

Interactive comment on “Profiling water vapor mixing ratios in Finland by means of a Raman lidar, a satellite and a model” by Maria Filioglou et al.

Anonymous Referee #1

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Review of the paper by Filioglou et al.

The paper mostly discusses the calibration of water vapor mixing ratios using Raman lidar measurements with the synergy of radiosonde measurements, satellite retrievals and model simulations. The paper is useful for the evaluation of different options to calibrate the lidar measurements depending on the availability auxiliary information near a lidar station and thus should be considered for publication in AMT. It is well written and structured but the authors should consider the comments below before the acceptance of the manuscript.

General comment:

C1

It missing in the discussion and the conclusions a reference to previous studies that deal with the calibration of lidar WVMRs. Are the values shown applicable to other systems? Are the estimated uncertainties larger or smaller, etc? In general the authors should provide a clear message to other groups that perform lidar measurements of water vapor. The evaluation of different options to calibrate the measurements is such a message but this has to be compared with what is considered common or best practice in the literature. In addition the authors should provide a comment what is the impact of these uncertainties for long-term studies and make a comment how these compare with other sources of water vapor measurements. This would help them to highlight the importance of lidar measurements for long-term studies.

Specific comments:

Page 2, line 1: something is missing from the sentence before “therefore”.

Page 4, line 8. Some quantitative information on the uncertainties should be provided here.

Page 4, line 12. Is there any reasoning for this large number of soundings at Hytiälä compared to other sites? Have these been used in this present study? Did the authors used only the nighttime soundings for the lidar calibration?

Page 4, line 20. Have these data been used only for the campaign period or these are routinely used for the Kuopio measurements?

Page 4, line 22. The authors should be more specific here for the uncertainties, especially for their possible height dependence.

Page 5, line 3. It is not clear here for the reader what would be the impact of these two configurations (if already known) on the water vapor mixing ratios simulated.

Page 5, line 28. Is the assumption to neglect the particle extinction contribution at such heights valid only for the clean conditions prevailing at the certain sites?

C2

Page 6, lines 6 and 10. The authors make use of the terms accuracy and precision in a confusing way. I guess they are referring in both cases to accuracy. Please correct or evaluate more.

Page 6, line 23. The authors should provide here some reasoning why they chose the regression method. Is this a matter of data availability? Do they claim this is better? As written, they give the impression that they made a random choice.

Page 6, line 29. What are the major drifts of the RS at higher altitudes and what is this altitude range? See also a previous relevant comment.

Page 6, lines 30-31. This trajectory-based matching methodology as described is confusing. What do the authors finally use? The closest satellite pixel to the site, or the pixel from which the forward/backward trajectories arrive closest to the site? What distance is considered close? Please clarify and provide a better description.

Page 8, line 2. Something is missing in the sentence after "between the".

Page 8, line 11. Are the larger differences observed below 0.5km affected by the overlap effect on the calibration? How large is this effect and down to what altitude can this be (or has been) corrected?

Page 10, line 1-2. The authors associate the higher discrepancies between the lidar and the model to different model setups. They should provide a comment what can cause this difference. See also a previous comment relevant to the model description.

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