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Atmospheric Measurement Techniques Discussions

Interactive comment on "Comparison of RO tropopause height based on different tropopause determination methods" *by* Ziyan Liu et al.

Anonymous Referee #1

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Context and general comments

The paper describes a comparison between two methods for retrieval of tropopause height from GPS radio occultation (RO) data: a) a method using the WMO tropopause definition based on the temperature lapse rate, and b) a method based on the covariance transform of the logarithmic bending angle profile. The latter method was designed [Lewis, 2009] to use RO data at an earlier stage in the RO processing chain, thus avoiding potential biases introduced by a priori information and an assumption about hydrostatic equilibrium. Collocated tropopause heights from the ECMWF operational archive, based on temperature lapse rates, are used as a reference for comparison.

Comparisons are done between RO and ECMWF data, separately for the two methods

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Discussion paper

and for data from Metop and from FY3C. The geographical distributions of biases are plotted. In addition, the differences between the two methods are studied, separately for Metop and for FY3C.

Comparisons between the two methods have been described before, particularly in the publication [Lewis, 2009] where the bending angle method was first described. However, the present study gives a more detailed description of the statistical distribution of the differences in latitude and across geographical regions. In addition, it provides some new information on the differences between observations made from Metop and from FY3C. A part of those differences could be caused by the different local times of the two LEO satellites, but particularly for the method based on temperature lapse rates it is not unlikely that different processing play a role (see below).

The manuscript is easy to follow and some new interesting observations on the differences between the methods and the differences between Metop and FY3C are presented. The analysis is fundamentally sound but leaves some important gaps, of which the most important is that the paper lacks a discussion of the findings and that some important facts are not clearly pointed out as they ought to be. In particular, the latitudinal bias structures (i.e., the biases between RO and reanalysis) are relatively similar for the two methods in Metop data, but distinctly different in FY3C data. Figs. 4 and 6 shows that the bias structures based on the temperature lapse rates are very different for the two satellite missions. This is most likely an indication that the processing from bending angle to temperature data, which according to Section 2.3 has been obtained from two different archives, have differences that affect the conclusions. Or could it be the fact that Metop and FY3C sample the atmosphere at different local times? The observation that the bias structures are relatively similar in bending angle provides an argument against this. An important question is thus what we can infer from the differences between the two methods, when these differences depend on which data set we use. A reader of the paper wouldn't know from the facts presented in the paper, it is not discussed, and the problem is not adequately pointed out. This is an important

weakness of the paper.

A publication of this paper would require that the above mentioned issue is analysed and discussed. What are the potential cause of the differences between Metop and FY3C and how does it affect the conclusions?

The English language in the manuscript is not of sufficient quality (note to Editor: does it comply with the standards of this journal?). It needs to be improved before publication. Some of the figures, in particular the latitude-longitude plots have a too low resolution.

Specific questions to the authors

1. Writing proper English for a non-native speaker can be difficult. I can only recommend that the authors ask someone fluent in English to have a closer look at the text, and suggest updates to the text.

2. The figures showing the coloured latitude-longitude plots need to have a higher resolution.

3. Are the ECMWF data taken from the ERA-Interim reanalysis? If so, it should be stated. Or is it operational NWP analysis data that are used?

4. Can the authors think of reasons for the bias structures in LRTH to be so different for Metop and FY3C (even if the biases are relatively small in absolute numbers, they are structurally significant)? Are there systematic differences in the set of temperature profiles? After all, the bias structures in BATH are much more similar between Metop and FY3C.

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