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Atmospheric Measurement Techniques Discussions

Interactive comment on "Calibration and validation of water vapour lidar measurements from Eureka, Nunavut using radiosondes and the Atmospheric Chemistry Experiment fourier transform spectrometer" *by* A. Moss et al.

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Interactive comment on "Calibration and validation of water vapour lidar measurements from Eureka, Nunavut using radiosondes and the Atmospheric Chemistry Experiment fourier transform spectrometer" by A. Moss et al.

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Response to Anonymous Referee #2

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"Overall the paper gives significant contribution to a region where a lot of changes have been happening lately due reasonable amount of warming in our climate. The presence of specialized equipment such as a water vapor raman lidar is extremely useful to verify the water vapor budget in the atmosphere. The paper is well written and well structured. There are some weak points though that should be covered in order to allow its publication in AMT, mainly regarding some lack of comments on aspects related to calibration given in the literature that are worth mentioning in this paper. Also

when the word climatology comes to the discussion, 3 years is a very poor sampling period and I would rather call it "TRENDS" other than climatology. There are also some issues I would recommend revising and will list below:"

We thank Referee 2 for the helpful comments on the manuscript, which we have tried our best to incorporate. Upon reading your comments, as well as the comments of the other Referees, we realized how confusing our figures were, particularly the ones concerning calibration. As we were intimately involved with the measurements we know what part of the curves were being used and what part were not, but it was wrong for us to assume the reader could easily figure that out based on the information they were given. While we only use the measurements up to $\sim$6 km altitude, we had included plots of fitting parameters to much greater heights, which was misleading as it suggests we were correcting the measurements at those heights. We have remedied this situation by re-drafting all the figures. In addition to increasing figure quality and font sizes we now are only showing the corrections over the range we actually used them. In the original manuscript the figures suggested, for instance, that we were making huge corrections to our measurements in the stratosphere, when in fact we were cutting off the measurements for low signal-to-noise ratio at much lower heights. We apologize for this confusion and hope that the new figures and improved text make it clear what corrections are actually applying to the measurements, and that over much of the upper range of measurements the corrections are modest (e.g. $<$ 10%).

We have revised the paper as per Referee 2's helpful suggestions, with details below. "Climatology" was a poor choice of words, "trends" is much better and has replaced "climatology" everywhere in the manuscript.

Introduction Page 5667 Line 5 I would put the LAT,LON coordinates right here. Line 11 calibration factor enter the analysis, then the calibration factor are determined - this could be improved. Line 14 Climatology could be exchanged by trends in face of the sampling periods applied to this paper.

1. LAT, LON moved as suggested
2. Line 11 revised
   old: The retrieval method is then discussed to show how the calibration factors enter the analysis, then the calibration factors are determined and compared to radiosonde measurements.
   new: The CEC Lidar must be calibrated to convert its photocount profiles into water vapor mixing ratios. The procedure and results of this calibration are presented in the following section.
3. Here (and elsewhere) "climatology" has been expunged and "trend" is now used.

The CEC LIDAR Page 5667 Line 24 Rapid Ozone variations - How rapid?

A few days (text is appropriately revised).

Transmitter and receiver Page 5668 Line 11 converting a portion... INTO 353 nm General Comment - What is the energy per pulse in each wavelength. Perhaps a Laser Feature Table might be handy instead of the writing throughout the text Line 17 and 18 Is the secondary mirror really at the focus of the telescope?

The manuscript has been amended. The powers at 308 and 353 nm (100 and 10 mJ/pulse respectively and as well as other details of the lidar) are included as well as a reference to the laser specifications (Pal et al.). The erroneous statement about the secondary mirror focus has been removed.

Data acquisition system A schematic drawing of the system might be useful here too. Section 3.1 Equations 1 and 3 Are the $\Delta\omega$q and Δwτ the same quantity. If so why the difference in naming the super and subscripts?

The requested figure already appears in another publication (the Pal et al. reference as well as Moss's thesis) and to keep the manuscript length down we would rather not include it. A sentence has been added directing the reader to the diagram, along with a reference.

The error in EQN (1) noted above has been corrected in the manuscript.

Page 5670 Line 16. For the CEC Lidar measurements the latter option is necessary. Maybe more explanation is needed here. I would mention papers by Sherlock and Leblanc which propose alternative means to derive the calibration which are cited in Whiteman's paper, but for the sake of self-consistency should be discussed in the present paper as well.

We were not able to perform any calibration other than what was presented here (e.g. empirical). We typically can only access the instrument once a year for a few weeks. We did review the calibration literature in this work, as particularly in a techniques journal, where we anticipate the reader will be aware of the substantial contributions in this area by the Table Mountain and ALVICE groups, as well as appreciate our dilemma of having to use empirical calibration. Note that except at the very lowest heights and the very top of our profile no system correction is necessary on the measurements; we only use a calibration constant obtained from the comparisons with the sondes.

Page 5671 Calibration In your results both day and night measurements were used?

Line 21 on pg 5671 says "nights". The Eureka DIAL lidar can only operate at night. We have revised the manuscript to include this fact.

Page 5673 Line 24 Changed enough - Please provide some numbers? Section 3.3 Please add more discussion - How good were your fits? What could be an average and standard deviation for your results. The number of used nights (10?) should be improved in which extent to give better results?

Of course longer measurement nights, and more of them, would improve statistics and our fits. Unfortunately, we are limited here to the measurements from the clearest nights available during the 2007, 2008 and 2009 campaigns. The quality of the fits is shown in Table 1, which has been revised.

Section 4. In my opinion the comparison with satellite data is valid only qualitatively.

The comparisons with the ACE-FTS offer a check of our calibration independent of the radiosondes. Agreeing with ACE-FTS doesn't prove anything, however if we were grossly different it would suggest a serious problem with the calibration in one (or both) of the instruments. We find that agreement between the instruments is as good as more comprehensive campaigns validation campaigns such as MOHAVE 1 and 2 and between other various water vapour instruments in the literature. These comparisons prove little more then that the lidar measurements appear reasonable, both against the radiosondes and against an independent measurement not related to the radiosondes.

Section 6 - Conclusions - More could be added in here. In terms of what could be improved and how are mentioning such as the CFH and first principles but not discussed here. For example how narrow are your filters? What about overlap corrections and so on.

We have revised the conclusion, but we want to keep this paper focussed on the Eureka measurements rather than a broader review of lidar water vapour techniques. The filters are planned to be brought back South for measurement at the next opportunity (hopefully March). The only overlap correction applied is the empirical one discussed in the paper.

References - A good number of papers cover the calibration and intercomparison and fron the references I would search for more titles in the subject.

We have followed your suggestion and have included several new references, including the work of Whiteman, LeBlanc, Sherlock and Venable.

Fig 1. This figure could be improved with larger fonts and less blurry. Fig 2. How did you figure out your Sonde RMS? Fig 3. Details of the how good your fit was are needed. Or even through the text. Fig 4. Some comments as above Fig 7 and 8 - Could they be merged? Also can you make the fonts larger?

All the figures have been redrafted with larger fonts. In addition 2 new figures have been added in a new section at the end showing the spatial-temporal resolution of the system. The RMS was calculated as follows

$$RMS = \sqrt{\sum_{i=1}^{N} \frac{(x_i - \bar{x})^2}{N}}$$

The reduced chi squared of the fits is about 0.2 and the linear fits have regression coefficients greater than 0.98.