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Atmospheric Measurement Techniques
Discussions

Interactive comment on "An evaluation of COSMIC radio occultation data in the lower atmosphere over the Southern Ocean" *by* L. B. Hande et al.

Anonymous Referee #2

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Comments by Section:

2.1 COSMIC

The authors need to review Kursinski et al. (1997) section 2.2, Atmospheric Bending and Refractive Index Profile Retrieval: Theory. In particular, Kursinski et al. (1997) eqn (1) and/or eqn (2) which show that the measured refractivity profile is derived as a vertical integral (starting from the top of atmosphere) from the observed RO bending angle. The RO bending angle is locally (in height) sensitive to the vertical change in refractive index. This is the basic RO concept which the COSMIC network exploits. The equation shown by the authors as eqn (1) on page 6 is misleading because it shows how the measured refractivity (on the left) should relate to atmospheric thermodynamic

variables. The authors then skip over the "dry" temperature product to discuss the "wet" 1D-var product. There are too many issues being glossed over quickly without making the important points. The interpretation of the results later suggest that the authors are not clear themselves about the data they are studying and how they are derived.

I suggest that the section on COSMIC data (section 2.1) needs to address three basic (and relevant) topics;

1) The actual equation used by COSMIC to measure raw refractivity from bending angle as given in Kursinski et al. (1997) eqn (1) and (2). Here the point needs to be made that the RO observation is of bending angle (which is known precisely from the satellite orbital knowledge) and that a refractivity profile is derived using an integral computed over the vertical coordinate. This explains why the authors method of computing the vertical derivative of the COSMIC "raw" refractivity works as well as it does. The local vertical derivative of the COSMIC refractivity gives the change in bending angle at that height, which is the fundamental COSMIC measurement. That is, the authors are essentially undoing the COSMIC refractivity calculation to get back to the local bending angle profile. Presumably, when the atmospheric boundary layer has characteristic thermodynamic shape that bends the RO signal, then the integral equation used to derive COSMIC refractivity shows this as a change in height. When the boundary layer does not produce a vertical gradient in refractive index, then the COSMIC RO signal will not be bent and thus the top of the BL will be invisible to the RO detection method. I think this explains the good height results show in the last section curiously titled, "Local statistical evaluation" and also the reduced frequency. The RO method is only sensitive to profiles that "bend" the radio waves, no bending means no RO sensitivity. This suggests that it might be even more accurate to look at the bending angle profiles themselves.

2) The theoretical dependence of N thermodynamic variables on p, T, and e (given in this paper as eqn (1)). Following the current eqn (1) there needs to be a brief discussion of the ambiguity between Temperature and Water Vapor in the refractivity

equation. It is not possible to derive the temperature without independent knowledge of the water vapor or vice versa. This is a fundamental limitation of the current RO method. The "dry" temperature is only valid in the stratosphere and upper troposphere where the water vapor partial pressure can be neglected in eqn (1). There are many references that could be given on this point and this motivates why the 1-D var method is necessary.

3) The discussion of the "wetPrf" can then be made. The current discussion is inadequate in that it does not specify which ECWMF fields are used in the 1-D var analysis and in particular if the MAC soundings were included in the background field through data assimilation or not. A paper reference to the 1-D var COSMIC method should be included as well as the COSMIC data version used. Typically the ECMWF temperature is used in the troposphere as truth and the COSMIC data is used to retrieve the water vapor concentration profile, but it's not clear if that is what was done in the dataset being analyzed. Some clarification of this point would be helpful to the later discussion.

2.2 ECMWF analysis My main complaint in the description of ECMWF analysis as "data" and the use of the word "independent" in this section and elsewhere in the paper. I am not familiar with the details of the ECMWF data used in this study of the Southern Ocean, but I would be very surprised if the MAC soundings were not already assimilated into the ECMWF analysis, especially given the sparse sampling in the southern hemisphere. This point needs to be clarified. Two options are possible; 1) MAC soundings have been denied the ECMWF analysis somehow, or 2) MAC soundings are included in the ECMWF analysis. In either case the issue of independence of ECWMF needs to be clarified. If the MAC soundings are already included in the ECMWF analysis then the comparison back to ECMWF is not an independent check at all, rather it's a measure of the "goodness of fit" of the ECWMF data assimilation methodology which attempts to fit smooth temperature and moisture fields to point measurements. If the MAC soundings are not in the ECMWF analysis fields then the authors should state what data is going into ECMWF assimilation to clarify what is

meant by "independent measurments". I recommend that this section be removed from the "data" section 2 and placed into a separate NWP section with some references and discussion of the relevant issues for this paper.

4.2 Thermodynamics Are MAC profiles assimilated into the ECMWF analysis? If so, explain why the ECWMF-MAC error is not zero. Does COSMICwet use the same ECMWF analysis as it's background, or is it using something else such as a reanalysis?

The statement that ECMWF analysis gives the smallest RMS error is likely explained by that fact that MAC soundings are already assimilated into the ECMWF and thus should not be used as independent data when compared to MAC. Added to this is the extremely small sample set of 35 COSMIC matchups over the course of eight years. This section is by far the weakest in the paper but it does illustrate the potential problems of deriving thermodynamic profiles from COSMIC measurements. Rather than drawing any conclusions however, I suggest that this section be combined with section 4.1 and the combined section be described as "case study investigations and discussion".

6. Conclusions

I have no objection to the conclusions as stated however I think that some of the questions raised about the results could be lifted if my explanations are adopted by the authors. The results actually seem quite sensible if you think about what the RO product is measuring and the strengths and weaknesses of the COSMIC measurement. Surely it's pretty important that COSMIC can to obtain accurate BL heights over the Southern Oceans even if it is limited to cases where that BL top causes measurable bending.

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