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Atmospheric Measurement Techniques Discussions

Interactive comment on "Ground-based water vapor Raman lidar measurements up to the upper troposphere and lower stratosphere – Part 2: Data analysis and calibration for long-term monitoring" *by* T. Leblanc et al.

Anonymous Referee #1

Received and published: 5 October 2011

General comments:

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

This manuscript mainly deals with the calibration of Raman lidar water vapour pro-

files even if the presented methods are already discussed in literature and most of the statistical estimations, provided for each method, are only partly supported by a corresponding analysis in the paper.

This second manuscript looks more self consistent respect to the companion. I support the publication but I suggest the authors to merge the two companion manuscript. The main result presented in this manuscript are related:

1. to the comparison between Raman lidar and CFH and the analysis about the equivalence between the precision of their observations.

2. to the conclusion about the hybrid calibration method that goes beyond the results already reported in McDermid et al. 2008.

The manuscript also reports many results from the application of existing calibration technique, only partly referenced in the manuscript itself, perhaps with the added value that they have been never dealt together using an extensive dataset of Raman lidar data.

On the other hand, comparison among the different calibration techniques and approaches is not reported but the authors only refers to the performances and accuracy of the single calibration techniques.

I agree with the reviewer #2 about when says that the theoretical part is presented in too much details. If you shorten this part of the manuscript and include a detailed and comprehensive description of the JPL system before the theoretical section, the merging between the two companion paper might result in a successful publication. Moreover, similar paragraph still present in both the companions manuscripts might allow to refine the shape in the merged paper.

I also agree that overlap issue should be described in a great detail. Finally in the abstract, the authors should include more information about the main scientific results of the manuscript.

Below specific and minor comments are also reported.

Specific comments:

Page 5113, line 6-14: This paragraph is very similar to another reported in the companion paper.

Page 5117, line 9: How do the authors retrieve the correction due to aerosol transmissivity in the troposphere and in stratosphere (when necessary like in presence of volcanic aerosol)?

Page 5117, line 10: this paragraph might be moved the section where the lidar system is described.

Page 5119, line 27: Please when a quantitative estimation of a parameter is reported mentioned, the authors should also insert a reference or the source of this information. Moreover, according to what discussed in the frame of GRUAN (GCOS Research Upper-Air Network) community, the best quality radiosonde accuracy should be lower than 4%. See also Immler et al., 2010 (AMT). Also the estimation of the water vapor content provided by a microwave profiler might be better depending on the atmospheric conditions.

Page 5123, line 14-18: the authors should provide more details about the assumption they do about the water vapour spatial and time scales for suing the coincidence criteria. Did the authors assess this assumption in a previous publication or simulations? Or are you referring to an existing publication? The reported assumptions are very similar to what reported in Sun et al., 2010 (JGR)

Page 5125, line 1-10: this paragraph is the same as reported in the companion paper. I also suggest to provide more details about this significant result.

Page 5125, line 13: this paragraph introduces the calibration with GPS or microwave radiometer. This technique is not new (see Turner et al, 1999 or Madonna et al., 2011, not mentioned in your paper). Moreover a comparison with the other calibration

techniques would a real added value for the manuscript. Reporting only the accuracy as "numbers" is not good as presenting an extensive comparison among the different techniques.

Minor comments:

Page 5113, line 12: I suggest the authors to report always a reference when they provide typical estimation of an atmospheric variable.

Page 5115, line 12: particulate is ok but aerosol should be better.

Page 5117, line 2: explain the improvements in the new software version otherwise the mention is not necessary

Page 5118, line 21: the use of “more” is not appropriate. I recommend the authors not be vague on quantitative estimation.

Page 5119, line 24: “microwave profiler” instead of “microwave instrument” sound better.

Page 5122, line 25: modify “participate to” with “participate in”.

Page 5127, line 7: “solution” instead of “fix”

Page 5127, line 10: “not cost effective” instead of “inapplicable”

Page 5145, Fig. 10: both the figure in the middle panel and the caption should be larger.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 5111, 2011.