

## ***Interactive comment on “Ground Based Lidar and Microwave Radiometry Synergy for High Vertical Resolution Absolute Humidity Profiling” by M. Barrera-Verdejo et al.***

**Anonymous Referee #3**

Received and published: 28 March 2016

Review of paper: amt-2016-46 Ground based lidar and microwave radiometry synergy for high vertical resolution absolute humidity profiling.

The paper combines LIDAR and MWR measurements to improve vertical profiles of humidity between the ground and the upper troposphere. It applies an optimal estimation technique to evaluate the single-instrument and combined retrievals.

I have a few reservations about the paper and the results. The first reservation is of a general nature. The RL is obviously a superior methodology to the MWR for humidity retrievals. Although the authors use the MWR measurements to improve the RL retrievals where retrievals from the latter are not reliable, the MWR retrievals themselves have very little information above the first kilometer.

C1

It is possible that the RL profiles could be improved above the boundary layer just by choosing a better climatology or may be a model output without going to the extent of doing an optimal retrieval estimation. The only place where I could see some real advantage of using MWR measurements is in the lowest 200-500 m although in the case shown the results appears mixed.

The second reservation is about the conclusions as I am not sure that the results entirely support the conclusions.

Specific comments:

Page 8: Line 225, Eq. 6: Can the author provide a reference for this definition of vertical resolution? Usually the Backus-Gilbert technique is used to define the vertical resolution from the spread function. An example of application of this technique to determine the microwave radiometer vertical resolution can be found in Westwater and Snider and Carlson (J. Appl. Meteor., vol. 14, pp. 524–539, 1975).

Page 16 Fig. 6. The results in Figure 6 are mixed. In the upper troposphere the RL seems to have the lowest bias up to 4 km. Above 4 km the combined retrievals show a very small improvement, however what the standard deviation is considered I am not sure that the improvement is clear.

Page 17, section 5.3 and 5.4 I am not sure what is intended by theoretical error shown in Fig. 7. I think the author means the “a posteriori” covariance. However this measure of uncertainty, although necessary, represents a partial picture. A better estimate of “error” intended as RMS Error is the one you provide in the comparison with radiosondes in Fig. 6. The authors should probably change “theoretical error” with “covariance” if this is what they meant. Otherwise they should explain what they mean by “theoretical error”. It is not clear how the uncertainty shown in Fig. 7 and 8 relates to the error bars shown in Fig. 2 (the text says they are both computed from Eq. 4), however the values seem considerably different. In particular the error bars above 2 km in Fig. 4 seem to be  $\sim 0.5 \text{ g/m}^3$ , but they seem smaller in Fig. 7. Or it is just due to the different

C2

scales of the plots? I am not sure I entirely understand the difference in what is plotted in Fig. 7 and 8, besides the classification between daytime and nighttime. Could you please explain that more clearly?

Page 25 line 576: "The improvement of the synergy have been analyzed in terms of several parameters like the reduction of the theoretical error or the increase in DOF, showing significant advantages..." I am not entirely sure about the accuracy of this statement. The theoretical error (which is the a posteriori covariance) is related to the DOF. The two metrics are not independent and essentially convey the same information in different form. Although it is true that the analysis shows the reduction of the covariance after the retrieval, the comparison with the radiosondes conveys mixed messages about the actual usefulness of the MWR measurements.

Overall the paper provides useful information but the discussion can be improved, therefore I suggest major revisions.

There are a few English corrections needed: Page 24 line 542 "is chosen kind of arbitrary" can be rephrased: "The increase in RL measurement uncertainty is arbitrarily chosen based on..." Page 25 line 569: "...synergy of different sensors has become come more..." remove come Page 576: "several parameters like" "like" can be replaced with a colon.

---

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2016-46, 2016.