

Comments on manuscript: “Long term Observations minus Background monitoring of ground-based microwave radiometer network. Part 1 Brightness Temperatures” by F. De Angelis et al.

The manuscript presents an analysis of differences between brightness temperatures simulated using a numerical weather prediction forecasted atmospheric profiles and measurements from 6 microwave radiometers in northern Europe. The analysis of the differences between forecasted and observed brightness temperatures is useful to assess the feasibility of assimilating ground-based microwave brightness temperatures into NWP models, especially for the purpose of bias removal and estimation of covariances. For this reason, the analysis presented is a useful step in this direction. The analysis of the behavior at different sites is crucial to understand how consistent the measurements are.

The paper is generally well written and organized. Some parts might benefit in my opinion from additional clarifications, perhaps due to my own incomplete background knowledge of NWP. Clarifications may however be useful for a broader audience. My comments are below:

1) **Page 3 lines 11-13:** “...while channels most sensitive to cloud liquid water (31, 51, and 52) are dominated by their representativeness errors”.

I am not sure I agree with this statement. Channels at 31, 51 and 52 GHz are very sensitive to the water vapor continuum as well and to the spectroscopy of the 50-60 GHz line complex (for example the mixing coefficients).

2) **Page 3 line 16:** “0.5 – 1 K (51-53 GHz)”.

This uncertainty for LN2 calibration is very optimistic in most field conditions.

3) **Page 5 Section 2.3:** It may be a good idea to give a quick summary of how the fast radiative transfer model RTTOV-gb works and why one needs to train it.

4) **Page 5 line 26:** I am not sure what NWPSAF is.

5) **Section 2.1 and 2.4:** In general, it is not clear how the measurements are used in the comparison with the model output. I see in section 2.2 that the model produces profiles every 3 hours, but I assume the MWRs produce brightness temperatures every few seconds or minutes depending on the scanning geometry. How were the two matched temporally? Were the MWRs brightness temperatures averaged at all? Or are just instantaneous temperatures?

6) **Page 6 Section 2.4 lines 10-14:** I do understand the cloud screening with the IR measurements for zenith observations. But if the IR measurements only look at the zenith most of the time they will not be representative of off-zenith measurements. Most likely there will be clouds at lower elevation angles unless you have some other ways to ensure that the clear-sky condition holds horizon to horizon. Therefore, the off zenith analysis is doubtful.

7) **Page 6 Section 3 line 32-35:** It is not clear why is the 31.40 GHz channel calibrated with LN2 and not with tip curves. Is this the case for all 6 radiometers?

8) **Page 7 lines 38-40, Page 8 line 1-18:** I don't understand this step at all. Why is this bias now corrected with a bias derived from another model? Why is it necessary to do this? I think the author should explain this step.

9) **Page 8 lines 21-34:** As already mentioned in the previous comment, are you sure that there are no clouds off zenith? Secondly scanning the radiometers at low elevation angles ( $< 20$  degrees) has a high chance of contamination of the readings. Even if a radiometer is elevated a few meters above the surface there is the chance of contamination from foreign objects such as electric cables, poles, buildings, mountain tops, antennas, etc. Are the authors sure that the location and installation of the six radiometers is adequate for such low-angle scanning geometry?

10) **Page 9 lines 3-10:** This part is not clear. The authors introduce now the difference between observation and analysis. It would be good to explain what are the Analysis data and how they differ from the forecast data.

From the subsequent discussion I infer that the Analysis data are the data produced by the model after the assimilation of additional information from measurements (?).

11) **Page 9 Line 8-10:** "Thus, forecast and analysis...this may suggest that there is useful information in MWR data from improving NWP data assimilation"

I am not sure I follow this reasoning, why the fact that the assimilated data didn't produce much difference with the respect to the MWR suggests that the MWR can improve the assimilation?

12) **Page 10 line 35:** "Statistics at K-band increase with decreasing..." Is statistics the right word here?

13) **Fig. 5 Caption:** Letters A, B, C, etc. are missing in the figure

14) **General comment about Figures 3,4,5, and 6:** I wonder if instead of having 2 panels (22-32 GHz, and 50-59 GHz) it would be better to have only one plot for the whole frequency range (22-59 GHz). This would place the differences on the same vertical scale and make it easier to appreciate the differences.