

Response to second two reviews

We thank the two reviewers for their careful reviews of our revised paper and their helpful suggestions. Our responses are included below in italics.

Report #1

Review Summary:

The authors have diligently addressed the comments of this reviewer. There are still some clarifications requested.

Detailed Comments:

Line 250 (track changes version): the word “Perceived” here does not seem quite right. A better word could be “Resulting”, or even no word at all.

Response: “Perceived” is removed as suggested in a revised sentence.

Line 331 (track changes version): “These are ...” seems to directly repeat the previous phrase?

Response: This was a typo. It has been corrected.

Figure 7 (original line 502): While the authors are correct that temperature uncertainty and water vapor uncertainty of ERA5 are expected to differ, there is no attempt by the authors to compare their determination of bending angle uncertainty with previous determinations of ERA5 uncertainty. Discrepancies between these results and the ERA5 literature may be present and require some explanation. Comparisons between ERA5 and radiosonde below 10 km altitude show roughly constant or gradually decreasing relative humidity uncertainty of ERA5 towards the surface, assuming radiosonde as “truth” (e.g. Gamage et al., Figure 2; Virman et al., Figure 4). Bending angle is sensitive to specific humidity. A constant relative humidity uncertainty with altitude would result in an increasing specific humidity or bending angle uncertainty with decreasing altitude as seen in Figure 7, but the sharp uncertainty reduction in the lowest ~1-2 km is not consistent with the radiosonde comparisons. What are possible explanations for the discrepancy between these results and the prior literature? Is there a sharp reduction in ERA5 temperature uncertainty near the surface that counteracts the increasing specific humidity uncertainty towards the surface in ERA5, so that bending angle uncertainty shows the pattern in Figure 7?

References:

Gamage et al. (2020), A 1D Var Retrieval of Relative Humidity Using the ERA5 Dataset for

the Assimilation of Raman Lidar Measurements, Journal Of Atmospheric And Oceanic Technology, doi: 10.1175/JTECH-D-19-0170.1.

Virman et al. (2021), Radiosonde comparison of ERA5 and ERA-Interim reanalysis datasets over tropical oceans, Tellus A, doi: 10.1080/16000870.2021.1929752.

Response: This figure is now Fig. 11. This is a good question. We do not think there is any discrepancy between the uncertainty profiles of the ERA5 bending angle (BA) in our study and uncertainty estimates of temperature and water vapor. ERA5 BA uncertainties are different from the ERA5 uncertainties of temperature or water vapor as discussed in the Gamage and Virman references. The maximum in BA uncertainty at about 3 km impact height and the sharp reduction below (Fig. 11 of our revised paper) is a common feature of uncertainty estimates of RO BA (e.g. Anthes and Rieckh (2018), Semane et al. (2022), Ho et al. 2023-references in the paper). We do not know of any references that explicitly explain the maximum of BA uncertainties around the top of the atmospheric boundary layer (ABL), but it is clear that it is related to the maximum vertical gradient of refractivity at the ABL top, as discussed by Ao et al. (2012) and Xie et al. (2012) and shown in Fig. 2 of the Xie et al. paper (reproduced below). The BA is related to the vertical gradient of refractivity, which can be extremely large at the top of the ABL.

This maximum in BA uncertainties at the top of the ABL is present in RO observations and models, which compute the BA from vertical gradients of refractivity using a forward model. Mean differences in the model ABL height and the ABL height estimated by the RO observations as found by Ao et al. (2012) may also contribute to the uncertainties.

The maximum in uncertainties in the observed and model RO profiles is not present in the temperatures and water vapor, as noted by the reviewer.

We have added an explanation of the large uncertainties and biases in models in Section 1.1 (lines 217-222) of the revised paper.

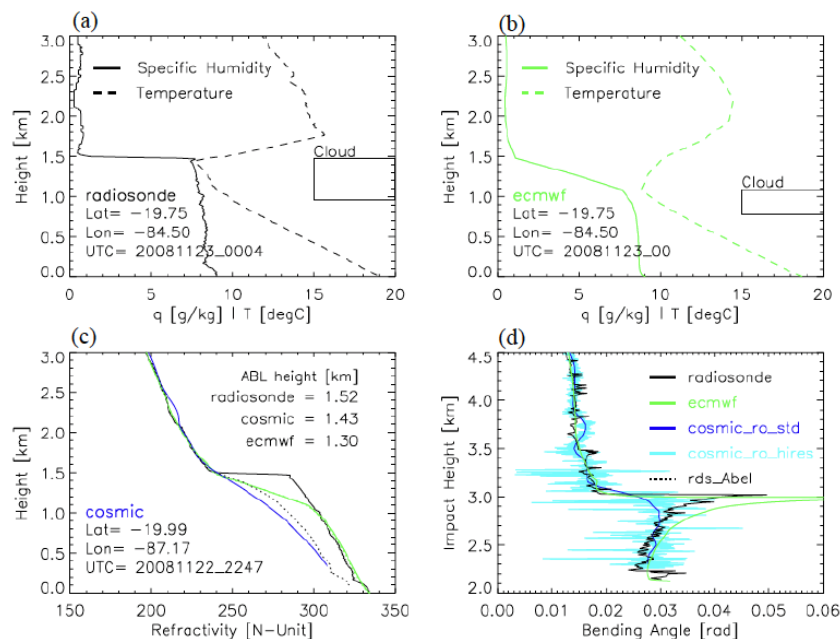


Fig. 2. Typical ABL structure in one VOCALS radiosonde (black) with the near-coincident COSMIC-RO (blue) and ECMWF analysis (green) profiles. (a) Radiosonde specific humidity and temperature; (b) ECMWF analysis specific humidity and temperature; The boxes in (a, b) indicate the cloud region with relative humidity exceeding 94%; (c) refractivity of radiosonde (black), COSMIC RO (blue) and ECMWF analysis (green) as well as simulated refractivity retrieval (black-dotted) based on the simulated radiosonde bending; the ABL heights are listed in the upper right corner, respectively; (d) simulated bending angle of radiosonde (black), ECMWF analysis (green) as well as the standard (blue-solid) and high-resolution (cyan) COSMIC RO bending angles.

Reference (the other references in Response presented in the paper):

Xie, F. et al. 2012: Advances and limitations of atmospheric boundary layer observations with GPS occultation over southeast Pacific Ocean. *Atmos. Chem. Phys.*, 12, 903–918, www.atmos-chem-phys.net/12/903/2012/doi:10.5194/acp-12-903-2012

Report #2

I am happy with most answers to my questions and changes to the manuscript. However, there are still a number of minor issues which I think should be addressed before I can recommend the paper for publication.

Below, I have indicated each issue by the original line numbers from my first review, which were repeated in the authors response, and with the corresponding line numbers from the revised manuscript in parenthesis.

L33-34(L33-34): The abstract should relate to the data that are analysed and presented in the main paper. Thus, I suggest to remove or qualify the sentence in the abstract that "They are similar on the average in the region of overlap (45°S-45°N)". The sentence is not correct for the data that are presented in Fig. 8 (between 10 and 15 km). Although I understand that a later

Yunyao processing has remedied the differences, the data that are presented are the ROMEX data that will be made available to the community (as I understand it).

Response: We prefer to leave this sentence in the Abstract. This statement is an important conclusion (it means that all RO missions can be treated similarly in NWP models) and is supported by the comparison of all 14 RO missions in Fig. 9 (revised version of paper), and the three CSY missions in Fig. 12 (revised paper) over most vertical levels between 0-60 km. The possible exception in the blip in the Yunyao uncertainties between 10-15 km and the reason for this blip is explained in the paper.

L87-88(L88-89): Both of the added references are not peer-reviewed, and one is a personal communication. It should be possible to find better references (the text says "widely studied").

Response: Schreiner et al. (2020) is peer reviewed. We inadvertently left the Bowler (2020) reference out of the revised paper; it is now included.

L91(L92): In the list of references (lines 1056-1063 in the revised manuscript), Cheng (2024) refers to the 1st ROMEX workshop, and Cheng (2025) refers to the 2nd ROMEX workshop. Thus, I think it should be (Cheng, 2024) here.

Response: Thank you; we changed the year to 2024.

L106-107(L108-113): There should be better (peer-reviewed) references than the ROPP guide, e.g., papers that it is based on.

Response: The ROPP guide referenced here gives a thorough, detailed discussion of the RO processing, much more detail than any peer-reviewed paper, and is readily available. It is the original and carefully reviewed document (internally) and we prefer to retain it in our paper.

L138-141(144-151): Refractivity biases above 40 km are shown and discussed in the Supplement. It would be relevant there to mention that there can be differences in refractivity biases at high altitudes due to different approaches in statistical optimization.

Response: We have added the following sentence to L718 in the revised paper "We note that the N biases above 40 km are affected by the statistical optimization, which can vary with different processing centers."

L144-146(L153-L158): It seems not right to measure/define "depth" in percent here. In line 156 of the revised manuscript it says "The penetration depth (lowest level reached)...", which makes more sense to me. I think the paragraph would be consistent by removing the words "depth" in lines 153 and 154, and the word "rates" in line 157.

Response: We have changed the wording in Lines 156-159 to read: "We estimate the lower tropospheric penetration depths (lowest level reached) of the RO profiles, the standard deviation of random errors (uncertainties), and biases. The penetration depths depend on the cutoff

criteria used in the processing, and so their comparison among different missions should be done with the same processing center.”

L180-183(L184-188): The title of the added reference to (Schreiner et al., 2020) is incorrect (line 1204 in the revised manuscript). It should be "COSMIC-2 Radio Occultation Constellation: First Results". The added reference to (Ho et al., 2023) is not in the reference list.

Response: Thank you. We corrected the Schreiner et al. 2020 reference and added the Ho et al. (2023) reference

L184-189(L189-194): I do not see the link to irowg.org/romex-events-meetings/ in the revised manuscript (as written in the response to my first review), but it is anyways not straightforward to find the presentations at the link. The reference to (Shao et al., 2025) is to the IROWG-10 meeting (which was before the second ROMEX workshop but does contain some information on what happened at the first ROMEX workshop). In addition to the reference to Shao et al., I suggest to include the links to both ROMEX workshops here.

Response: We added the link to the ROMEX workshops as suggested.

Figure 1: As I understand it, separate captions for each panel is not allowed according to the journal policy. I think the problem is best solved by renumbering 1a to 1e as figures 1 to 5. I don't see much relation that justifies keeping them as one figure.

Response: We have renumbered the different panels of Fig. 1 as suggested.

Figure 1a: The underlying world map with continents should be explained or removed. This is a plot in latitude vs local time where a world map makes very little sense.

Response: The faint background of the continents is present to give the reader an idea of the scale of the orbital distributions (for example, the scale of the gaps in local time coverage of Yunyao). We have added a statement to this effect in the figure caption.

L374(L391): I don't understand why is it called 3CH QC. There is no reference to 3CH QC in section 2.3. Perhaps it could be called "final QC" as it is referred to in section 2.3.

Response: We changed “3CH QC” to “final QC” as suggested.

L468(L492): The sentence has not been changed as written in the response to my first review.

Response: This was an oversight. The revised sentence in L511 (with the new Figure number reference) is: “The biases of all ROMEX missions with respect to ECMWF analyses appear very close to zero on this scale of the x-axis (Fig. 9), but a closer look shows a small negative bias of approximately -0.1% in most ROMEX missions between 10 and 35 km as shown in Fig. 10a.”

Figure 8: Perhaps say 'barely visible' in the caption instead of 'not visible'.

Response: Suggestion accepted in the caption to Fig. 12 (the new Figure 8).

L651-652(L696-697): I am very sceptical that ERA5 (including forward modeling errors) could have biases up to -15% at 3 km impact height. In Section 1.1 it says "such as errors in the coefficients of the refractivity equation". I suppose that cannot create what we see in Fig. 12a (I think letters should be added in Fig. 12 panels, as they are in Fig. 9 panels). I don't know if the use of a 1D model could be the reason. If so, it would be good to mention this explicitly here.

Response: Fig. 12 is now Fig. 16 and letters have been added.

These are mean differences between the CSY BA observations and BA computed from a forward model from the ERA5 data. We do not claim that the large (15%) biases seen over the tropical West Pacific are biases in the ERA5 data, they could be mostly due to the biases in the RO data. Also, it is important to note that the biases and uncertainties in the model BA do not necessarily imply biases and uncertainties of similar magnitudes in the model temperature or water vapor. The BA are a function of the vertical gradient of these model variables, and may also arise from systematic errors in the forward model, such as errors in the coefficients of the refractivity equation. We have added this explanation to lines 217-222 in the revised paper. However, the reasons for the large biases in the tropical West Pacific, which are present in all three CSY missions (Fig. S33 of Supplement), are uncertain and require further investigation.

L672-674(L717-719): The sentence could be misunderstood as "... mean and standard deviation of differences between BA and N". I think the whole sentence could be removed without loss of information.

Response: We removed the sentence as suggested.

L694(L749): My suggestion to say "biased relative to" was accepted in the response to my first review, but the sentence has not been changed.

Response: Another oversight—the sentence has now been changed as suggested.

L724-725(L778-782): I'm happy with the inclusion of the new Fig. 15, which shows a rather broad distribution of azimuth angles for both C2 and Spire. Thus, the text saying "predominantly oriented in an east-west (E-W) direction" seems a bit imprecise to me. I would rather say "predominantly oriented within $\pm 45^\circ$ of the east-west (E-W) direction ... occultation planes generally oriented within $\pm 45^\circ$ of the north-south (N-S) direction".

Response: We have made this change as suggested (L810-813) of revised paper).

L746(L807-808): Although I cannot immediately see the implication for the bending angle in the response, I think the authors are right. I suggest to say "... due to traversing a slightly longer path within an atmospheric shell ...".

Response: We have made this change as suggested (L843 of revised paper).

L801(L864): I don't understand how the correction was only applied to impact heights (as noted in the response to my first review). In the new Fig. 19 both BA and N corrections are shown. It is not clear how the N correction was applied as a function of altitude in that figure.

Response: The correction is applied to the impact height and then transferred to the impact parameter (by retaining the local radius of the best-fitted sphere to Earth's surface at the occultation point). The Abel inversion is subsequently performed to derive the refractivity, followed by the standard procedure to obtain the MSL altitude. Thus, the correction is applied only to the impact height, but its influence naturally propagates to the altitude.

We have added the following sentence to L904: "Assigning the retrieved BA to an adjusted impact height is effectively equivalent to modifying the BA for a given impact height. Consequently, this adjustment further influences the refractivity as a function of altitude through the subsequent Abel inversion."

L808(L871): "Difference ... minus". The sentence should be rephrased.

Response: This is the R_c across the ray path minus the R_c along the ray path so we think the wording is OK. We added parentheses (across minus along) which may help.

L827-831(L890): Typo: B should be N.

Response: Typo corrected.

L834-835(L891-893): I suggest to say "The reduction is smallest in the lower troposphere ...".

Response: We said the reduction is smallest at 10 km because that is the lowest level shown in Fig. 23 (old Fig. 19). However, the suggested change is better, and we have accepted it.

Figure 18 (Figure 19): It is unclear how the correction to N was applied (see earlier comment).

Response: Please see response to L801(L864) above.

L899(L960-962): As I understand it, the sideways sliding correction was applied to both bending angle as a function of impact height and refractivity as a function of altitude (Fig. 19). I'm unsure if this last statement refers to what was actually done in the paper, or if it is something that has been done only in a revised processing mentioned in the response (which is not otherwise mentioned in the paper). I suggest to mention only what was done in the paper.

Response: The last statement is correct (L1002) in revised paper), and affects both the BA as a function of impact height and N as a function of altitude as described above. It is what is done in this paper.

L906-907(L967-968): The data should be put on the ROM SAF ROMEX server. It was not there the last time I checked.

Response: We agree and are disappointed as well. We sent the UCAR-processed data to EUMETSAT in April 2024 and have repeatedly asked them to put the data on the ROM SAF ROMEX server, including numerous times since we submitted our paper in August 2025. Despite many assurances that the data would be there “soon,” it has not been done and we have given up. We have changed the wording to: “The ROMEX data processed by EUMETSAT are available free of charge through ROM SAF under the ROMEX terms and conditions. Further information is available at <https://irowg.org/ro-modeling-experiment-romex/>. The ROMEX data processed by UCAR are available from UCAR under the ROMEX terms and conditions.”

Supplement:

Fig. S2.1: I suggest to remove new sentence saying: "Precise version of top label is <<(CSY-<ERA5>)>>/<<ERA5>>. Same for Figs. S2.2-S2.8". I don't think it is explained anywhere what it means.

Response: We prefer to keep this brief note. It is defined in lines 44-45 under Fig. S1 in the Supplement.