Review of "On retrieval of lidar extinction profiles using Two-Stream and Raman techniques" by I. S. Stachlewska and C. Ritter

This paper compares range-resolved extinction and backscatter coefficients derived using a 'two stream' inversion to the same quantities derived using the well established Raman technique. The two stream method is applied to elastic backscatter lidar measurements made by a nadir-pointing airborne lidar (AMALi) and a zenith-pointing ground-based system (KARL). Because KARL is equipped with a Raman detection channel, KARL also provides the measurements used in the Raman retrievals.

The idea of validating the two stream retrieval methods with simultaneous Raman measurements is a good one, and one that has not, to my knowledge, been previously presented in the peer-reviewed literature. The authors do a good job of explaining the mechanics of the two stream technique, and (equally important) of discussing its applicability and limitations. However, there are several substantial obstacles that should prevent the paper from being published in its current form. Below I provide lists of both general and specific remarks that I hope the authors might consider in crafting a revised manuscript.

General Comments

The paper contains numerous spelling and typographical errors; see for example pages 20231 ('commonly' on line 5, 'usually' on 20, 'severely' on line 21, and 'rarely' on line 23) and 20235 ('coefficient' on line 4, 'mathematically' on line 20, 'severely' on line 21, 'subtracted' on line 27, and 'subtracted' on line 28). An English language spell checking utility should be sufficient to repair most of these mistakes. However, there are also many other types of errors that cannot be cured by a spell checker alone (e.g., verb tense, subject-verb agreement, and missing articles). I encourage the authors to have a native English speaker thoroughly review their paper before the resubmit it.

The KARL calibration procedures seem unnecessarily complicated and/or are not clearly explained. The relevance of the sun photometer measurements described on page 20236 is not readily apparent without reviewing the material in Appendix C. I would suggest moving parts of Appendix C into the main body of the paper. Furthermore, with regards to the discussion of additional calibration constraints provided in Appendix C:

- Presumably the KARL data were always cloud-free above 10-km. In that case, the accuracy of C_K would depend on the accuracy of the sun photometer measurement $\left(\tau_{\text{sun}}^{\text{part}}\right)$, the fraction of the total column optical depth that lies above 10-km, the aerosol scattering ratio in the calibration region, and the fidelity of the molecular model used. I do not understand the additional requirement that $\tau_{\text{KFS}}^{\text{part}}$ lie within 10% of $\tau_{\text{sun}}^{\text{part}}$. Achieving this degree of correspondence will be influenced by the selection of lidar ratio (as pointed out by the authors), assumptions about extinction in the KARL overlap region, and the SNR of the profile between the ground and 10-km. While all of these are important in retrieving an extinction profile using the KFS method, none of them are germane to the actual instrument calibration.

- The match between τ_{KFS}^{part} and τ_{TS}^{part} in the two-stream solution region will also depend on the lidar ratio chosen for the KFS solution. Why should the ability to guess the lidar ratio in this region have any influence on the computation of the calibration coefficient? The appropriate lidar ratio for the KFS solution should depend on the value of the calibration coefficient, not the other way around.
- A lidar ratio of 12 sr for cirrus clouds seems unrealistically low. HSRL measurements reported by Eloranta et al. (2006) suggest that a value around 28 sr would be more appropriate for Arctic cirrus.
- On page 20249, line 14: what does the flight altitude of AMALi have to do with the calibration of KARL? As it is necessary to calibrate the KARL data prior to retrieving the two-stream lidar ratio estimates, I wonder why the authors are only concerned with the specification of the lidar ratios above AMALi.

The authors state that "the applicability of the Two-Stream method depends critically on the constraint that both lidars probe into the same air masses". What then is the RMS distance between the KARL site and the AMALi footprint for those profiles selected by the correlation test described on page 20235? Was the correlation coefficient a strong function of the distance between the two measurements? In the Abstract and again in the Introduction, the authors assert their intention to investigate the feasibility of applying the two-stream method to CALIPSO data. It seems to me that understanding spatial correlations temporal variabilities will be critical to this effort, especially when considering the very poor SNR of the CALIPSO lidar.

The readers' understanding of the source(s) of the disparities shown in Figure 1 would be improved by including color coded time-height plots of the attenuated backscatter measurements made by both KARL and AMALi. Such plots would be especially useful in interpreting the 15 May results, where the vertical extents of the two-stream and Raman layers are significantly different. Adding error bars to Figures 1–3 would also be helpful.

Specific Remarks

page 20230, line 19: the lidar satellite is spelled 'CALIPSO', not 'CALYPSO'.

page 20233, ~line 11: Is neglecting multiple scattering justified by the instrument and measurement geometries of the lidars used?

page 20234, line 16: Since the variability of lidar ratio distributions reported in the literature is in the neighborhood of 40% or larger (e.g., Sassen & Comstock, 2001; Chen et al., 2002), I wonder how the lidar ratio of any specific cirrus cloud could be sufficiently well-known to use a priori in deriving a profile of backscatter coefficients. An uncertainty of 40% would seem to be a huge impediment to retrieving a high quality backscatter solution.

page 20235, line 11: Does that phrase "easily detected as a shift in the data sets" mean only that the correlation coefficients were unusually low? Or is something additional implied?

page 20235, line 14: I believe the term 'overlap function' is much more widely used – and hence much more readily understandable – than 'geometrical compression'. (Searching Google Scholar for {lidar AND (cloud OR aerosol) AND "geometric compression"} yields 12 hits; {lidar AND (cloud OR aerosol) AND "overlap factor"} returns 312 hits.)

page 20235, line 18: What constitutes “sufficient SNR”? Later on in the Discussion section, the authors state that an “SNR of at least 100 is required”. This limit should be specified earlier in the manuscript. If this is the SNR specification for a single 60 m range bin, that point should be clarified as well.

page 20237, lines 1–4: I’m afraid I do not understand the intended meaning of this paragraph.

page 20237, lines 5–14: The authors appear to be saying that the scattering ratio in the “aerosol-free calibration range between 10 [and] 12 km” is $R = \frac{\beta_m + \beta_a}{\beta_m} \approx 1.30 \pm 0.05$. This hardly seems like an “aerosol-free” region.

page 20237, line 15: It’s not clear whether the boundary condition for obtaining $\beta_{TS}^{part}(h)$ was C_K or β_{ref}^{part} (though perhaps it doesn’t matter, since presumably these are mutually consistent values).

page 20237, line 21: Please add a reference for “[the] standard for KARL’s Raman returns”.

page 20237, line 25: Was the 10-minute attenuated backscatter measurement centered within the 20-minute Raman data acquisition sequence?

pages 20238–39 : This section could probably be shortened a bit. While the authors provide a fine description of the meteorological context in which their measurements were acquired, it seems to me that, for a paper that intends to validate a specific measurement technique, it would be much more useful to describe the differences seen in the two sets of retrievals.

page 20238, line 23: Please add a reference for the assertion that lidar ratios of ~20 sr and extinction coefficients of ~1.5 x 10^{-5} m^{-1} are characteristic of ‘clean Arctic air’.

page 20239, line 21: Should “uniform strait transport form” actually be “uniform transport straight from”?

page 20240, line 3: Liu et al., 2006 describes useful procedures for estimating λ.

page 20241, line 6: Here and elsewhere, change ‘insecurity’ to ‘uncertainty’. In the context of data analysis, the two words are not synonymous.

page 20243, line 9: Instrument noise seems a highly unlikely culprit for the sorts of differences seen in Figure 1.

page 20245, line 9: I think the authors mean ‘accurately’, not ‘precisely’.

page 20245, line 16: I am skeptical of the authors’ claim that “small spherical water droplets” could produce lidar ratios of ~80 sr. Using a Mie calculator, I have to posit a modal droplet radius of less than half a micron to begin to approach a lidar ratio of 80 sr for gamma-distributed spherical water droplets. However, a cursory literature search suggests that the smallest measured droplet sizes are a good deal larger (e.g., Garret et al., 2004).

page 20245, Fig. 3: What explains the difference of ~50% seen below ~1 km in the extinction and backscatter profiles?

References

Chen, W. N., C. W. Chiang, and J. B. Nee, 2002: "Lidar ratio and depolarization ratio for cirrus clouds", *Appl. Opt.*, **41**, 6470– 6476.

Eloranta, E. W. , I. A. Razenkov, and J. P. Garcia, 2006: "Arctic Observations with the University of Wisconsin High Spectral Resolution Lidar" in Reviewed and Revised Papers Presented at the 23rd International Laser Radar Conference, C. Nagasawa and N. Sugimoto, Eds., pp. 399–402.

Garrett, T. J., C. Zhao, X. Dong, G. G. Mace, and P. V. Hobbs, 2004: "Effects of varying aerosol regimes on low-level Arctic stratus", *Geophys. Res. Lett.*, **31**, L17105, doi:10.1029/ 2004GL019928.

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Sassen, K., and J. M. Comstock, 2001: "A midlatitude cirrus cloud climatology from the facility for atmospheric remote sensing. Part III: Radiative properties", *J. Atmos. Sci.*, **58**, 2113–2127.