

Interactive comment on “Potential of remote sensing of cirrus optical thickness by airborne spectral radiance measurements in different viewing angles and nadir geometry” by Kevin Wolf et al.

Anonymous Referee #1

Received and published: 17 November 2016

The paper “Potential of remote sensing of cirrus optical thickness by airborne spectral radiance measurements in different viewing angles and nadir geometry” presents a very interesting sensitivity analysis of the influence of different parameters in the measured upward radiance using radiative transfer simulations in presence of cirrus. Results from in-situ aircraft measurements using SMART and a scanning mini-DOAS are also presented. The paper is suitable for publication in ACP after some minor corrections and improvements are performed. See detailed comments below:

In general, I don't see a clear connection between the sensitivity analysis presented in

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Section 2 and the results and discussion presented in Section 4. Some paragraphs or sentences explicitly linking the two sections where necessary would be appropriate.

Abstract, lines 17-18. The simulations indicate that off-nadir measurements are more adequate to retrieve τ of thin clouds, but that is not observed in the retrievals from the aircraft measurements presented here (at least in the way they are currently presented). Please, rephrase.

Page 2, line 19. “better quantify” instead of “quantify better”. It is not clear what you mean by “appear worthwhile”, rephrase.

Page 3, line 1. Add a comma after “relevant parameters”

Page 3, line 5. Elaborate more the statement “As a result, airborne remote sensing is required to bridge local in-situ and global satellite observations.”

Page 3, line 20: “and are not routinely be used in trace gas measurements” is not clear. Please, rephrase.

Page 5, line 5. The use of the acronym SZA and the symbol θ_0 for the solar zenith angle is redundant. Remove the acronym.

Page 6, figure 2. In the lower part of the figure it will be more convenient to plot the relative differences normalized to the Radiance. That will help with the corresponding discussion in lines 13-16. Also, some text is missing in the figure caption.

Page 6, line 3. Replace “wavelengths less. . .” by “wavelengths lower. . .”

Page 7, lines 4-5. “The RTS suggest that off-nadir observations at near infrared wavelengths ($\tau > 900$ nm) are more suitable for the detection of SVC and cirrus.”

Page 8, figure 4 and lines 9-13. Because of the different values of I under the different constraints you should consider providing the sensitivity in percentages.

Page 9, line 2. Do you mean “thick clouds, for larger optical thickness. . .” here?

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Page 9, line 13. Remove “especially”

Page 9, line 25. You should consider include a plot with the steepest derivative γ (maybe a subplot in Figure 5?)

Page 11, figure 6. Please, include a subplot with the relative differences between the different ice crystals. This will help with the discussion in lines 8-13.

Page 12, line 6. “were investigated”

Page 12, line 11. Provide references for SMART and the calibration procedure.

Page 12, line13-14. Provide references for the mini-DOAS and the DOAS technique.

Page 13, line 21. The symbol ILmD has not been defined before. Please, define.

Page 14, line 26. Why are multiple scattering effects neglected?

Page 16, Figure 8. Can you add the error bars to the plots? Especially to plots b and d. Idem for figure 9.

Page 19, lines 26-27. Please, elaborate the statement “These stop criteria determine the accuracy of the iterative retrieval.”

Page 20, lines 1-15. What happens for off-nadir observations?

Page 21, Figure 12. Axis labels are missing.

Page 22, lines 14-15. Are these average values obtained for the coincident measurements only? Otherwise, comparing the different values is not realistic. Especially for the DOAS off-nadir, which have a smaller temporal resolution and does not capture all the variability observed during the analyzed period.

Page 23, line 3. A more in-depth analysis of the uncertainty will be useful, mainly for inter-comparison purposes between the different datasets presented in figure 14.

Page 23, line 9 and figure 14. It looks like there is a better agreement between the

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DOAS off-nadir and the reference WALEs than between the DOAS off-nadir and DOAS nadir or SMART. Can you comment something on that? Can you further discuss the advantages and disadvantages of having nadir and off-nadir measurements and link it with the sensitivity analysis in section 2?

Page 23, lines 20-21. This statement is not clear. If the data points contaminated by the second cloud layer are excluded from the calculations, what do you mean here?

Page 24, lines 10-12. This is not clear either. From the results and the discussion presented before, it looked like you were using the wavelength of 532 nm for all the instruments. Please, clarify where necessary.

Page 26, line 14. Agreement is within the uncertainty but I would not consider a 66.6% Numerical values for the differences between DOAS nadir and Wales and DOAS off-nadir should be included separately. Relevance was given to the comparison between the nadir and off-nadir observations in the sensitivity analysis and it will be interesting to do a clear distinction also for the in-situ airborne data and include a significant conclusion at this respect.

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