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Atmospheric Measurement Techniques Discussions

Interactive comment on "Ground based lidar and microwave radiometry synergy for high vertically resolved thermodynamic profiling" *by* M. Barrera-Verdejo et al.

Anonymous Referee #2

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

The paper presents a method to combine passive microwave radiometer (MWR) measurements with active Raman lidar (RL) measurements in an optimal estimation (OE) framework for improved profiling of absolute humidity and temperature. Data from the HOPE campaign are analyzed and the main focus of the study is water vapor. One retrieval of temperature and one simultaneous retrieval of temperature and absolute humidity is presented at the end.

The approach is novel in two ways: To my knowledge this is the first study where MWR and RL data are combined in an OE framework treating both as measurements. Second, the idea to use the lidar retrievals, i.e. absolute humidity profiles, as input with a simple forward model that does only unit conversions and interpolation is an elegant approach, that avoids the costly development of a forward operator of RL raw data. But, as shown in the study, it is still possible to compute the uncertainty budget of the retrieval accounting for measurement uncertainties in both MWR and RL data.

However, the study has some important deficiencies as explained below in detail: (1)The specification of the RL measurement covariance matrix is oversimplified considering only random uncertainty. (2) The OE retrieval is analyzed in terms of error reduction and degrees of freedom (DOF). The presentation of the averaging kernels (AVK), which is a classical diagnostic of OE retrievals, is missing. (3) For all water vapor results shown, the RL data have been artificially clipped at an arbitrary altitude (2.5 km for absolute humidity) for practical reasons. Consequently, the reader does not get any flavor of the nighttime performance, nor of the quality of the optimal product, i.e. using all lidar data available. (4) The analysis and discussion of the temperature retrieval and the simultaneous retrieval of temperature and absolute humidity is poor compared to Section 4 relying on one single comparison with radiosonde (RS).

The paper is quite well written though some improvements of the language could still be done to improve clarity. The study fits well in AMT. Major revisions as specified below are required before the paper can be considered for publication in AMT.

Major remarks

The OE retrieval is characterized in terms of error reduction and DOF. The AVK is introduced in equation (5) but not shown. However, for a study so much focused on the method, the presentation and discussion of the AVKs to illustrate the sensitivity to the measurements as well as the variations in vertical resolution is imperative. The community has not yet seen many AVKs of combined active and passive remote sensing

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retrievals, if any, and its inclusion would complement the discussion of the DOF and clearly enhance the value of this paper.

The specification of the measurement covariance matrix (Sy) is crucial in OE methods, as stated by the authors, but the description of how Sy is assembled for MWR and RL is insufficient. The authors explain that "the covariance matrix associated with the MWR measurements was obtained empirically by calculating the correlation between the different channels, while constantly viewing an ambient black-body target with known temperature".

- How is the covariance derived from correlation?

- If equation (8) is used, how are s_a and s_b determined?

- Is the same covariance assumed for all elevation angles?

- If so, how is this justified?

Even less information is provided for the RL part in Sy: "the part of Sy corresponding to the RL is defined as a diagonal matrix containing the variances of every altitude". The RL measurement is itself a retrieval and it needs to be specified which error sources have been taken into account for its uncertainty budget (calibration, FOV, saturation, background, ...). Reference to corresponding publication is needed. The matrix is assumed diagonal. However, uncertainty due to calibration (5% as specified in 2.1), FOV, saturation or background, for example, introduce correlation between the errors at different altitudes which may have consequences on the retrieval. This issue must be properly addressed and discussed in the paper. In particular, Section 4.2.5 must be revised in this respect.

Given all the limitations of the external calibration of the lidar as explained in Section 2.1, I would suggest to include the lidar calibration factor in the forward model and to treat it as a retrieval parameter. This would resolve to a good extent the issue of correlated errors of the RL input data and introduce the capability to improve the a

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priori RL calibration by combining it with MWR observations, another strength of the combination of RL and MWR.

For all water vapor results presented, the RL data have been clipped at an arbitrary altitude for practical reasons. This is somehow in disagreement with an “optimal combination” of the two instruments. The results should also be produced and discussed for nighttime, i.e. for a clipping altitude of 7-8 km. It would be interesting to see how information is distributed in this case, for example if MWR contributes more in zone (a) during night than during day because of the different constraints. Depending on the obtained results, it would be worthwhile to include figures 6-8 for nighttime.

Section 5 and 6 are poor compared to Section4 and do in their present form not add much value to the paper. They rely on a single comparison with RS and the most important diagnostics of the OE methods are missing. I suggest to remove these section or to extend them significantly according my following remarks.

The determination of Sy of the MWR measurements for different observation angles is not discussed with enough detail and no values are presented. Information on Sy of the RL temperature measurements is missing completely. As for the absolute humidity retrieval, it is required to show the reduction of the a priori error (analog to Figure 6, i.e. a posteriori versus a priori error) and the averaging kernels.

While Section 6 presents a very interesting aspect of the study, its analysis and discussion are not convincing. Again, a priori error reduction, averaging kernels and DOF have to be presented and compared to the individual temperature and absolute humidity retrievals. Even if this is done only for a single retrieval, it tells much more than the comparison of a single profile with RS. Further, the reader has to guess that temperature and absolute humidity are retrieved and relative humidity is calculated afterwards (see minor remark below). However, it would be an interesting approach to retrieve temperature and relative humidity directly. Has this been investigated?

Minor remarks

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P5469, l9: I think this statement is too strong. Many publications show that the lidar technique provides the high spatial and temporal resolution needed to study convection and turbulence.

P5470, l1: replace “the quality” by “the range”

P5472, l9: replace “frequency” by “repetition rate”

P5472, l28: here and in the entire paper, specify if height is given above ground or sea level.

P5473, l1: What kind of “problems” is referred here?

P5473, l9: It is not clear how the lidar is calibrated. The calibration coefficient used in this study is the mean of all calibration coefficients derived from all individual BASIL-radiosonde intercomparison?

P5375, l16: the forward model acts in the other direction: the measurement is calculated from the atmospheric state. To invert the forward model, OE is used.

P5477, l1: the underlying assumption in OE is that the variables follow a Gaussian distribution. Has it been investigated if q is “sufficiently” Gaussian for the analyzed period? Have the authors tested also to retrieve log(q) instead? Please comment on this.

P5478, l18: replace “profile” by “data point”, or so.

P5479, l12: This phrase is not clear. Underline that level 1 data is used from MWR and level 2 data from RL.

P5479, l14: The statement “a clear forward model cannot be defined” is certainly not true. The lidar equation is a form of the Beer-Lambert law, as is the radiative transfer equation for MWR, and is implemented in many languages.

P4579, l16: In this section “correlation” and “covariance” are used as synonyms, please

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rectify.

P5479, l20: using angular data, the dimension of the MWR covariance matrix would be 27x27, no? please correct and complete.

P5480, l4: what do you mean with “scaling the temperature grid”?

P5480, l16: what is the motivation to have a dynamic retrieval grid? Wouldn’t a MWR retrieval also run on a 30 m grid?

P5482, l17: Why is Figure3 only shown up to 5 km? All other Figures go up to 10 km, so please show Figure 3 also up to 10 km to illustrate the varying performance as a function of time and height. I suggest to add a dashed line to mark the boundaries of the RL data.

P5483, l17: see comment P5480, l16.

P5486, l13: The reduction of the error depends on altitude, however, here one “integral” value is presented. It is not clear from the text how this number has been obtained. Is this the reduction of the error averaged over all altitudes? Please clarify the text here and for all other occurrences of this error reduction estimation.

P5487, l10: Sy has hopefully not been determined to the author’s best knowledge. Instead, a very much simplified version of Sy has been assumed neglecting systematic error terms which are correlated. How is it justified to increase the measurement error by, as it seems, an arbitrary factor 4 and without introducing correlation? See my major comments.

P5491, l5: It is not possible to retrieve temperature (T), absolute humidity (q) and relative humidity (RH) simultaneously, but either T and q or T and RH. Given Sa from Figure 1, I guess T and q are retrieved here, and RH is calculated afterwards. Please clarify the text. In my opinion retrieving T and RH would be an interesting approach. Have the authors investigated this, could you comment?

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P5491, l8: Are the numbers in the right hand Panel of Figure 10 really the differences averaged vertically? I consider this bad practice, because positive and negative differences cancel each other. For example, is a value of 2.46% in any way representative for the differences between “combined (no RL temperature” and “RS”, which are in the order of 10-20% at most altitudes?

P5492, l18: this is shown for absolute humidity, partially for temperature (DOF in Table 3) and not at all for simultaneous T, q retrievals.

P5493, l2: “The joint information . . .” there is not enough evidence for this conclusion.

P5493, l4: I guess RH has not been retrieved, but calculated from T and q. Please be precise and correct the text wherever necessary. Further, what is considered “successful” here? The fact that a T and q profile comes out of the retrieval even if no RL temperature data is used, is not surprising. More interesting would be to see what accuracy (a posteriori error!) is achieved with or without RL temperature data.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 5467, 2015.