**Review: Sensitivity of airborne radio occultation to tropospheric properties over ocean and land**
**Xie et al.**

This manuscript presents the results of the ferry flight of an airborne campaign, during which different atmospheric conditions were found. The data were acquired for both a zenith-looking omnidirectional antenna and two limb-oriented ones and they have been processed with three techniques.

The topic and possibilities of the data set are relevant and worth publishing, but some aspects are not clear and require further work on the manuscript.

— The authors mention that Haase et al. 2014 and Murphy et al. 2015 did present results from analysis of RO data obtained during the same campaign. It is not very clear which are the main differences between this manuscript and the two former articles (except the use of the ferry flight instead of the scientific flights). Which are the new aspects tackled in this manuscript not covered in the former two?

— how the obtained numbers (precisions, biases, …) compare with the one obtained in Haase et al. 2014, Murphy et al., 2015 and any other paper on ARO analysis?

— I understood that these main differences could be either the open-loop vs. close-loop data or the comparison between different analysis (GO/FSI/PM) or the comparison between the different antennas. However, most of the presented work is based on antenna CH1, mostly FSI and only a short section is devoted to the comparison between different methods, antennas, etc. On the other hand, longer sections are devoted to compare the results with radio-sondes and COSMIC data, both data sets not optimal for the comparison (I would simply delete these sections, keep the comparison with ECMWF fields, and give more weight to the most novel aspects of the study).

— The abstract gives a lot of information about the final precision and biases with respect to the ancillary data sets, while, to my opinion, the novelty of the study lies on the diversity of hardware, low level processing and high level analysis (different antennas, open-loop vs. close-loop, GO/FSI/PM).

— The abstract seem to give two different numbers for RMS precision (0.3% in line 20, 0.2% in line 30)

— page 3, line 17: type PRECICT (PREDICT)

— page 4, line 10-15: the way the sentence is written, it seems that the use of 3 antennas was only done during this ferry flight, and not during the science flights of the same campaign. Is this interpretation correct? if so, this should be one of the main focuses of the manuscript, and the analysis, comparisons and conclusions derived from these special (thus unique) settings. If this is not true, then change the sentence to make it clearer.

— page 5, line 22: the authors do not comment on the potetential effects of setting the refractivity at the receiver level as given by ECMWF. How is this affecting the rest of the retrievals?

— page 6, lines 3-4: which are the effects of correcting the clock errors by pairing with another PRN? how would it change if using another PRN? Is this well-known and done before in RO or ARO? if so, please add references, otherwise comment on possible side effects.

— page 7, line 6, typo: bracket not close (PM, Wang et al. 2017

— page 8, line 6: the authors claim that the retrievals below 11.5 km are independent from the ECMWF (above 11.5 to the receiver level they set the refractivity to ECMWF fields). The fact that at a given range of altitudes they are fixed tot the ECMWF seems to me that it affects the rest of the retrievals, so they are not independent of the ECMWF fields. Could the authors comment on this?

— page 8, lines 14-17: new ERA-5 fields are now available, hourly at 25 km resolution. The interpolation process would be easier.

— page 10 and afterwards: the RMS difference with respect to ECMWF fields, are they computed up to 11.5 km or up to the receiver level? The comparison should stop at 11.5 km, as above this altitude the solution is set to ECMWF and including this upper segment in the RMS difference would artificially lower the number. Could the authors clarify this point? preferably in the body of the manuscript (not only in the rebuttal letter).

— pages 11 to 14: as mentioned before, I would remove the comparison with COSMIC data (and radiosondes), it is not optimal and does not contribute to the work, adds 'noise' to the study.

— page 15: the authors do not provide details about the antennas, could they present the gain of each of them? either the antenna or a pattern or something that might help understanding why an omnidirectional antenna looking to the zenith achieves ARO of better quality of the dedicated limb oriented ones. Perhaps Table 1 could include the gain of each of the antennas at the observation direction of each ARO event, or the azimuth direction w.r.t. the antenna boresight. Different colors in Figure 10 would also be a useful way to include this information.

— page 17, 3rd paragraph: rather useless comparisons, either remove or shorten, give less weight.
— Summary and conclusions: comparison with Haase et al., 2014 and Murphy et al., 2015 and other ARO experiments. Are these precisions and biases better or worse and why?

— page 18, line 10: it is remarkable that the precision with an omnidirectional antenna looking to the zenith achieves good or better precision than the dedicated side-looking antennas. This should be highlighted here. Also, authors could discuss the potential that this finding might have in the use of commercial flight GPS data for ARO (no need of dedicated side-looking antennas!).

— Figures 4, 5, 7, 8, 9 11, captions: which antenna? CH1? please add this information in the caption.

— Figures 4d, 7a, 7b, 8a, 8b: why the bending-impact profile starts at 2 km impact and climbs up to 14 and then down again? I don't understand the segment at low bending angle, with the impact parameter climbing up from 2 to 14 km. Is this well-known and understood?

— Figure 10: perhaps it would help to plot these line with a color code, accounting for either the gain of the antenna at the direction of the ARO event, or the azimuth difference w.r.t. the antenna boresight.

— Figure 11: why authors plot the 150 deg azimuth line? is this related to the orientation of the antenna boresight?