First Round of Review of Manuscript AMT-2024-119 Submitted to Atmospheric Measurement Technology

Manuscript Title: Observing Atmospheric Rivers using Multi-GNSS Airborne Radio Occultation: System Description and Data Evaluation

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Study Summary:

This manuscript describes the use of airborne GNSS radio occultation for observing atmospheric river (AR) events that impact the west coast of the United States. These airborne radio occultation (ARO) observations are shown to be successful in observing AR events due to the inherent ability of ARO profiles to ignore clouds and precipitation, resulting in data that can observe the thermodynamics of ARs where other remote sensing methods fail due to low vertical resolution or signal attenuation. A ARO full observation and retrieval system is described, and retrieved results were compared to ERA5 model reanalysis output as well as in-situ dropsonde observations. Mean refractivity differences between ARO profiles and ERA5/dropsonde profiles was found to be less than 0.5% magnitude above 3 km with varying standard deviation that is higher at lower altitudes, indicating the high quality of the observations and their potential usefulness in numerical weather prediction of AR events.

General Comments:

This manuscript is generally well-written and provides very unique data and results. However, my major comment regarding the manuscript is that it is quite long, with more specific provided as part of my comments. Overall, I would recommend publishing this paper after the below comments and suggestions are sufficiently addressed. It is likely that I will not catch all errors, so I would encourage additional read throughs to find any additional issues. My overarching notes for this study are the following:

1. <u>General Readability/Structure and Grammar:</u>
    a. In general, I would avoid the use of phrases like “we implemented” or “our tests” in regard to the experiments that took place. Pronouns are generally not used in technical writing.
2. <u>Introduction and Motivation:</u>
    a. The introduction is generally well-written with only a few issues to fix detailed in the line-by-line comments.
    b. Based on a Google Scholar search, the authors do not mention recently published work on engineering of new airborne RO payloads and the use of commercial aircraft for airborne radio occultation (e.g., Chan et al., 2022; Xie et al., 2024). This would be most relevant to the discussion around line 110. Do the authors have a specific reason they chose not to include this

potentially relevant information? If not, I would encourage them to work relevant references into their introduction

3. Methodology:
    a. I was not able to find the authors' Equation 1 in (Rüeger, 2002) or (Rüeger, 2002). I am concerned that the authors are not using the correct formulation for refractive index and refractivity. The authors' equation 1 is especially concerning because the second term in Eq. 1 actually removes some of the contribution of water vapor rather than the traditionally-documented addition effects of water vapor to atmospheric refractivity. The authors will need to either fix this error or adequately justify the use of this different equation.
4. Results and Discussion:
    a. The first few sections of the results are primarily dedicated to various retrieved profile properties such as resolution, penetration depth, and obliqueness. While I understand the authors' desire to be complete in their analysis, it seems to me that while this information is unique to AR events, this type of information has been described by previous studies that develop the methodology. I would suggest trimming and/or cutting some of the material regarding resolutions, durations, etc. so that the authors can focus more on the observations themselves and their impact on NWP simulations of AR events, as the purpose of the manuscript purports to be focusing on based on the title an abstract.
    b. The analysis of the effect of the azimuth of the RO relative to the aircraft (e.g, Figure 12) is very interesting. Is the rate of obstruction due to the aircraft something that could be resolved with additional antennae? Or perhaps switching to open-loop tracking? It seems like the highest rates of obstruction occur for the most impactful ARO profiles, so future solutions to this would represent a significant increase in the quality of the ARO data.
    c. How do the authors correct for errors in bending near the aircraft (receiver)?
    d. Are the authors down-sampling the ARO profiles to the ERA5 vertical resolution? It is not clear to me, because the authors state they use the pressure-level ERA5 data but then refer to the model grid points in line 593. I am concerned that there would be significant loss of information from the ARO as a result of down-sampling the ARO. Model level data would be more sufficient for this comparison, particularly in the lower troposphere. Additionally, is linear interpolation of the ERA5 temperature and humidity sufficient? Why not use a higher-order interpolation?
    e. I would suggest that the authors do more to put their results in context with other recently published in-atmosphere RO studies, particularly those that occur in high-moisture environments.
    f. While the ARO data from AR recons undoubtedly provide significant information about the atmospheric state, it is not clear to me how much the ARO profiles, which generally do not penetrate below 4 km due to closed-loop tracking, actually penetrate into the clouds and precipitation resulting

from AR. Can the authors comment on what the profiles that penetrate below 4 km show in regard to the regions that are cloudy and/or actively precipitating? I feel like this information would contribute more to the manuscript overall than the long discussion of various resolutions, penetration, etc. of the profiles.

5. Conclusions:
    a. Does the AR dataset have a formal citation? What about a DOI? Archiving the data with NOAA would allow for the creation of both of these things fairly easily. The data should have a proper citation regardless, but archiving with NOAA is merely a suggestion based on reviewer experience.
    b. Again, the comparisons of the ARO profiles with dropsondes and ERA5 are really not properly put into context with more recent studies. I strongly suggest that the authors do a more thorough review of recently published work to compare their statistics with other observations from SRO and ARO, particularly if high-moisture observations are available.

Please see my line-by-line comments for more specific details.

Line-by-Line Comments:

1. L013: "Lock" is a little bit general. I assume the authors mean signal phase lock. Please consider referring specifically to signal phase lock.
2. L026-027: Please reword this sentence to remove repeated occurrences of "on the other hand" within the same sentence. I would suggest at least two sentences from this one.
3. L144: I would advocate the use of an Oxford comma here. Specifically, after "transmitter (satellite)…".
4. L241: There should be an Oxford comma after "and" here for lists.
5. L249: "… relativity effect" should be "relativity effects" here.
6. L250: Should "effort" be "effects" here? Also, is this simply the first-order ionospheric correction? Was any testing done using higher-order corrections? If not, what would be the expected effects, if any?
7. L262-263: I would specify that the "positive bending angle" and "negative bending angle" are really "positive elevation angle bending" and "negative elevation angle bending" (or something similar) throughout the paper. Otherwise, it implies that the bending angle itself is negative, which is not physically consistent. This should be true throughout the manuscript.
8. Figure 3; What is the reason for the color shading? The link to the AR Recon data in the caption of Figure 3 should be formatted into a proper citation as a dataset to be consistent with AMT regulations.
9. L332-333: The description of the colored lines in Figure 4 should probably be limited to the figure caption.
10. L350: I don't believe that "decimated" is the correct word here. Perhaps the authors meant "delineated"?

11. L404: I would be careful describing this as “tomographic-style”. I’m not entirely convinced that this would fit the traditional tomography definition.
12. L527: I would change the end of this sentence to be “ …starboard and port directions, respectively.”
13. L545: “co-located” should be “colocated” to be consistent with journal hyphenation rules and consistency. Typically, “collocated” or “colocated” are the journal-recognized spellings. This should be changed throughout the manuscript.
14. L550-552: Please re-word these sentences to remove the second sentence. Perhaps something like “… solely from the G-IV, resulting in a much more extensive dataset…”
15. L554: “dropsonde” should be plural here
16. L556: “ARO profiles … from the track” is not needed as it was heavily discussed in previous sections.
17. L560: I think there is an indent here where there shouldn’t be one.
18. L596: Please see general comment #2a regarding the calculation of atmospheric refractivity. This is likely creating errors due to the incorrect refractivity formulation.
19. Table 4: I would suggest replacing the “nan” values with something to indicate that there is simply no data there. Maybe a dash?

Reviewer References:

Chan, B. C., Goel, A., Kosh, J., Reid, T. G. R., Snyder, C. R., Tarantino, P. M., Soedarmadji, S., Soedarmadji, W., Nelson, K., Xie, F., and Vergalla, M.: Commercial GNSS Radio Occultation on Aerial Platforms With Off-The-Shelf Receivers, navi, 69, navi.544, https://doi.org/10.33012/navi.544, 2022.

Rüeger, J. M.: Refractive Index Formulae for Radio Waves, XXII International Federation of Surveyors, Washington, DC, USA, Integration of Techniques and Corrections to Achieve Accurate Engineering Survey, 13, 2002.

Rüeger, Jean M.: Refractive Indices of Light, Infrared, and Radio Waves in the Atmosphere, University of New South Wales, 2002.

Xie, F., Nelson, K. J., Chan, B. C., Goel, A., Kosh, J., and Vergalla, M.: First Results of Airborne GNSS Radio Occultation Sounding From Airbus Commercial Aircraft, Geophysical Research Letters, 51, https://doi.org/10.1029/2024GL110194, 2024.