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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spectral albedo of arctic snow during intensive melt period" *by* O. Meinander et al.

Anonymous Referee #1

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General comments

The manuscript by Meinander reports on measurements of spectral albedo in northern Finland. In my opinion, the experiment and the analysis of data collected are seriously flawed. The size of the snow patch used for the measurements was far too small (it extended only 3 m from the radiometer in one direction); the duration of the campaign (4 days) was far too short to determine whether results are representative for the study area or a larger area; spectroradiometric measurements are not consistent with broadband measurements taken only 20 m away from the primary site of study; and changes in albedo were attributed to the solar zenith angle even though the main driver was melting snow. Based on the data presented, I also don't agree with the author's assessment that meltwater moved into deeper layers over the day. Some re-

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sults of the paper are correct, for example the observation that half-melted snow has a lower albedo than fresher snow. However, this observation has been published already some time ago [e.g. M. Blumthaler, W. Ambach, Solar UVB-Albedo of various Surfaces, Photochem. Photobiol., 48(1), 85-88, 1988]. I therefore don't see how the authors' results can be useful to other researchers or help to improve climate models. Based on these general remarks and my more specific comments below I recommend that the manuscript should not be published in ACP.

Specific comments

P 27076 L13-15: The sentence is not clear. What was the value of the "regional albedo" used in the RT model? Was it the value of the locally measured albedo or something else? Was the regional albedo assumed to be constant or was it varying with wavelength?

P27078 L7: The SNORTEX-2009 campaign was very short, lasting from 20-25 April. The short duration is problematic as is not clear whether the results are representative for the site. It is also not clear whether the results can be applied to a larger region or are specific to the measurement location only. These limitations greatly reduce the value of the paper.

P27078 L16: Please provide more details on how the quartz fiber was split and the functionality of the "internal switch".

The setup uses two identical entrance optics. Ideally, albedo measurements should not depend on which of the two collectors is facing upward. Was the set up turned around during some time of the experiment? Confidence in the results could be strengthened if albedo measurements are identical regardless of the choice of the collector that is on top.

P27080 L13: How is "regional Lambertian albedo" defined? What albedo value was used in the model? Was it a constant value or was albedo varying with wavelength?

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In what way was the locally measured albedo used to determine the regional albedo? Line 23 indicates that regional albedo was based on the measured albedo minimum and maximum, A_{min} and A_{max}. How were these two values combined to calculate regional albedo? Was it the average or something else? I assume that there are trees, buildings, roads, etc within the radius of the measurement site relevant for regional albedo. How did these features influence the choice of the "regional" albedo value used in the model?

P27081 L4: Define "UVI" (It may not be clear to all readers that UVI means UV Index).

P27081 L9: In my opinion, the size of the patch of snow underneath the sensor was far too small for reliable albedo measurements. According to the text, the surface was free of snow at a distance of more than 3 meters towards the North. This translates to a nadir angle of 50°. I even consider 10 m (distance from the sensor in South, West, and East direction) not sufficient (Nadir angle 76°). Albedo was likely lower at the border of the snow patch, with effects on the measurement.

The authors report that the albedo was decreasing during the course of the campaign. Since the patch of snow was so small, I hypothesize that some of the change was not caused by the change of the albedo directly underneath the sensor but by shrinking of the snow patch. This possibility should be discussed by the authors.

P27080 L22: Please provide a reference for these "semi-empirical equations."

P27081 L18: Why wasn't the "2 min time step" used throughout? During 6 minutes, the solar zenith angle changes appreciably, with effects on radiation levels also during clear skies, in particular at short wavelengths.

P27082 L1+: Is it sensible to parameterize albedo as a function of SZA when the dominant driver of albedo change is not SZA but the melting of snow? Also, Figure 2 shows that that albedo is declining on every day as a function of time. In the morning, SZA is decreasing with time and in the afternoon SZA is increasing with time. The

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difference (SZA-SZA_min) of Eq. (1) is therefore always positive. In the morning, A is larger than A_midday, so the coefficient c should be positive, not negative (c = -0.0024).

In very cold environments, where the snow does not melt or change its morphology during the course of a day, it is conceivable that albedo depends on SZA, which means that surface reflectance is not isotropic (i.e. the albedo is not Lambertian). With the data presented by the author, I think it is not possible to decouple changes in albedo due to non-Lambertian reflectance from changes in albedo caused by melting snow.

P27082 L7: Given albedo with a precision of 6 digits is unreasonable considering the measurement uncertainty. Also, according to Eq. (1) , c = [A(06:00) - A(10:00)] / [68.9-55.38)]. According to Figure 2, A(06:00) is larger than A(10:00)], not smaller as in printed in the equation in line 8.

P27082 Eq. (3): Rˆ2 is only 0.6, indicating that a linear model is not a good method to calculate albedo in the visible from albedo at 310 nm.

P27082 L22+: How small was the "other smaller field close by?" How large was the patch of snow on that field?

Figures 3 and 4: The product of the solar spectrum and the responsivity of an instrument measuring erythemal irradiance, such as SL501, peaks at about 313 nm. The albedo measured with the SL501 should therefore match the albedo measured at ∼310 - 320 nm with the Bentham spectroradiometer. On 22 April, the albedo at 313 nm measured with the Bentham was about 0.55 (Figure 3). On the same day, the albedo measured with the SL501 was about 0.45 (Figure 4). What is the reason for this discrepancy? Is it perhaps the different sizes of the snow patches at the two locations? This discrepancy further deteriorates my confidence in the albedo measurements presented by the authors.

P27083 L7: I cannot discern from Figure 5 that "after several hours of accumulation, the

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water moved into deeper layers". To me, Figure 5 only snows that there is a tendency of increase in liquid water content as function of time, regardless of depth. For example, when the first measurement was taken (at about 7:00), the highest water content was at the deepest depth (24 cm), contrary to the statement of the authors.

P27083 L10: Snow grain sizes changed from 0.25 mm to up to several millimeters. According to Section 2.3., grain size was estimated visually with a mm-grid. I think it is not possible to accurately determine a grain size of 0.25 mm visually with a mm-grid, even with a magnifying glass. How was it really done?

P27083 L21: I think there is some confusion on the meaning of "Lambertian." Lambertian means that the radiance of the snow surface is isotropic, i.e. independent of the direction. A Lambertian albedo can still depend on wavelength. So it seems that both types of calculations performed assumed Lambertian albedo. The albedo used for the first type was independent of wavelength, while that used for the second type was not.

P27084 L11: As already stated above, SZA is not the driver of the albedo change observed over any given day. With the author's experiment, changes in albedo due to the non-Lambertian nature of the snow reflectance cannot be decoupled from changes caused by melting snow.

P27084 L13-22: The SZA-dependence of albedo in Antarctica has a different cause than the apparent change of albedo with SZA at the study site in Finland. Without having read the paper by Pirazzini, I assume that changes in albedo with SZA observed in Antarctica are related to the non-Lambertian character of snow reflectance. If a snow surface does not have any preferred orientation (e.g. no structures formed by the prevailing wind such as sastrugi), changes in albedo should only depend on SZA, not on the solar azimuth angle, and should therefore be identical in the morning and afternoon. That is not the case at the Finland site.

P27085 L20+: The relationship of albedo at UV-B, UV-A and visible wavelengths established by the authors (Eqs. (2) and (3)) is likely not universal. So I don't see how these

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relationships can help to "achieve as accurate albedo estimates as possible" (L20), which could be used as input to climate models.

Typos

P 27076 L22: Change "have effect" to have an effect"

P 27076 L12: Delete "monochromator" after "150 mm"

P27079 L2: . . .showing deviations of less than +/-1% for the entire spectral range based on measurements of the three lamps used for the calibration procedure.

P27079 L6: Replace "above another field at ∼20 m distance" with "which were located 20 m away from the site of the spectroradiometric measurements."

P27080 L14: Delete "at the wavelengths of UVB (280-310)" The SL501 resembles the action spectrum from erythema, which has also a contribution from the UV-A.

P27080 L15+: . . .in the morning and afternoon would indicate changes in radiative forcing caused by changes in albedo due to melting snow. Only relative changes were considered when comparing the measurements with the model. . .

P27080 L19: Change "Absolute calibration scale" to " The absolute . . ."

P27082 L9: Change "at one time" to "at any given time"

P27083 L24: Change "for the both" to "for both"

P27084 L2: Change "was to appear" to " was observed when A_max was used for the model calculations"

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 27075, 2010.