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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Airborne hyperspectral surface and cloud bi-directional reflectivity observations in the Arctic using a commercial, digital camera" *by* A. Ehrlich et al.

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The comments of the reviewer have been helpful to improve the manuscript. Especially the advice to discuss additional literature did help to revise the introduction of the topic. We thank the reviewer for addressing potential problems with regard to the measurements. They have been investigated and strengthened our confidence in the camera measurements. The detailed replies on the reviewers comments are given below.

The reviewers comments are given italicized while our replies are written in roman letters. Citations from the revised manuscript are given as indented text.

Detailed Replies

1) A discussion about the interest of such instrumentation comparing to classical radiometer is clearly missing and would be worthy with the advantages (price) and drawbacks (distorsion, saturation, polarization effects..) of such system.

Thanks for this advice. We did not deeply discus these issues in the original manuscript. In the revised version we added the following sentences to the introduction:

> Compared to scanning instruments, digital cameras instantly obtain a full scene of measurements without the need of high-precision movable components. The camera is easy to mount on an aircraft and relatively cheap. The high spatial resolution of the camera allows to measure with an angular resolution of about $0.1°$. However, due to the imaging system including lens and sensor, a careful calibration of the camera is required to quantify the angular dependence of the camera sensitivity which might be affected by dark noise, saturation, distortion, or polarization effects.

2) The first main problem, in this paper concerns, scientific references, which are missing or not well used. Lot of main works concerning other spatial or airborne multi-angular measurement such as POLDER, air-MISR or RSP and their exploitations are missing: For example, among others. Descloitres, J., J. C. Buriez, F. Parol, and Y. Fouquart (1998), POLDER observations of cloud bidirectional reflectances compared to a plane-parallel model using the International Satellite Cloud Climatology Project cloud phase functions, J. Geophys. Res., 103(D10), 11,411–11,418, doi:10.1029/98JD00592. Ovtchinnikov, M and Marchand R.T, Cloud model evaluation using radiometric measurements from the airborne multiangle imaging spectroradiometer (AirMISR), Remote Sensing of Environment, Volume 107, Issues 1-2, 15

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March 2007, Pages 185-193, ISSN 0034-4257, 10.1016/j.rse.2006.05.024.

Many thanks to the reviewer for giving these references. Unfortunately we did not know them before. In the introduction we added a section giving reference to these studies which are closely related to our study.

> Spaceborne multi-angular observations are obtained by instruments such as the Polarization and Directionality of the Earth's Reflectances instrument (POLDER, Descloitres et al., 1998) and the Multiangle Imaging SpectroRadiometer (MISR, Ovtchinnikov and Marchand, 2007). While POLDER provides a full image in $\pm 43^{\circ}$ along track and $\pm 51^{\circ}$ across track, MISR uses nine separate line cameras to cover nine different viewing angles. Using the airborne version of POLDER, Descloitres et al. (1998) compared the measured cloud $HDRF$ (without atmospheric correction) to plane-parallel radiative transfer simulations assuming spherical cloud particles. Differences ranged between 2 % for liquid water clouds and 9 % for ice clouds, which indicates that the scattering phase function of the cloud particles is essential for calculating $HDRF$. Assuming nonspherical ice crystals for the simulations, the differences are reduced to 2 %. With a similar approach, Ovtchinnikov and Marchand (2007) compared the radiance of different view angles measured by the airborne version of MISR and three-dimensional radiative transfer simulations. Differences appeared mainly in the nadir direction and are suggested to result from differences in the three-dimensional structure between observed and simulated clouds.

In the discussion on the averaging of the images, we added the following sentences:

> For airborne measurement with POLDER, Descloitres et al. (1998) showed that after averaging a sequence of cloud observations, the scene acts like a

plane-parallel cloud. The averaging approach assumes that the temporal cloud variability observed by each pixel in a sequence of images is similar to the spatial variability of one single image. Descloitres et al. (1998) found that about 10 images are required to sufficiently reduce the spatial variability for the observed cloud cases.

3) In the introduction, the part concerning the cloud BRDF need to be worked again because it is not clear and some references are not well used. Plane parallel model to derive cloud property is imperfect and certainly not sufficient but so far, given the diversity and complexity of cloud and the computational time of 3D calculations, it exists no other solution to have operational product such as optical thickness and TOA albedo is the PP model is used, which is not always the case. TOA albedo can indeed also be derived from angular distribution model. See for example, Loeb, N. G., S. Kato, K. Loukachine, and N. Manalo-Smith (2005), Angular distribution models for top-of-atmosphere radiative flux estimation from the Clouds and the Earth's Radiant Energy System instrument on the Terra satellite. Part I: Methodology, J. Atmos. Oceanic Technol., 22, 338– 351. Buriez, J.-C., F. Parol, C. Cornet, and M. Doutriaux-Boucher (2005), An improved derivation of the top-of-atmosphere albedo from POLDER/ADEOS-2: Narrowband albedos, J. Geophys. Res., 11 0 , D05202, doi:10.1029/2004JD005243. Sun, W., N. G. Loeb,R. Davies, K. Loukachine, and W. F. Miller (2006), Comparison of MISR and CERES top-of-atmosphere albedo, Geophys. Res. Lett.,33, L23810, doi:10.1029/2006GL027958.

Again thanks for the references. We agree that the discussion on how TOA albedo is derived from satellite sensors was not sufficient and not well written. We included the literature indicated by the reviewer and change this part of the introduction accordingly.

To estimate the impact of clouds on the Earth's energy budget from spaceborne measurements the $BRDF$ of clouds is required. Satellite instruments

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primarily measure spectral radiance/reflectivity and mostly do not cover the entire hemisphere. However, the energy budget is calculated by hemispheric irradiance/top-of-atmosphere (TOA) albedo. To convert the satellite observations of reflectivity into TOA albedo, the cloud $BRDF$ has to be known in terms of an angular distribution model (ADM Loeb et al., 2000, 2005). From multiangular instruments such as the Clouds and Earth's Radiant Energy System (CERES, Loeb et al., 2005) and the Polarization and Directionality of the Earth's Reflectances instrument (POLDER, Loeb et al., 2000) empirical ADMs are derived from 24 and 5 months of observations, respectively. A different approach utilizing radiative transfer simulations is applied by Buriez et al. (2005) to measurements by POLDER. Plane-parallel radiative transfer calculations of the cloud $BRDF$ for different cloud properties are used to convert the observations into TOA albedo.

However, for inhomogeneous clouds plane-parallel radiative transfer calculations are not sufficient to simulate the angular reflectivity above clouds (e.g., Loeb and Davies, 1997; Varnai and Marshak, 2007). Analyzing observations of the Earth Radiation Budget Satellite (ERBS), Loeb and Davies (1997) found that plane-parallel simulations underestimate the reflectivity in the backscattering direction. Varnai and Marshak (2007) observed a bias in the cloud optical thickness retrieved by MODIS which depends on the viewing angle of the sensor and cloud inhomogeneity. Both effects are significant for viewing angles of about 60° and larger. Three-dimensional models may improve cloud $BRDF$ simulations. However, given the diversity and complexity of clouds and the computational time of three-dimensional calculations, there exists no other solution than using plane-parallel models for an operative product such as optical thickness and TOA albedo. These problems show that there is a need for measurements of the directional reflectivity above clouds.

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4) section 5.1: I'm surprised that the authors does not succeed to reproduce the open water signature. The simulation done by the authors overestimate the sun-glint observed values whatever the wind speed. There is not really explanation in the text, but I think that this difference may illustrate the limitations of the use of the commercial camera. Indeed, The authors use the cox and Munk model, which is well validate to simulate open water angular signature. A higher glitter peak is obtained compared to observations, it could results of a saturation effects of the camera or because of polarized light, which is important in this specific direction. Discussion is needed.

We are aware that the comparison failed for the sun glint. Unfortunately, we still could not find any satisfying explanation for the differences. A detailed discussion on possible reasons affecting the camera measurement is given in the revised manuscript. Thanks to the reviewer for issuing saturation and polarization effects. However, those effects do not significantly increase the uncertainty of the camera measurements. Saturation of the camera sensor can not explain the differences as the camera sensor was not saturated. In the camera settings a low exposure time with 1/2656 s was chosen which was optimized for bright scenes like clouds and sea ice. The water scene was much darker. The radiance reflected in direction of the sun glint is still lower than the radiance reflected by the sea ice. We checked the raw data of the images and found maximum digital counts of about 12 000 for images above water and about 25 000 counts for sea ice. Thus the sensor was far from being saturated during the measurements above water. We add the following discussion to Section 5.1:

> The images were also checked for saturation. With a low exposure time (1/2656 s) which was adjusted to the bright scenes of clouds and sea ice, no saturation was evident in the data. The raw data of the images showed maximum digital counts of about 12 000 in the sun glint and about 25 000 counts for sea ice with a saturation value of 65536 (16 bit).

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Also polarization effects do not effect the camera measurements in a way that it might explain the differences in the sun glint. We checked the camera for possible differences in the sensitivity to radiation with different orientation of linear polarization. Maximum differences between measurements of parallel and perpendicular radiation were estimated with about 3 %. This 3 % hold only for 100 % polarized radiation. For natural surfaces this effect is even reduced by the degree of polarization. We added this discussion in Section 3.3 and in Section 5.1:

> Polarized radiation (e.g., sun glint) might increase the uncertainty of the camera measurements if the camera lens acts like a polarization filter. The sensitivity to linear polarized radiation of different orientation was tested in the laboratory using a source of 100 % linear polarized radiation. Differences between measurements of parallel and perpendicular polarized radiation were found to be negligible for the center of the images. Toward the edge of the image this polarization effect slightly increased. Maximum effects were estimated to be 3 %. It has to be taken into account that for radiation which is not 100 % polarized this effect will be reduced by the degree of polarization.

> Radiation reflected at angles similar to the sun glint is partially polarized which may have affected the measurements for these scattering angles (Takashima, 1985). For the solar zenith angle (61°) and a scattering angle of about 60° where maximum differences show up between simulated and measured $HDRF(\vartheta)$, the degree of polarization may reach maximum values up to 0.9 (A. Hollstein, pers. comm.). However, the uncertainty of the camera due to polarization was estimated to be 3 % at maximum and cannot completely explain the differences between measurements and simulations.

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Minor corrections

Section 2 P24594, line 15-18: what is the reference? P24595, line 27, if exists, reference for the SORPIC campaign?

For the SORPIC campaign no reference exists as this is the first report on data from SORPIC. On P24594, line 15-18 the reference was given the sentence before. As this was misinterpreted, we changed the sentence to:

> However, instead of calibrated radiance, Long et al. (2006) and Schade et al. (2009) used the radiance-uncalibrated signals of the camera sensor to detect clouds by analyzing the three spectral channels (red, green, blue; RGB) of the CCD (charged coupled device) sensor.

Section 3 P24597, line 16: as it is used for validation a reference is needed Reference for the Smart-albedometer P24600: In the definition of the scattering angle (which could be numbered as others equations). it seems that the expression is not exact. in the second line of the expression, I would add .

References for the SMART-albedometer have been added at P24595 line 17 and P24602 line 2:

> The SMART-Albedometer was described by Wendisch et al. (2001) and Ehrlich et al. (2008).

> The accuracy of the calibration has been verified by comparing the nadir radiance of the camera to spectral measurements of the SMART-Albedometer which has an uncertainty of 6 % for radiance measurements (Ehrlich et al., 2008).

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The equation for the scattering angle has been corrected. There must have been a mistake during proof-reading. Thanks for that hint.

Section 4 p24603, line 25: Lambertian instead of Lambertain. Section 4.3: The number of averaging needed to obtain a smooth HDRF is interesting but limited to this case. Indeed, this number depends on the cloud homogeneity and also on the cloud altitude variation, which can lead to a stereo shift. Mentioned it in the text.

Thanks for finding this typo. The misspelling of Lambertian has been corrected. Regarding the number of averaging, we agree with the reviewer that the non generality of these numbers was not pointed out clearly. We added the following sentences at P24606 line 1:

> The number of 50 images is limited to this single case study only and may significantly differ for clouds with stronger inhomogeneity and observation at different altitude. A stronger inhomogeneity would require more images to be averaged. On the other hand, images taken close to cloud top (not shown here) indicated the glory and cloudbow already in one single image.

In the conclusions P24613 line 15 we also included a statement on potential stereo effects due to alternating cloud top altitudes.

> Additionally, stereo effects for inhomogeneous clouds with varying cloud top hight may broaden the cloudbow.

Fig.6: For information, indication of the solar incidence angles could also be mentioned in the legend.

The solar zenith angles have been given in Table 1. For convenience of the reader, we added them in the caption of Figure 6.

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Section 5.1 - In figures 9(c,d,e), sunglint simulation present a high anisotropy, so I find that the simulation over the entire section is not very appropriate and bring nothing to the discussion. I would advice to delete them. - Page 24609, line 12. There is an error in the scattering angle value 12° and 80°. 12° is outside the angles plotted in the figure.

We agree with the reviewer and removed the simulations for the entire section in all plots. The scattering angle 12° was wrong and is changed now to 120°. Thanks for having noticed.

Section 5.2: - The authors used the Nakajima and King model to retrieved optical thickness and effective radius. However, this method is based on the use on a nearinfrared wavelength to retrieve the effective. This information being not available in the camera channels, the effective radius obtained with this method is thus not very informative and should be deleted. - Again, in Figure 10. I find that the simulation for all the section does not bring something interesting to the paper.

Cloud optical thickness and effective radius have been retrieved using data of the SMART-Albedometer. No camera data was used. The SMART-albedometer covers the wavelength range required for the Nakajima and King method and provides reliable retrieval data. Thus we think that the retrieved cloud properties are worth to be mentioned as they help to describe the observed cloud and to set up the radiative transfer model. Therefore, we did not delete this information from the manuscript. In Figure 10 the simulations showing all the section are removed in the revised manuscript.

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