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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

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 #3

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"Water vapour is an important climate gas. Instruments like the CEC lidar, that measure water vapour in the high Arctic, are rare and are, therefore, potentially important for our global observing system. Unfortunately the authors show no convincing evidence that the CEC does measure water vapour any better than the Vaisala radiosondes.

To get overall agreement with the radiosondes (and with ACE) the authors introduce large and purely empirical corrections (exceeding 50% of the measured values below

1 km and exceeding the radiosonde values by more than 100% above 8 km). However, there is no physical explanation and there is no quantitative estimate from physical principles for these very large corrections. Instead the large corrections are introduced ad hoc, are fudged to match the sondes and are “explained” with a bit of “arm-waving” only. After reading the manuscript, I get the impression that the CEC lidar does not really measure water vapour (certainly not better than the radiosondes), and that the authors do not know why. This is not sufficient for an AMT paper."

We thank Referee 3 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 addressed Referee 3’s first concern about the how lidar measure-

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ments complement radiosonde measurements by adding a section at the end which shows 2 contours for nightly measurements that demonstrate the large changes that can occur in water vapour even in the winter at these latitudes. These changes would go undetected by twice-daily radiosonde flights. Thus, the radiosondes are important because they give excellent long-term coverage at low temporal resolution, but the lidars offer the ability to track changes in water vapour over periods of minutes. The revised manuscript has a new section showing examples of the lidar's temporal-spatial resolution.

Good point. The original manuscript did not do a clear enough job of explaining our corrections. The text has been revised to explicitly discuss pulse pile-up and geometric overlap. We have tried in the revised text to describe what physical instrumental effects give rise to the measured differences from the radiosonde at the lowest and greatest heights. Since these are empirical corrections with very few measurements affected (6 range bins at the bottom and 8 at the top), we chose the simplest functions that adequately fit the measurements, a 3 parameter exponential at the bottom and a straight line at the top. As part of re-evaluating the fits over a more limited range at the top we no longer require an exponential fit, as the linear fits have regression coefficients greater than 0.98.

While we agree in principle it would be best to better characterize the system, it is not practical to do so. It costs approximately $20,000 to send one student North for 3 weeks to take measurements (assuming we can obtain space on a subsidized charter flight). Once at the site it is an 18 km drive from the sea level weather facility to the PEARL Observatory at 600 m elevation. The drive can be quite a challenge when the winds are blowing. The observers, like Ms. Moss, often can only get to the lab when weather conditions are good, in which case the priority is to get measurements.

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Better characterized measurements are a priority in the future, but given we have been not been able to find any high spatial-temporal resolution of water vapour at these latitudes (except for the recent CANDAC RMR lidar measurements) the data set is so valuable we are willing to make some compromises in the calibration. By the way, it is common in the lidar community to do a variety of styles of calibration ranging from empirical (like in this work) to first principles. We have added substantial material to the text (see the highlighted manuscript) including the following sections.

New in Data Acquisition Section: Tests for signal-induced noise (SIN) on the Eureka system were performed by Steinbrecht (1994). His results showed the SIN is more important at 308 nm than any non-linearities in the counting system. He also showed that SIN at 353 nm was small (but significant), and negligible at longer wavelengths. Since this study uses measurements at 385 and 406 nm SIN effects, if present, should be small.

New in Introduction to Calibration Method: The most important height-dependent corrections that may need to be applied to the raw photocount profiles at the lowest altitudes are corrections for phototube nonlinearity (e.g. pulse pile-up) and geometric overlap of the transmitted and received beam. At higher altitudes SIN can be an issue (as discussed previously). Ideally one tries to measure these effects to correct the raw measurements. However, due to the limited time available on site during the observing campaigns a careful sorting out of these effects was not possible, so it was decided instead to use an empirical correction using the radiosonde measurements of relative humidity, as will be discussed in detail below.

Section 3.2.3, Fig. 1: Where does the large difference come from, that is corrected here? Is it from pulse pileup in the detection system (Donovan et al. 1993)? If so, then this should be measured and quantified. Or is it from the lack of overlap between receiver field of view and transmitted laser beam? In the

latter case the correction would change all the time with changing system alignment, and a meaningful correction can probably not be obtained. Water vapour profiles, that are accurate to a few percent, can probably not be obtained in this "lack-of-overlap" range.

This is an excellent point and the answer is both. The variability of the correction from season to season is certainly predominantly due to alignment. We have added the following to the end of the final paragraph of section 3.2.3:

The major factors influencing the calibration at the lowest heights are any counting non-linearities in the nitrogen channel and geometric overlap corrections due to alignment. The significant variations of the corrections at the lowest heights from season to season are primarily due to changes in the system's alignment, in large part a consequence of the system being shutdown and "winterized" for 11 months a year, a procedure that takes 2 days.

Section 3.2.3, Fig. 3: Again, where does this huge difference come from? Above 8 km the lidar values are 2 to 5 times higher than the radiosonde values, which are likely not good and too high at these altitudes. With such a large and unexplained correction, the lidar simply does not measure water vapour at these altitudes, it sees something else.

As discussed in the first paragraph of our reply, what appeared to be huge differences were in a region where we were not using the measurements (e.g. $>$ 6 km). The new figures show that the corrections are quite modest where the lidar signal-to-noise ratio is reasonable. The radiosondes used in this work are *not* reliable above about 6 km due to the extremely cold

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temperatures, whereby hysteresis in the sensors makes the RH humidity measurements unreliable.

Contrary to what the authors claim, I don't think it can be the transmission ratio. There is little atmosphere, and hardly any water vapour above 8 km over Eureka in late winter. There would be no significant altitude dependence of the transmission ratio above 8 km. Certainly it would never explain a correction by factors of 2 to 5.

The same goes for the temperature dependence of the effective Raman scattering cross-section, wrongly claimed by the authors. When the temperature of the scattering layer goes up, higher Stokes orders become more probable, but may not be transmitted by the interference filter. This depends on filter bandwidth (what is used in the CEC Dial?), but is usually a small effect. It would be even smaller above the polar tropopause (8 to 10 km), where temperature changes little with altitude.

As discussed in the first paragraph, we apologize for the confusion our figures caused in regard to correction. We have re-written this paragraph to de-emphasize the effect of transmission ratio and to include discussion of possibility of alignment affecting the measurements. It would be ideal if we could visit the instrument specifically to investigate any possible problems in this area. We hope to remove the filters this spring and bring them back South to be re-measured.

I rather think that there is a problem with the background measured and subtracted from the lidar return signals. How have the authors handled the background. Is there significant fluorescence or signal-induced noise? These things need to be addressed.

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We have carefully looked at the background signal and it is constant with height, verifying the original measurements on this system by Steinbrecht. SIN has been investigated as discussed earlier.

Without a better handle on the lidar measurements, the comparison with ACE (Figs. 6 to 8) is hardly meaningful. Why not use the radiosondes? They have errors too, but there are many routine radiosonde launches, and there is ample documentation of radiosonde humidity accuracy in the literature.

The comparisons with the ACE-FTS offer an independent check of our calibration independent of the radiosondes. Agreeing with it 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 validation campaigns such as MOHAVE 1 and 2 and between other various water vapour instruments in the literature. These comparisons prove little more than the lidar measurements appear reasonable, both against the radiosondes and against an independent measurement not related to the radiosondes. In addition, the ACE-FTS comparison allows us to check our high-altitude correction in a region where the radiosonde measurements are not useful (due to temperature effects).

Given these major deficiencies, I feel that a substantially deeper new investigation is needed. After that, it would be worthwile to re-submit. The CEC lidar should be able give accurate water vapour measurements in the higher Arctic troposphere.

We hope the extensive changes and revisions we made to this manuscript as well as the inclusion of the higher spatial-temporal resolution measurements makes the Referees more comfortable with the validity of this unique

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set of measurements. We will address more rigorously the water vapour corrections discussed in the paper if the Eureka facility remains open and we have any further access to the equipment. As the contour plots suggest, the 39 nights of measurements available will be the most detailed high spatial-temporal measurements set available for understanding tropospheric water vapour during the Arctic sunrise.

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