines 355-359).**

7) line 120: non-linear -> non-equidistant.
    a. **Text has been changed from "non-linear" to "non-equidistant".**
8) line 357: looks like it is more than 15 m in Fig 8.
    a. **The reviewer's observation is correct, and the numbers have been updated in lines 363-365 with the following text:**
        i. **"…maximum difference of 100 m (−157.5°) and a minimum difference of ~70 m (−142.5°) while the ERA5 PBLH shows greater values for maximum difference (140 m at −132.5°) and minimum difference (60 m at −157.5°)."**
    b. **The reason for the change is that upon checking the reviewer's noted inconsistency it was discovered that the data describing the difference between the PBLH and height of the peak *N*-bias were from a previous iteration of this calculation and were not consistent with the values depicted in Figure 8. This has been corrected in the text as described above.**
9) line 358: of -> difference.
    a. **Text has been corrected from "minimum of" to "minimum difference".**
10) line 419: approximately 80 meters below -> slightly below (also in abstract). The 80 meters came out of Section 3.3.3, which has been removed.
    a. **The text "approximately 80 meters below" has been changed to "slightly below" in both locations so there is no longer reference to a section that has been removed from the document.**

Editing Comments from Response Dated 2024-03-27

Notification to the authors:
Please ensure that the colour schemes used in your maps and charts allow readers with colour vision deficiencies to correctly interpret your findings. Please check your figures using the Coblis – Color Blindness Simulator (https://www.color-blindness.com/coblis-color-blindness-simulator/) and revise the colour schemes accordingly. => Figs. 2 and 6

- **Figure 2: The vertical profile lines are depicted in separate textures as noted in text, in caption, and the legend and they are consistent for all four panels within the figure. Additionally, the texture of the vertical gradient profiles in panels b and d correspond to the original variables in panels a and c, respectively. The ducting layers are described in a similar fashion with the top and bottom layer textured with dotted lines and the height of the minimum gradient identified with a dashed line. The green dashed line has been changed to gold as it is more distinguishable when viewed with each filter from the Coblis tool.**
- **Figure 6: The figure has been altered to use colors more agreeable to color vision deficiencies (red, gold, blue, purple). Additionally, instead of all open circles, each**

bin uses a different character (circle, square, diamond, asterisk, plus sign, triangle, and X). These combinations are identifiable using all filters provided in the Coblis tool. References in text and captions have been altered to reflect the change of this figure as well.