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Atmospheric Measurement Techniques Discussions

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Interactive comment

Interactive comment on "Shortwave Radiative Effect of Arctic Low-Level Clouds: Evaluation of Imagery-Derived Irradiance with Aircraft Observations" *by* Hong Chen et al.

Anonymous Referee #1

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General comments

This paper describes an analysis of solar broadband and spectral irradiance data from an airborne measurement campaign in the Arctic in September 2014. Comparisons were made with radiative transfer model calculations (RTM) that take into account concurrent cloud products from MODIS satellite observations and retrievals of local spectral surface albedos from the aircraft measurements. A significant fraction of the work is concerned with the determination of the spectral ground albedos taking into account measured upwelling and downwelling irradiances, video images from the ground and literature data in spectral ranges where measurements were not feasible. The effort is justified because under the Arctic conditions, accurate ground albedos are crucial to distinguish cloud radiative effects from the influence of the relatively bright snow- or ice covered surface.

In general, the agreement of measurements and RTM calculations is satisfactory for both broadband and spectral measurement (at two selected wavelengths). However, significant differences were observed during some periods where no clouds were present according to the satellite data. This is taken as a hint towards the presence of undetected, optically thin clouds. However, in my view this interpretation is not as clear as currently presented in the paper and the influence of experimental uncertainties requires more attention. Nevertheless, the paper is well written, structured and documented. It should be published after minor revision.

Specific comments

Page 2, line 10: Clarify "radiative transfer model" instead of "model"

Page 2, line 15: "...about 22% of clouds remained undetected (cloud optical thickness less than 0.5)." This statement is not in agreement with the main text, see page 11, line 36.

Page 3, line 14: " ...every 10^6 km^2 decrease... a 2.5 W m^{-2} increase". The statement is unclear. For which area does this apply?

Page 4, line 36: Give more information on the flight area, altitudes and times e.g. in the text, in a table or a modified Fig. 1.

Page 5, line 16: The broadband instruments were probably not actively aligned. That should be made clear. Were data excluded from the analysis when the aircraft attitude was not horizontal?

Page 5, line 25: "... that keeps the zenith light collector horizontal..." I assume that both collectors were actively aligned. If so, that should be clearly stated because for the determination of net irradiances this is certainly important.

Page 5, line 28: "… that cannot be corrected." Another reason why low sun elevations are more challenging for the downwelling direct irradiance is that the gradient d cos(SZA) / (d SZA) increases with SZA.

Page 6, line 9: "… atmospheric effects". You probably mean "collector effects"?

Page 6, line 25: I am convinced that the complicated collector-specific corrections and calibrations were done thoroughly. But can you estimate the remaining uncertainties for the upwelling and downwelling measurements, both broadband and spectral? For example in Appendix B, Fig. 13 the scatter in the ratios indicates uncertainties of the independently calibrated instruments that will not vanish by applying the azimuth correction. I assume Fig. 13 shows downwelling irradiances? If so it would be interesting to see a similar plot for the upwelling (with similar scatter but no azimuth dependence).

Page 6, line 36, Eq. 1: The different weighing implies a conversion to "brightness" or "luma" rather than a (relative) physical radiance. The source of the three coefficients should be cited and what they represent.

Page 7, line 15: "…robust estimates". I wonder what robust means. The procedure is quite complicated and there are obviously several sources of uncertainties: (1) the coefficients in Eq. 1, (2) the blending technique Eq. 2, (3) the adaptive thresholding (parameters given in Appendix C) and (4) the (presumably) limited field of view of the camera compared to the irradiance collector. For example, does it make a significant difference if 0.333 is inserted in Eq. (1) for R, G, and B? It would be convincing if you could provide an uncertainty estimate for the snow fraction based on a sensitivity study taking into account the different aspects (1)-(4).

Page 8, lines 12 and 13: Exchange hygrometer and thermometer in 3) and 4)

Page 8, equation (4): Why not define "$\alpha_{\text{SSFR}} =$" here?

Page 9, line 6: I assume the atmospheric correction was comparatively small because the altitude was below 300 m? You should give the reader a rough idea.

Page 9, line 33: To support this statement you could include the (much lower) albedo of the open ocean in Fig. 5. Moreover, in the introduction you mention a climatological surface albedo for the region by Moody et al., 2007 that is used for the MODIS cloud retrievals. It would be interesting to see how this compares with the locally measured data.

Page 10, line 9: Why 70%? In Fig. 4 a majority of data points is well above 80%.

Page 10, line 21: "cloud optical thickness"

Page 10, line 21: "...agree with measurements within 10%" Does this statement refer to data in Fig. 7b or to other flight periods? In Fig. 7b agreement within 10% under no-cloud conditions is only visible at the very beginning. My impression is that good agreement during periods where no clouds were detected could be reached by increasing the snow fraction to about 80% without significantly affecting the upwelling in the presence of clouds. So the 70% snow fraction assumption and its uncertainty are crucial here and should be discussed.

Page 10, line 23: "... below 0.5" In Fig. 7b it seems that the missing about 30 W m^{-2} upwelling RTM irradiance (above the clear baseline) correspond to a missing COT of about 2 which is well above the threshold of 0.5. Moreover, if the COT were indeed around 2 no ice-structures on the ground would be visible in the photographs (i)-(iv).

Page 10, line 23: The "... continuous variation from leg to leg..." Compared to the COT data the measured peaks in the upwelling irradiance look smoothed out. I wonder if this has to do with the fields of view of the instruments which at 7 km altitude are much greater than that of the satellite (1 km). So even if you fly above a correctly detected cloud gap, the sensors could receive irradiance from surrounding, even distant cloud fields. So, overall wonder if the results in Fig. 7 can be explained without "undetected clouds", i.e. by field of view effects and an underestimated albedo.

Page 11, line 3: "... via the snow fraction" What was the mean snow fraction during that flight?

Page 11, line 7: "...except...22:22:48" I cannot find that period in the figure.

Page 11, line 11: "... no evidence of any cloud gap". This also hints towards different fields of view of satellite and aircraft. Even if you fly underneath a cloud gap, at estimated solar zenith angles of around 70 deg no direct radiation (and a corresponding increase of irradiance) may be present. It may be detected at other places (also underneath a cloud) but the chances for that are low if the cloud gap is small and cloud thickness is substantial. It would be helpful to estimate the size of the cloud gaps detected by the satellite. Moreover, a statement should be included whether or not the cloud top and bottom heights were correctly retrieved from the satellite data on both days.

Page 11, line 15: Table 1 and 2 contain a main result of this study that is not adequately discussed here and should clearly enter the abstract and conclusions: Under conditions when clouds are detected the numbers in the first three columns agree, at least (I assume) within experimental uncertainties which need to be specified.

Page 11, line 23: Do September 11 and 13 belong to the Arctic dry or wet season?

Page 11, line 36: "Of all pixels along ... 22%" To determine the fraction of undetected clouds as stated in the abstract, green/(cloudy +green) should be calculated.

Page 11, line 37: "... (highlighted in green) are actually cloudy." Again the question arises if during the green periods clouds were undetected by the satellite or radiation was captured unintentionally by the irradiance sensors at some distance from the aircraft.

Page 12, line 19: "... surface albedo ... is biased low by about 9%". The 9% come from the albedo + atmospheric correction which is probably significant at 7 km altitude.

Page 12, line 24: "Simply changing the snow fraction does not improve the agreement..." I am not so sure. Increasing the snow fraction will lift the RTM irradiances for shorter wavelengths more strongly than for the long wavelengths (Fig. 4).

Page 13, line 31: “Undetected thin clouds (COT<0.5) led to a high bias. . ..” I don’t think this statement is justified. Looking at Fig. 8 the measured downward irradiances vary around 200 W m^{-2} and COT vary around 6 (where detected) with consistent RTM results. I assume a COT of 0.5 would produce downward irradiances well below the clear ones but also significantly greater than measured. So the measurements are inconsistent with the presence of thin clouds which makes an explanation as given above (page 11, line 11) more likely.

Page 13, line 32: “. . . above clouds . . .” should be “. . .below clouds. . .” See Fig. 8 and page 11, line 13.

Table 1 and Table 2: Please indicate in the captions place, time and altitude. Specify above or below cloud conditions and the unit of the numbers. Explain what is listed in the last column called “RTM” The precision of the numbers implies an accuracy that is unrealistic. I assume that the numbers in the first three columns agree within experimental uncertainties but that needs to be specified. Please state the average COT, cloud top and bottom heights.

Fig. 1: Specify the maps’ latitude and longitude ranges, e.g. in the caption. A costal line can be vaguely recognized showing that the flight area was west of Banks Island but I assume hardly any reader is familiar with the area.

Fig. 2: 80.70% implies a precision that is certainly not justified by the method (see comment in text). Indicate the field of view of the circular areas in (a), the flight altitude, time and location. The y-axis label should read “spectral flux density” or better “spectral irradiance” in accordance with the main text.

Fig. 3: Approximate times and locations should be specified in the caption. The indicated flight levels are confusing here without additional information.

Fig. 4: Given the spectral resolutions, the indicated wavelengths are too precise. 640,

1240 and 1630 nm, as stated in the text, are appropriate.

Fig. 5: Consider including open ocean albedo and data by Moody et al., 2007 (see comment in text).

Fig. 7 and Fig. 8: Indicate the flight altitudes in captions and maybe the cruise speeds so that the size of the cloud gaps can be inferred.

Fig. 13: Please indicate that these are ratios of downwelling irradiances (I assume).

Appendix B, Fig. 13: Were the solar zenith angles during this flight comparable to those during the other flights? The azimuth dependence may change with solar zenith angle for geometrical reasons and because of a varying (wavelength dependent) contribution of direct irradiance.

Appendix C: In order to understand the meaning of the factor d=1501 the images' total pixel dimension should be stated.

Technical corrections

Page 4, line 13: Introduce "CRE" as "cloud radiative effects"

Page 9, line 23: "2014-09-13"

Page 11, line 31: Fig. 7?

Page 11, line 37: Fig. 7?

Fig. 5: Wet and dry season colours are hard to distinguish. Second citation should be Brandt et al.

Fig. 7 and Fig. 8: y-axis: broadband irradiance. Typo "MOODIS" in Fig. 8

Fig. 9 and Fig. 10: y-axis: spectral irradiance

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