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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.

Anonymous Referee #1

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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?

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.

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

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first used, index “tau” is used afterwards.

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?

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)

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.

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.

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).

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.

Section 6: The conclusion mentions “10-min contours”, but those are not shown in the

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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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