

***Interactive comment on “Ground-based water vapor Raman lidar measurements up to the upper troposphere and lower stratosphere – Part 1: Instrument development, optimization, and validation” by I. S. McDermid et al.***

**Anonymous Referee #1**

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General comments:

In the first review provided before entering the discussion phase, the reviewer suggested the authors to check the structure of their papers and to opt for merging the two companion papers.

This suggestion was mainly based on the fact that the main result, discussed in both the papers, is related to the comparison between Raman lidar and CFH and the analysis

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about the equivalence between the precision of their observations

As I mentioned, fragmentation of research papers should be avoided.

In detail, the manuscript discusses the technical solutions for the improvement of water vapour measurements performed with the JPL system in Table Mountain Facility. However, the originality of the implemented solutions with respect to what is already described in literature (also partly cited in this manuscript) and operative in worldwide lidar systems is still not pretty clear.

Most of the proposed solution are not a novelty in the field of Raman spectroscopy and they do not increase our scientific knowledge. I do not think that the detailed description of the system and its technological evolution might be the topic of scientific publication, even if it has good performances, unless the system is new or designed with highly innovative solutions. Obviously, this not a review paper and it sounds like a technological report for a scientific project or so.

Considering the fact that this result is also discussed in the companion paper by Leblanc et al. (using also a very similar paragraph), I suggest to revise the structure of the manuscript to provide a shorter but still comprehensive description of the JPL system. Then, this description could be put at the beginning of the companion paper. Further suggestion I provided in the review of the companion paper will help to re-design the paper. Accordingly I suggest to strongly reduce the number of figures that strongly overload the manuscript. Moreover, similar paragraph are still present in both the companions manuscripts and should be removed.

Below specific and minor comments are also reported.

Specific comments:

Page 5087, line 21: A comparison based on 10 profiles might demonstrate nothing. It might provide good remarks relative to an hypothesis testing, here is the higher accuracy of lidar in UT/LS than the radiosondes. In my opinion, a larger dataset should

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be investigated.

Page 5088, line 7: what happens to the signal-induced noise and fluorescence?

Page 5089, line 3: please, provide more details about the large increase (one order of magnitude) in the signal levels.

Page 5090, line 9: what about effective lidar resolution?

Page 5091, line 2: how these lidar profiles have been calibrated?

Page 5091, line 15: a quantitative comparison between lidar and other devices should be performed considering the lidar effective vertical resolution and using the same vertical resolution for the cross-compared profiles. Did you follow this approach?

Page 5092, line 9: Is also the lidar a research-grade instruments?

Page 5092, line 18: Vertical resolution is mentioned several times throughout the manuscript but it has been never defined and quantified. This has to be reported even if discussed in the companion manuscript.

Page 5093, line 8: The conclusion is on target, but if you classify your system as a system that can be strongly improved using current (not future) technologies, in some way you are confirming reviewer's opinion provided in the general comments. These lines must be rephrased.

Minor comments:

Page 5081, line 3: I suggest the authors to report always a reference when they provide typical estimation of an atmospheric variable

Page 5099, Fig. 3: Please, add more details to the caption.

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Interactive comment on Atmos. Meas. Tech. Discuss., 4, 5079, 2011.