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Atmospheric Measurement Techniques Discussions

Interactive comment on "Optical thickness and effective radius of Arctic boundary-layer clouds retrieved from airborne spectral and hyperspectral radiance measurements" *by* E. Bierwirth et al.

Anonymous Referee #1

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Title: Optical thickness and effective radius of Arctic boundary-layer clouds retrieved from airborne spectral and hyperspectral radiance measurements

Author(s): E. Bierwirth et al.

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This paper presents airborne remote sensing measurements of reflected irradiance

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Interactive Discussion

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(SMART-Albedometer) and radiance (SMART-Albedometer and AisaEAGLE hyperspectral imaging camera) above Arctic (Norway) boundary-layer clouds. The measured radiances are used to retrieve the cloud optical thickness and droplet effective radius of clouds. Results from measured irradiances are not discussed in the paper. The authors describe a method used to designate common pixels between the nadir radiance measurements of the SMART-Albedometer and the radiance measurements of the AisaEAGLE imaging camera. The authors compare the radiance measurements in the common pixels of the SMART-Albedometer and the AisaEAGLE imaging camera and find good correlation. The authors then investigate two different cloud retrieval approaches based on reflected radiance measurements; a 2-wavelength approach and a 5-wavelength approach. They conclude that while 2-wl is adequate to retrieve cloud optical thickness, the retrieval of droplet effective radius is more sensitive to the retrieval method applied. They summarize that time delays between in situ measurements and the remotely sensed measurements prevents closure about which retrieval method is the best.

I find the manuscript requires additional direction and clarification. I don't think this will require additional calculations. Because of the required text additions and possible rearrangement of text, I advise to accept this paper with major revision. Below, I list my general comments justifying this decision. Following are specific comments that should be addressed to improve the readability and understanding of the paper. Lastly, are technical comments of a minor nature.

General Comments 1. What is the primary purpose of the paper? I don't get a good indication of the direction of the paper. Is the primary purpose to:

a. Retrieve Arctic cloud properties,

b. Intercompare a 2-wl or 5-wl cloud retrieval method with 'closure' provided by comparison to in situ measurements, or

c. Introduce the AisaEAGLE hyperspectral imaging camera?

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I offer several suggestions, depending on the authors answer to this question.

For an answer 'a', I find that the authors present histograms of retrieved optical thickness and droplet effective radius (Figure 7), yet no discussion is given as to what was learned from this addition of data to the 'data base of the Arctic climate system' (line 11, p 7755). Many of the important issues in the role clouds play in Arctic climate touched on by the authors in the introduction were not discussed further in the text. Namely, validation of satellite cloud retrievals for measurements over a bright surface, mixed-phase clouds, and verification of retrieved products impacted by instrument errors (this topic is discussed by authors) and forward model errors. A more developed description of the experimental data will help. Were the measurements only over ocean and not a bright surface? Of the 13 research flights, is it correct you are presenting results from 1 flight and do the other 12 research flights add new results for the Arctic data base? In the introduction, mixed-phase clouds are emphasized; are your results for water and ice phases? Is there the possibility of comparison with satellite results?

If the answer is 'b', I find the need for additional discussion. The authors, by propagation of measurement error, make determinations of retrieval performance for two different retrieval methods. However, as they discuss in the introduction (page 7755, line 4) the assumptions made in the radiative transfer model also impact retrieval error. The authors do not reference recent work that has been done to investigate the impacts of both measurement and model errors on retrieved cloud properties. Such impacts should be included for an attempt to 'provide closure' between the retrieval methods. L'Ecuyer et al. [2006] and Coddington et al. [2012] are two examples of the objective assessment of retrieval errors on retrieved cloud properties. Recent work by King and Vaughan [2012], use a similar form of objective assessment in the retrieval of the vertical profile of cloud droplets, which is a topic that is mentioned in this manuscript to be the reason for discrepancy between in situ and retrieved cloud optical properties.

In addition, the authors discuss that time delays between in situ measurements and the remote measurements prevent providing closure between the 2wl and 5wl methods. It isn't clear in the authors discussion that the in situ data presented was obtained in such a manner (such as statistics of vertical profiles of in situ measurements by Min et al., 2003] to provide a rigorous intercomparison to the remote measurements. What is the maximum time difference allowed between comparing in situ measurements and remote measurements?

The authors also did not discuss the non-orthogonality in cloud optical thickness and droplet effective radius for the thin clouds presented in this study. For water clouds of thickness less than around 40, it's well known that errors in optical thickness can propagate into errors in effective radius (or vice versa) [for example the Nakajima and King, 1990 paper that is cited].

Finally, more description should be included regarding the inputs used in the radiative transfer model: What is the spectral resolution of SMART, and the AisaEagle?; Was the surface boundary condition assumed to be ocean?; Was the Sunphotomer measurements from ground or air, and what was the relationships of aerosols with respect to clouds (under, above)?; Was their knowledge of the aerosol absorbing and scattering properties?; What assumptions were made in the meteorological profiles above the level of the dropsonde? What cloud scattering properties did you use and how were they developed (i.e. model for Mie scattering for water clouds, or what ice crystal model for ice clouds)?

If the answer is 'c', I find this topic is more developed and request only some additional discussion. You refer to the benefit of the off-track pixels with the hyperspectral imager data, yet do not (I believe) include relevant discussion or support for this statement. Please expand on this important point. For example, further discussion regarding the data shown in Figure 10, and implications of added potential information available from the hyperspectral imager for heterogeneous clouds.

Some references for the comments above: L'Ecuyer, T. S., P. Gabriel, K. Leesman, S. J. Cooper, and G. L. Stephens (2006), Objective assessment of the information content of

visible and infrared radiance measurements for cloud microphysical property retrievals over the global oceans. Part I: Liquid clouds, J. Appl. Meteorol. Climatol., 45, 20–41, doi:10.1175/JAM2326.1.

Coddington, O., P. Pilewskie, and T. Vukicevic (2012), The Shannon information content of hyperspectral shortwave cloud albedo measurements: Quantification and practical applications, J. Geophys. Res., 117, D04205, doi:10.1029/2011JD016771.

King, N. J. and G. Vaughan (2012), Using passive remote sensing to retrieve the vertical variation of cloud droplet size in marine stratocumulus: An assessment of information content and the potential for improved retrievals from hyperspectral measurements, J. Geophys. Res., 117, D15206, doi:10.1029/2012JD017896.

Min, Q.-L., M. Duan, and R. Marchand (2003), Validation of surface retrieved cloud optical properties with in situ measurements at the Atmospheric Radiation Measurement Program (ARM) South Great Plains site, J. Geophys. Res., 108(D17), 4547, doi:10.1029/2003JD003385.

Specific Comments

Some of these comments may repeat above requests, but I have provided line numbers to direct the authors.

Page 7755, line 4; Instrument uncertainty and forward model assumptions occur with all remote sensing platforms (ground, air) and not just satellite. As it currently reads, it sounds like the air and ground-based measurements/retrievals do not have this problem.

Page 7755, line 27-30: I like that you have defined ‘hyperspectral’. The given definition does confuse me a little as a hyperspectral imaging cube has two spatial dimensions (x and y) and a spectral ‘z’ dimension. The added time dimension you include would result in 4-dimensional dataset. Is this in line with your definition?

Page 7758, line 11 and Figure 4: The figure and text would be easier to interpret if you added an altitude (km) scale.

Page 7758, Description of RT calculations: Please see request for more details (above) under the paragraph beginning, if the answer is ‘b’

Page 7759, line 26 through end of paragraph: first, change ‘reflectance’ to upwelling radiance. I believe that is your measured quantity that you want to discuss; if you are normalizing by the downwelling please define as such in your paper. (Check for consistencies throughout paper to make sure you aren't bouncing back and forth between reflectance and radiance unless that is your desired intent).

Page 7760, line 1-16: Coddington et al. [2012] investigated the added information information in adding retrieval wavelengths. They found, even for optical thickness, that information with additional wavelengths was gained due to a reduction in radiometric uncertainty. The improved knowledge in optical thickness, could propagate over into improved knowledge in effective radius because, for clouds of optical thickness less than approximately 40, the lines of constant effective radius and cloud optical thickness are not orthogonal. For example, your figure 5 (bottom) shows larger uncertainty in optical thickness (from measurement error propagation) north of 75 degrees. Including a reference to Platnick, 2000 or similar would be suggested in discussing the change in cloud droplet size with height within a cloud. Both 2-wl and 5-wl would be expected to reach same penetration depth within the cloud, being that their longest retrieval wavelength is near 1600 nm. King and Vaughan (reference available above) use information content in the retrieval of the vertical profile of cloud droplet. In addition, what references can you provide that show in situ measurements can be used to validate remote sensing measurements? Lastly, please provide a more in-depth synopsis (perhaps add a cartoon or figure, or build on figure 4) showing a) the time delay between the remote and in situ measurements, b) the location of cloud, and location within/above cloud for remote sensing and in situ measurements.

Page 7762, line 12: What maximum time difference do you allow between in situ and

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remote sensing measurements? In addition, with these different choices for determining effective radius, what is the size of the entire field? It would also be nice to know the spread/distribution in the different five options for effective raiuds.

Page 7763, line 5: It is difficult to understand why the in situ values of effective radius, when considered only at the same aircraft location of the remote sensing measurements, would be a poor choice, yet the average of all in situ effective radius over entire field would give such a good choice. Please expand. Likely the request for added discussion (above) will help: Lastly, please provide a more in-depth synopsis (perhaps add a cartoon or figure) showing a) the time delay between the remote and in situ measurements, b) the location of cloud, and location within/above cloud for remote sensing and in situ measurements.

Page 7763, line 20: Can you provide some indicator of spatial scale (lat/lon or km) on Figure 10?

Page 7764, line 15 through 20: The discussion of the difference in histograms shown could be more developed. Are conclusions contradictory? For example, the shape of cloud property distribution of SMART albedometer is 'the same' as AisaEAGLE (12) yet on line 19, differences in distributions are attributed to off-track deviations. Technical Comments

Page 7756, line 8: "Parts of the instrumentation. . ."; replace Parts with Some.

Page 7756, line 16: "The AisaEAGLE covers the . . ."; replace covers with measures.

Page 7756, line 25: replace 'fibre' with 'fiber'.

Page 7757, line 5: "have an own Inertial. . ."; replace an with their.

Page 7758, line 8: "On the aircraft,.."; replace On with From

Page 7759, line 26: The LUT discussion indicates gridded values are in radiance units. Please define reflectance that you are using, or perhaps change reflectance to reflected radiance, as reflectance typically means the reflected light has been normalized by incident light.

Page 7761, line 19: The Use of the symbol :=, what does it add to the text? What do you lose by simply using the '=' symbol?

Page 7761: line 20 and 23: You have used the variable 'd' in two defintions. First as the width of the observed strip of cloud in AisaEagle's filed of view. Second, as the distance covered by the radiance spot on the cloud top.

Page 7762, line 2: Incorrect relational operator (should be « to support your assumption).

Page 7763, line 13: Include pointer to Figure 9 somewhere in the discussion.

Page 7764, line 16: "that are more pronounced."; awkward end to the sentence allows for ambiguity in interpretation. Pronounced with respect to...?

Figure 2: "marked with crosses at exemplary wavelengths."; Please explain as the data shown is comparisons of radiances at a single wavelength, 870 nm.

Figure 7: I would suggest swapping the order of the plots, for consistency with Figures 4, 5 and 6.

Figure 12: I may have missed it in text, but are the results shown for 2wl or 5wl?

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 7753, 2012.

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