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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.

Anonymous Referee #1

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This paper presents airborne radiative measurements obtained from a commercial digital camera. After camera calibration, measured bi-directional reflection functions are presented for sea ice, open water and clouds and resulst agree with others measurements cited. Simulations are next done for open water and clouds HDRF and compared to the measurement. The paper presents a new instrument certainly cheaper than classical airborne radiometer and the results obtained seems to be coherent. However, as I mention it below, some references to others airborne radiometers are missing as well as a discussion about the interest and the disadvantages of the presented system. So I recommend publication but after the major corrections suggested below:

Major corrections:

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.

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

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.

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.

Minor corrections:

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

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 .

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.

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

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.

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 near-infrared 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.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 24591, 2011.