Review of revised version of „Ground based lidar and microwave radiometry synergy for high vertically resolved thermodynamic profiling" by Barrera-Verdejo et al.

The major changes in the revised manuscript are the addition of an Appendix showing averaging kernels and Jacobians, and the removal of the part on temperature and simultaneous retrieval of temperature and humidity. Further, the authors addressed all my comments in detail.

I acknowledge that the manuscript has improved and the response has clarified many things, but the study the way it is presented does still not meet my standards for scientific publication for the reasons presented below.

Therefore, I cannot recommend the manuscript for publication in AMT. However, I consider the work innovative and significant for the scientific community and I strongly encourage the authors to solve the remaining issues and resubmit the work for publication.

Major comments

Definition of measurement covariance matrix, Se, of Raman lidar (RL)

The adaptation of the text to address this issue is minimal. The authors state that they use a simplified version of Se and refer to other publications where this simplification has been used. With this, the situation is correctly presented in my view. A statement on the fact that systematic errors are present, important and correlated would have been adequate.

In the response the authors introduce various contradictions by claiming that systematic errors are not important (response p13, manuscript l134). Because at the same time the error in the calibration constant is reported to be around 5% and hence comparable to the random error, and second Section 4.2.4 is dealing with FOV effects, which shows that systematic errors are present and important. Despite this, the authors go as far as claiming in the response, that "no systematic error associated with the FOV, […] is present in the reported Raman lidar measurements".

I do also not agree with the discussion of the error correlation in the response. The statement "…one can say that there is no correlation between lidar-derived atmospheric products at different altitudes as long as no vertical smoothing is applied to the data." is not correct. I make an example: if all altitude levels have been calibrated with one and the same calibration factor, then all levels have the same uncertainty due to calibration, and hence are correlated.

Presentation of averaging kernels (AVK) in Appendix

I am very happy to see AVKs in the Appendix! However, the Appendix in its current form raises more questions than it supports the results in the manuscript. The following figures need more explanation:

Fig. 10:

- Why do the AVKs at lidar levels not reach a value close to 1? We would expect that at the levels of the lidar (0.5 km < z < 2.5 km) a change in the atmosphere would seen in the same way by the retrieval, i.e. the AVK would be close to 1.
- How come that the AVKs above 2.5 km have the same or even higher peaks than below 2.5 km? One gets the impression that the retrieval is almost more sensitive above 2.5 km.

- The AVKs in panel b) look very strange, individual AVKs cannot be distinguished and the red AVKs of higher levels look very unreasonable (same for panel c)). Here I would expect "normal" MWR AVKs for a humidity retrieval (i.e. something similar as shown here http://www.atmos-meas-tech.net/4/1891/2011/amt-4-1891-2011.pdf)

Fig. 12:

- J is the derivative of the forward model with respect to the atmosphere. Hence, why is J=0 for z<2.5 km? If one perturbs at 2 km, this has an effect on all MWR channels, no?

Minor comments

A3 is quite incomplete and imprecise. For example:

- Definition of vertical resolution is missing.
- What is "small resolution"? if resolution is high, the system's capability to resolve small scale features is good. Low resolution is the opposite. Here, I have the impression small resolution means high resolution.
- L568 isn't it the other way around, high layers induce small variations in the opaque channels (we perturb the atmosphere and see the effect on the observations)

Clipping of RL profile

I understand the argumentation of the authors to clip the RL profiles at 2.5 km and agree with it. However, the new Fig. 6 is not well described:

- Why do the solid lines (daytime) stop at 5.5 km? the retrieval is defined for the entire range, at least for MWR and BOTH.