

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

**Anonymous Referee #3**

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Referee report on the manuscript "Spectral albedo of arctic snow during intensive melt period" by Meinander et al.

### General comments

The paper studies the reflectance of a snow surface in spring and the effects of the reflectance on the radiative modelling of the atmosphere, and therefore fits into the scope of ACP.

The paper presents interesting new albedo measurement data from an intensively melting snow field. However the research relies heavily on existing ideas and methodologies (snow melt effects in the tundra: Weller, G., 1972. The tundra microclimate during snow-melt at Barrow, Alaska, Arctic, Vol 25,no. 4. Albedo of melting snow: Winther,

C11726

J.-G., 1993. Short- and longterm variability of snow albedo, Nordic Hydrology, 24, 199-212.). The main novel aspects are in the fact that the measurements are from a boreal region in Sodankylä. If the authors revise their manuscript with this in mind, and concentrate on the quality of the measurements and interpretations, the results would be very valuable in interpreting previous albedo results from the same region.

The albedo measurement methodology is outlined in great detail, and rightly so, because the albedo needs to be measured accurately for it to be of any significance, especially in the presence of multiple sources of error. The authors have done a good job of calibrating their instruments and describing their usage of them. The description of the ancillary measurement methods and results however is lacking, which is unfortunate because they could be used to explain the observed albedo levels.

The conclusion that Arctic and Antarctic albedos follow a similar diurnal pattern cannot really be drawn from this study because the results were not really obtained from the Arctic, but from a boreal forest area. The results support the conclusions only up to a site specific level. They cannot be generalized to an Arctic level simply because the measurements cannot have been said to be made in the Arctic except in the general sense that Sodankylä is north of the Arctic Circle. The title should be changed to reflect this. A more precise description of the measurement area is lacking and a photograph or schematic illustration of it and the surrounding snow field would be useful.

The language used in the manuscript is reasonable but it should be sent to be checked by someone fluent in English. It would help if the variables would be defined using equations at the start of the paper. The text needs to be clarified, especially regarding the methodology and conclusions. The conclusions should be indicated in a separate section either at the end of the discussion or at the end of the paper. The abstract is concise and provides a good summary of the work done and the conclusions obtained.

The authors seem to have a good sense of the work done in the snow-albedo field in the previous years. They are therefore in a position to indicate where their own work

C11727

fits in. However, the place of these measurements within the framework of albedo research is not explained, nor is the new contribution indicated. It would seem that the largest value of this paper comes from producing new measurement results from an intensively melting snow field.

#### Detailed comments

Title: The authors should consider whether their research has really been made in the Arctic, because it seems that it has been made in a boreal forest area. The title should be changed to reflect this if necessary.

Page 27076 line 25. The authors state that "the topography may affect the measured albedo despite a flat measurement area". They could state more clearly what they mean by this because it is unclear in its present state. Do they mean that the surface features of the snow affect the albedo?

Page 27076 line 26. It is not clear to this reviewer how the bacteria, or the mentioned chemical reactions affect the albedo of the snow. The authors could perhaps indicate the optical significance of these substances in the snow. Since bacteria are mentioned in the respect, then algae should maybe also be mentioned.

Page 27077 line 10. The authors could consider whether they have actually made measurements of Arctic snow, or measurements in a boreal forest. A photograph of the study area would help in this respect.

Page 27078 line 9. Why was the radiometer placed at a height of 2.5m? Some kind of explanation is required for this, especially because the authors state that the standard height for albedo measurements is 1-2m (page 27076 line 20).

Page 27079 line 14. If the action spectrum of the SL501 is not linear, then a few words regarding the effect this has on the measured albedo should be included. Can the instrument even be used to measure reflected radiation which can have a different spectrum from the incident solar radiation?

C11728

Page 27079 lines 17->. The description of the ancillary measurements is not as detailed as the description of the radiation measurements. It would for example be necessary to know what the limits and accuracies of the SnowFork instrument are, because wetness is crucial to the conclusions in the paper. Also the grain size and shape affect the albedo to a great extent, but grain shape is not reported at all. Therefore more effort needs to put in describing how the ancillary measurements were made, and the results obtained should be reported in more detail.

Section 2.4 This section is unclear. It seems that the model was run with a constant value for albedo, the regional Lambertian albedo, which was taken from the measurements, and then the temporally varying spectral albedo, which was also taken from the measurements. The model produces irradiance values which are then compared. The different model runs should be clearer, perhaps with the aid of a table listing the different input variables and their values. The research question should be put into the introduction.

Page 27081 line 7->. The description of the surface below the instruments should be put into a section describing the measurement environment. A photograph, or schematic illustration, of the measurement setup would help in understanding the surroundings. The authors describe the extent of the snow cover but do not indicate what lies beyond the snow. Does the forest start there or is the ground bare? Apparently part of the sky is blocked by trees, but the authors do not mention the effect this has on the incoming irradiance. The direct irradiance is not blocked, but what about the diffuse irradiance? The diffuse irradiance is very important in the wavelengths relevant to this study.

Page 27081 line 8. What is meant by "moving clouds"? The amount of clouds should be indicated in octas, and the type of cloudcover should also be mentioned if that data is available.

Page 27081 line 25. The speculation regarding the frost in the night time could be

C11729

avoided by producing data for night time air temperatures.

Page 27082 line 9. This reviewer does not understand what is meant by the statement "Independently from the temporal decrease of albedo, the snow albedo at one time increased as a function of wavelength"? By studying the results of eg. Dozier et al. 1988 (The Spectral Bidirectional Reflectance of Snow, Spectral Signatures of Objects in Remote Sensing, Proceedings of the conference held 18-22 January, 1988 in Aussois (Modane), France. Edited by T.D. Guyenne and J.J. Hunt. ESA SP-287. European Space Agency, 1988., p.87) it can be seen that the albedo increase in these wavelengths has been known for decades.

Page 27082 line 10->. The number of measurements these regressions are based on should be indicated. The importance of these regressions eludes this reviewer however.

Page 27082 line 22. Broadband albedo results are reported here for "another smaller open field". The similarities and differences between this and the main measurement site should be reported as well. Was the main measurement site also a "small open field"? A photograph of the area would help here.

Page 27083 line 15. Here are presented some results from an automatic snow depth measurement system. It would be useful if this was presented already in the methodologies section and a time series of snow depths could be reported in the results section. It seems that it would show that the snow was in fact melting quite fast.

Page 27084 line 5. The authors do not show the data that confirm their results. Maybe in this case it would be useful to include these results and maybe omit the spectral dependence section that just confirms results previously known.

Page 27084 line 12->. The authors report that they detected a SZA-asymmetric albedo due to the intensively melting snowcover, after which comes a comparison with Antarctic snowcovers. The authors fail to mention the distinguishing differences between

C11730

their site and the semi-infinite Antarctic snowcover. This leads to the question: is the comparison relevant?

Page 27085 line 5. The conclusion about the Arctic and Antarctic albedo declines being the same is a bit strong, because the measurement site is in a boreal forest.

Page 27085 line 18. A spectral albedo is always behind a broadband albedo because the broadband albedo is defined as an integral of the spectral albedo.

Page 27086 line 5. The authors do not report which dimension of the grain they use for the grain size, or what shape the grains were. It would be interesting to see a more detailed timeseries of grain size and shape, because they mostly define the albedo together with the wetness in the absence of impurities or pronounced surface features. Also even a qualitative description of the impurities in the snow would help in determining why the albedo values in the UV become so small.

Table 1. The authors do not introduce the "snowball test" which they use. It is assumed to be some kind of test for wetness, but it should be defined.

#### Technical corrections

Page 27076 line 4. The words "of water" should perhaps be added after the word "accumulation" to indicate that the accumulation of snow is not meant here.

Page 27076 line 22. The word "dirt" should perhaps be changed to the word "impurities" which is normally used to describe everything in the snow that is not ice.

Page 27076 line 24. The word "penumbral" was not known to this reviewer. Perhaps it could be substituted with the word "partial" which also describes the shadows meant in the text.

Page 27081 line 20. The word "procedures" should perhaps be changed to "processes" or a similar word.

Page 27085 line 15. The year seems to be missing from the reference to Feister and

C11731

Grewे.

Fig 5. The 0 and 24cm lines seem to be a very similar colour and the symbol is the same. One of them should be changed.

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

C11732