Atmos. Meas. Tech. Discuss., 5, C3811–C3819, 2013
www.atmos-meas-tech-discuss.net/5/C3811/2013/

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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Received and published: 9 February 2013

[11pt]article geometry a4paper

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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.

R. J. Sica

9 February 2013

Anonymous Referee #1

Received and published: 28 September 2012

“This paper describes rare results from the Eureka (Canada) PEARL Differential Absorption and Raman lidar measurements, in this case water vapor profiles. Measurements such as those described are rare in this region. Unfortunately, I find the measurements described in this manuscript of poor quality, in need of unusually large empirical corrections. Because of this, I strongly suggest major revisions to the manuscript, with the revised objective to fully characterize these corrections (i.e., including uncertainty and stability in time) in order to provide a minimum of credibility to any subsequent science or validation work utilizing the data. Considering the challenges met by the investigators, the methodology used to correct the data as described in the manuscript is somewhat understandable and relevant. However, the magnitude of the corrections point towards a need to revisit the instrumental design. Many water vapor Raman lidars exist today with a careful design of the receiver allowing a data processing free of the empirical corrections described here. Not surprisingly, and as mentioned by the authors, applying two exponential functions to fit the bottom and top of the profiles (what's left after that?) lead to temporally unstable measurements for any mid- and long-term studies (recalibration needed at short intervals). Each fitting function is a 3-parameter exponential function having very little (or no) physical meaning, and with the sole objective to make the lidar profiles eventually agree with the radiosonde profiles. It is therefore not surprising to see the lidar and radiosonde profiles agree (in average) within the stated uncertainties after the signals have been corrected using the radiosonde as reference. I believe this agreement simply reflects the accuracy of the fits. In order to be published, this manuscript must include additional information on the statistical significance of the corrected profiles, and on the "life-time" i.e., temporal stability/variability of the correction functions. The real question here is: Can the measurements calibrated as described in the manuscript with a correction applied at the beginning of a measurement period reflect the state of the atmosphere and be physically interpreted throughout this measurement period without referring to the correction process? In other words, are the CEC lidar measurements doomed to simply replicate the radiosonde measurements?"

We thank Referee 1 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 ~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 agree the instrument is in need of redesign, including consistent monitoring of a white light source (which was not a commonly practice when these measurements were made) as well as an upgrade to the counting and data acquisition system. Unfortunately, these upgrades were not available for the 2007–2009 measurements presented here. Given their "rare" nature we believe they should be made available to the community despite a less than optimal setup. To repeat something in answer to a comment below, we are severely constrained in our ability to afford visits to PEARL (e.g. $20,000 to send one student North for 3 weeks to take measurements assuming we can obtain space on a subsidized charter flight, which if we can't means paying thousands of dollars to charter: Eureka is *not* a town/settlement, it is a weather station with no commercial flights and no residents).

We agree that the time variations of the calibration are important. This is why in the original (and revised manuscript) we showed the flight to flight variation, as well as the year to year variation, of the calibration so the reader could get a feel for the variability.

Addressing Referee 1's final concern, about replicating the radiosondes, we now see after reading your comments that it was a poor decision to not show the contours in the original manuscript which make it clear how the lidar measurements capture variability in the water vapour that would not be seen by radiosondes. The revised manuscript has included 2 contour plots which emphasize this capability. Radiosonde flights are available only every 12 hours at Eureka, while our measurements show water vapour can often vary over tens of minutes.

In the text below we have highlighted how we have tried to improve the paper by integrating your suggestions.

Specific comments/suggestions:

Section 2.2: A description of how the 385 nm and 406 nm signals reach the detectors would help. Considering the observed moist bias in the UT, it would be interesting to know if the receiver design is likely to be sensitive to fluorescence.

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, and negligible at longer wavelengths. Since this study uses measurements at 385 and 406 nm we have not discussed SIN. We have added a summary of Steinbrecht's work and a reference to the text.

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Discussion Paper

Section 3.1: Melfi et al., APL, 1969 would be more appropriate for the original source of the water vapor Raman lidar formulae. Also check your extinction term, index "q" is first used, index "tau" is used afterwards.

Reference to Melfi's pioneering work in this area is included in the revised paper. EQ(1) is corrected in the revised manuscript as well.

Section 3.1, MODTRAN: The paper by Berk et al. describes MODTRAN for wavelengths greater than 1 micrometer. Is there a different reference that points out to the model at UV wavelengths?

Yes, we have replaced the Berk et al. work with an early one that does include the UV:

Anderson, G. P., J. H. Chetwynd Jr, J. M. Theriault, P. K. Acharya, A. Berk, D. C. Robertson, F. X. Kneizys, M. L. Hoke, L. W. Abreu, and E. P. Shettle (1993), MODTRAN2: Suitability for remote sensing, pp. 514–525.

Section 3.1, height-dependent calibration terms: There are two major height-dependent calibration terms that are not even mentioned and which surely are the source of the bottom empirical correction: signal saturation (pile-up) and telescope-beam overlap. These should be mentioned, and addressed in the context of the applied corrections. Can they be separated? Which one plays a greater role? Can they be corrected for in a physical manner? (which would provide a higher credibility to the corrected results)

Referee 1 makes a good point here. The text has been revised to explicitly state about pile-up and overlap. While we agree in principle it would be best to better characterize the system, it is not practical to do so. It costs

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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.

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.

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.

Section 3.2: I think the saturation vapor pressure equation used by Vaisala in their internal sonde calibration is from Hyland and Wexler, ASHRAE, 1983. Though the magnitude of the differences with Murphy and Koop 1985 are probably small compared to the magnitude of the empirical corrections, I would suggest using it in order to remain consistent with Vaisala.

The entire data set was re-processed with the Hyland and Wexler model.

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Referee 1 is correct that the differences were small (less than 0.03%). We have revised the text with the re-processed measurements using the Hyland-Wexler 1983 model.

Section 3.2.2 "from the surface": How can the lidar sample the surface? The lidar is blind in the lowermost layer of the atmosphere. What is the starting measuring altitude above the instrument? Please specify.

Referee 1's point is correct, "surface" has been changed to "0.94 km."

Section 3.2.3, "An exponential fit is used": What is actually fitted? I assume it is the ratio of the lidar signal to the radiosonde, but this has to be mentioned at least once in the manuscript (it is mentioned for the upper part correction but not for the lower part).

The text has been changed as Referee 1 suggested.

Section 5: The discussion on the water vapor climatology is much too short and undocumented. Some "effects of the vortex" are mentioned but no specific details are given, making the discussion completely useless. Please expand or delete.

This section has been dropped; a detailed study with meteorological analysis of the vortex location is in progress.

Section 6: The conclusion mentions "10-min contours", but those are not shown in the C2353 paper. A conclusion must summarize what has been described in the paper. Please add the contours as part of the results, or remove this sentence from the conclusion.

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We now realize not showing contours was a mistake on our part. They were in the earlier draft of the original manuscript but we removed them. We added a new section to the paper which shows 2 contours that demonstrate the large changes that can occur in water vapour even in the winter at these latitudes, that could go undetected by twice-daily radiosonde flights.

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