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Atmospheric Measurement Techniques Discussions

Interactive comment on "Detection of the cloud liquid water path horizontal inhomogeneity in a coastline area by means of ground-based microwave observations: feasibility study" *by* Vladimir S. Kostsov et al.

Anonymous Referee #1

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In this paper, the authors want to analyze liquid water path (LWP) gradients in a coastal area based on microwave radiometer (MWR) measurements. While the topic in general is of interest, I have substantial concerns about the paper in its present form. The main issues are related to the methodology and conclusions which are drawn.

A large part of the paper is dedicated to the analysis of measured off-zenith brightness temperatures (BTs) in comparison to calculated off-zenith BTs based on the retrieved atmospheric profiles from zenith MWR measurements. The authors state correctly that the BT difference (DTB) which they then derive is related to the gradient in LWP, gra-

dients in T and q as well as further errors and uncertainties. The latter point is really crucial. Large uncertainties are related to the forward calculations they performed using the retrieved T and q profiles (highly smoothed!) and the retrieved LWP. Even if the retrieved LWP is quite accurate, it is still unclear where to place the liquid water vertically. This is not discussed at all and will lead to large uncertainties in the calculated brightness temperatures and brightness temperature differences. This has large implications for the results shown in Figs. 6-10, but the authors merely discuss them. The authors see the problem of disentangling the BT signal of the LWP gradient and that is why the analysis is very qualitative. However, this discussion does not provide a new insight. The conclusions which are drawn could be made without having these measurements: e.g. a liquid cloud located over the instrument with a clear-sky scene around will cause positive DTB values. In my opinion, the whole section on the BT comparison does not provide new insights but rather leaves the reader with many more open questions.

The authors recognize that the best way to proceed is to develop and apply LWP retrieval algorithms and compare LWP directly for the different elevation angles. I agree that this is the way to go, however, again the methodology that they follow to derive the retrieval coefficients is not sound: the authors take the retrieved T and q profiles together with the retrieved LWP again to simulate the BTs for the various elevation angles. Also, here it is not reasonable to use the retrieved profiles for the forward calculations due to the very smoothed T and q profiles (which are thus not representing the realistic atmospheric state). It is again not clear how LWP is vertically distributed. A proper way to generate retrieval coefficients is to use a representative, realistic set of atmospheric profiles from radiosonde or NWP model data.

The MWR measurements/simulations are also set into context to a SEVIRI LWP product. In order to be able to set the results in context to SEVIRI, which views a different scene than HATPRO, a more thorough analysis of the representatively is needed. I am not sure how much can be concluded from the comparison provided in Figs. 11-

12. Yes, on the one hand, SEVIRI and the MWR reveal similar signatures to some extent, on the other hand there are also quite differences. It is totally unclear if this is due to sampling issues, viewing geometry or methodology. Even if uncertainties are discussed I do not see a robust result that can be provided from this comparison.

In the end, the authors state that: "The main conclusion of the study is the following: the approach to detection of the land-sea LWP gradient from microwave measurements by the HATPRO radiometer operating at the observational site of St.Petersburg State University has been successfully tested and the results confirmed the presence of the horizontal land-sea LWP gradient in the vicinity of the radiometer". When looking at Fig. 12, D_LWP for HATPRO reveals various kinds of differences for LWP in zenith and off-zenith directions. These differences are sometimes positive, sometimes negative but there is not a scientific conclusion which can be drawn in my opinion; at least from the results which are presented and considering all the uncertainties which are prevailing in the methodology. Thus, the paper does not provide substantial new insight in this topic in its current form.

In my opinion, the paper needs substantial revision which is beyond major revisions. For this reason, I recommend to decline the manuscript. I suggest to extensively revise the study and encourage the authors to submit a paper at a later stage.

I suggest to concentrate on the analysis of the LWP variability in LWP space and not in BT space and to proper set up multivariate regression-based retrievals for zenith and off-zenith LWP. A physical motivation and discussion for the LWP gradients is currently missing. Why should LWP be enhanced over land than over water? Do you always expect this feature? If the SEVIRI LWP product is used, it needs to be properly introduced and uncertainties discussed (SEVIRI is not the truth!) as well as the representativity of SEVIRI for the HATPRO site and vice versa. Are the LWP pdfs similar for SEVIRI and HATPRO? If case studies with LWP gradients are presented, the physics behind including the role of the meteorological/synoptic situation could shed more light on why certain gradients exist or not. A qualitative analysis is nice but quantifying the LWP

gradient would even add more value to the paper.

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