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Atmospheric Measurement Techniques Discussions
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Interactive comment on "Ground Based Lidar and Microwave Radiometry Synergy for High Vertical Resolution Absolute Humidity Profiling" *by* M. Barrera-Verdejo et al.

Anonymous Referee #1

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Retrievals of absolute humidity from a microwave radiometer (MWR) using an optimal estimation (OE) technique are enhanced by including a Raman lidar profile in the measurement vector. The vertical resolution and accuracy of the MWR profile are improved. A priori information is drawn from local radiosonde observations. The performance of the algorithm is demonstrated with data from the HOPE field campaign, exploring the influence of the two data sources both separately and together. The combination is shown to reduce the discrepancy compared to coincident radiosonde launches and GPS observations of integrated water vapour.

Having reviewed the previous submission of this paper (http://www.atmos-meas-tech-discuss.net/amt-2015-63/), I am surprised by how few changes

have been made. Parts of the text have been streamlined to present a more coherent argument, the input data is now pruned by considering the measurement uncertainty, and a validation of integrated water path includes some fairly convincing scatterplots. However, the text and figures are often identical (leaving about half of my previous comments unaddressed).

I still cannot recommend the publication of this paper, but I am not opposed to it's publication if other reviewers favour it. In essence, I find this an unsatisfyingly simple technique with far too little information available to justify the resolution and algorithm used. However, comments on the previous paper convinced me that vertically resolved humidity measurements are sufficiently poorly constrained that any validated attempt to improve their resolution is a step in the right direction. This algorithm meets that standard and the improved text makes it clearer that this is primarily a microwave retrieval that takes advantage of available lidar data.

The understanding of the subtleties of optimal estimation theory is poor but, as it is a complex theory that is widely misused, I cannot hold that against the authors. If the paper is published, the following issues must be corrected:

- The discussion of the averaging kernel in lines 219 to 228 is wrong. Quoting p. 47 of Rogers (2000), "rows of $\mathbf{A}$ are generally peaked functions . . . with a half-width which is a measure of the spatial resolution of the observing system, thus providing a simple characterisation of the relationship between the retrieval and the true state."

  On line 220, the averaging kernel actually describes the final values' dependency on their true magnitude, in this case indicating the smearing of information across multiple levels. The subspace of state space in which the retrieval must lie is constrained by the a priori covariance matrix.

  I have never encountered the use of degrees of freedom to measure vertical resolution, as in Eq. (6). After conferring with colleagues that work more closely with OE of thermal profiles, we consider it to be at a confusing and poor choice of metric, if not intrinsically wrong. A much more common and robust metric would be the width of the averaging kernels (e.g. the full-width at half maximum of a row of $\mathbf{A}$).

- You still misuse the term 'error' in Section 5.3. In layman's terms, the error is how wrong your measurement was and the uncertainty is how wrong you think it might be. Robust definitions can be found at http://www.iso.org/sites/JCGM/GUM-introduction.htm. The error can be approximated by considering the difference between the retrieved value and a more accurate reference measurement, as you do in Fig. 5. What OEM estimates is the uncertainty on the retrieved value, which describes the range of errors you would expect to see if you infinitely repeated the observation.

  I also understand why you add the word 'theoretical', but it isn't necessary as the uncertainty is a prediction of a probability distribution.

A few more minor points and comments:

- TB is a non-standard (and frankly annoying) abbreviation for brightness temperature. I would suggest BT or T_B.

L145 The word 'drift' doesn't appear in that Whiteman paper. I think you mean the thermal sensitivity of the filters.

L150 There is a subtle point here that, though you don't need to mention it in the paper, you may wish to consider. Poisson statistics state that the variance of a measurement sample is equal to its mean. The lidar community uses this to assume that the value of a measurement is equal to its uncertainty squared. However, that measurement is only one sample from the distribution and is therefore an imperfect estimate of the mean; it's simply the best estimate available. This isn't usually

important but in a statistical analysis, such as OEM, this approximation implicitly states that smaller values are more accurate as they have smaller uncertainty (e.g. the OEM will fit 1 ± 1 more closely than 100 ± 10). Hence, you may wish to investigate if your analysis is biased towards small values in the presence of exceptionally negative noise (i.e. data noticeably smaller than that around it).

L208 The test this sentence describes doesn't match the condition given in Eq. (3). Which one do you actually use?

L234 If you're using the radiosonde data both to determine the lidar calibration factor and as an a priori, why don't you put the calibration factor in the state vector and constrain it (and it's uncertainty) with the a priori? For example, the difference between the blue and black curves in Fig. 2 is about 5%, which would be accounted for by the uncertainty in the calibration factor.

L245 This reviewer is pleased to see correlation matrices rather than covariances.

§3.4 I remain disappointed that you do not consider a more detailed forward model for the lidar. Could this be mentioned as possible future work, in an attempt to inspire other researchers?

Fig. 2 The error bars don't cover the discrepancy between the retrieval and the radiosonde. Does this mean that your uncertainty estimate is too small or is the uncertainty on the radiosonde data large enough for the two profiles to be consistent?

L335 The value at 5 km is consistent with those at 3 and 4 km, so you can't necessarily call that an increase. Your argument is strongest when pointing out that the joint technique gets 3 km closest to the radiosonde.

While the English isn't native, I find it comprehensible and appreciate the thorough descriptions and arguments. I would recommend the lead author spend an afternoon proof-reading the paper, especially the middle sections, as there are a number of misspellings and duplicated words. The technical corrections I caught while reading follow:

- I do not know if this journal prefers ‘ground-based’ to be hyphenated; you use both so please pick one.
- Units are frequently italicised (presumably because they have been included within a `$$` environment). Please consistently use plain font.
- Many of the references list both a DOI and a URL; the URL is redundant.

L2 Nowadays there are a wide

L31 which is difficult to capture with one instrument

L41 Perhaps use ‘have become’ rather than ‘became’ and ‘over recent years’ rather than ‘during the last years’.

L45 You use ‘day time’ here and ‘daytime’ on line 49. Please pick one.

L59 Are you sure you mean ‘features’? I thought the measurements of a MWR would be better described as ‘levels’.

L83 ‘to incorporate’ could be removed without changing the meaning of this sentence. If you prefer to keep it, ‘one’ needs to precede it.

L85 You don’t need to pluralise ‘month’ when used as an adjective.

L111 Raman scattering of the 355 nm beam

L133 During HOPE, BASIL was calibrated

L134 calibration coefficient was estimated by comparing

L144 Use `H$_2$O` rather than `$H_2O$`. Repeated on L165 and L167.

L163 of the K-band contain

L166 liquid water increases with

L195 This equation doesn’t conform to the journal’s style guide.

L197 the MWR and the profile of the mixing ratio

L203 when a perturbation is added to the atmospheric state vector

L222 degree of freedom and can be interpreted as

L215 represent the number of independent

L295 divides the atmosphere into layers

L298 To typeset the second exponential, I would recommend
`\exp\left( -\int_0^s \alpha(s’) \mathrm{d}s’ \right)`

L317 a complete profile from the ground up

L324 a dominant role in defining the vertical

L329 The uncertainty is small in the region

L350 the vertical resolution for only-RL becomes infinite.

L381 during HOPE, and therefore this period

Fig. 5 Invert the order in which the three plots of 5(b) are described to mimic the left-to-right manner in which they are presented.

L462 regions (see section 5.2)

L510 The average total number of DOF

L511 increasing by almost 2 DOF

L542 The magnitude of the increase in RL measurement uncertainty is based on the

L545 error. Therefore, we have

L548 The new averaged errors are very similar

L569 different sensors has come more and more into focus

L616 The page number for Delanoe and Hogan (2008) is D07204.

L619 The page number of Di Girolamo et al. (2004) is L01106.

L656 The page number of Löhnert et al. (2007) is D04205.

L662 The page numbers of Löhnert et al. (2014) are 1157–1174.

L680 An extraneous BibTeX field appears to have been printed between the DOI and year.