

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

Meinander O. et al.

First of all, we'd like to thank for receiving the valuable comments by the Referee#3 which have been used to improve the revised manuscript accordingly. Here we give our reply to the Referee's comments.

Before that, we would like to bring out the fact, that due to the interactive comment given by Grenfell and Warren (<http://www.atmos-chem-phys-discuss.net/10/C10240/2010/acpd-10-C10240-2010.pdf>) on our manuscript, and referring to our reply to their comment (<http://www.atmos-chem-phys-discuss.net/10/C10801/2010/acpd-10-C10801-2010.pdf>), we have prepared the revised manuscript to include results on element carbon (black carbon) and organic carbon in the snow in Sodankylä. These were not in the original manuscript. We have also included analysis on the origin of EC/OC in the snow (introducing new co-authors Rostislav Kouznetsov and Mikhail Sofiev), and the evaluation of the snow EC/OC analysis method and results (resulting in a new co-author Jonas Svensson, from the prof. Ström group, Sweden). Furthermore, we have included the new independent albedo results first presented in our reply to Grenfell and Warren (introducing a new co-author Aku Riihelä), and new radiative calculation results on the edge effect as presented in our first reply to Referee#1 (<http://www.atmos-chem-phys-discuss.net/10/C11474/2010/acpd-10-C11474-2010-supplement.pdf>) (introducing a new co-author Petri Räisänen). The revised version has been prepared to give answers to all the general and detailed comments given by Grenfell and Warren, and the three Anonymous Referees, as will be shown more in detail in our replies and in the revised manuscript. Due to all these valuable comments our revised manuscript has really been improved.

Referee's General Comments

/1/ “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, 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.”

Our reply:

We thank the Referee for this comment and have followed the instructions given by the Referee. The revised manuscript now i) includes the references by Weller (1972) and Winther (1993); ii) the place of measurements is defined to represent boreal region; and iii) the ancillary methods are described and used more detailed:

i) Introduction, second chapter:

“The measured albedo is determined by the target’s basic properties and the overall environment around the target. According to literature (Wiscombe and Warren, 1980; Warren and Wiscombe, 1980), and RT models (Flanner et al., 2007; Gardner and Sharp, 2010; Mayer and Kylling, 2005), the effective snow grain size; i.e. grain size and shape distributions, or specific surface area (Domine et al., 2007); is the most important factor to determine snow albedo. According to theory (Wiscombe and Warren, 1980), snow albedo decreases as the grain size increases, as a smaller effective radius increases the probability that an incident photon will scatter out of the snowpack (Gardner and Sharp, 2010). Other important snow properties include the liquid water content; the concentration of absorbing inorganic and organic impurities in the snow; as well as their vertical distribution in the snowpack together with snow depth and albedo of the underlying ground. During melt, snow undergoes a metamorphosis process that modifies the spectral albedo (e.g., Weller 1972). The liquid water content of snow increases, and wet snow has a lower albedo than dry snow (e.g., Blumthaler and Ambach, 1988). Also, as snow ages, with or without melting, the grain size increases and therefore albedo lowers (Wiscombe and Warren, 1980).”

Discussion:

“Winther (1993) has presented the progress of snow albedo for a Norwegian research site, where the albedo decreased as the snow went under a process of metamorphosis. Snow albedo was first determined as a function of temperature index alone. An improved accuracy of 2 - 6 % in estimated snow albedo was obtained when solar radiation was included. “

ii) Materials and methods:

“The snow albedo measurements were made at Sodankylä (67°22’N, 26°39’E), belonging to the northern boreal forest zone with the snow type of taiga. “

iii) The ancillary measurements are now described more detailed (whole new chapters in the revised manuscript were included, please see the revised re-organized manuscript for this) and are used to explain the observed albedo levels, e.g.:

Discussion:

“We have presented spectral and broadband UV and VIS albedo results on seasonal intensively melting snow at Sodankylä, beyond the Arctic Circle. Prior published measurements of albedo for clean snow in this spectral range are 0.97 - 0.98 (Figure 4 of Grenfell et al., 1994) and 0.98 - 0.99

(Figure 6 of Hudson et al., 2006); consistent with the extremely small absorption coefficient of ice in this spectral region (Wiscombe and Warren, 1980; Warren et al., 2006; Warren and Brandt, 2008). On the contrary, our albedo results reveal spectral albedo for UV and visible, at wavelengths of 300 - 560 nm, for SZA 55 - 70 degrees, and for clear sky and cloudy sky, to be in the range ~0.5 - 0.7. These low albedo results are supported by three simultaneous independent albedo measurement setups (one Bentham spectrometer, one SL-501 filter radiometer, one CM-14 albedometer) measuring during the same days at slightly different locations at Sodankylä, as well as by simulated albedo data using SNICAR-online (Flanner et al., 2007) with realistic large snow grains (3 mm diameter) and black carbon (87 ppb). We have also measured previously UV albedos of 0.5 - 0.7 for melting snow at Sodankylä (Meinander et al., 2008). Here a plausible explanation for our low albedo results is given and discussed. "... (whereafter the discussion follows)

We feel that the novel aspects of our manuscript are not only that our results represent boreal region in Sodankylä (as stated by the Referee), but also the contradiction to albedo values found in literature for clean snow. In our revised manuscript we also give new data of snow impurities for Sodankylä, beyond the Arctic Circle, for a comparison with the work presented by Doherty et al (2010) for Arctic Scandinavia.

/2/ 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

Our reply:

According to the Referee's suggestion, the word "arctic" will be removed from the title. The boreal was included in our Reply #1, item ii) here above. We suggest the following new title:

"Spectral Albedo of Seasonal Snow during Intensive Melt Period at Sodankylä, beyond the Arctic Circle"

/3/. 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.

Our reply:

We agree that a photograph or a schematic illustration could be useful, and suggest therefore a new Figure (Fig. 1) and table (Table 1) on the measurement areas to the revised manuscript.

/4/ The language used in the manuscript is reasonable but it should be sent to be checked by someone fluent in English.

Our reply:

We agree that our manuscript could benefit from a language check. We feel that our revised manuscript has been greatly improved from the original one, although it is not language checked. We naturally agree to have the language check, if the Referee finds it necessary.

/5/ It would help if the variables would be defined using equations at the start of the paper.

Our reply:

We have now included definitions with equations for the key albedo variables, e.g. erythemal albedo:

“To gain the erythemally weighted broadband albedo $A(ery)$, the ratio of the hemispherically measured up-welling (\uparrow) to down-welling (\downarrow) UV solar radiation is then calculated:

$$A(ery) = \frac{Q_{ery\uparrow}}{Q_{ery\downarrow}} \quad (2)$$

where Q_{ery} is the bi-hemispherically measured temporal (T) and spectral (λ) integral of the convolution of the solar radiance (E) and the erythemal response function (ϵ).

/6/ The text needs to be clarified, especially regarding the methodology and conclusions.

Our reply:

We have now clarified and re-organized the methodology and the conclusions.

/7/ The conclusions should be indicated in a separate section either at the end of the discussion or at the end of the paper.

Our reply:

We have now a separate section for the Conclusions.

/8/ 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 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.

Our reply:

We thank the Referee for this positive comment, and have now included new text to show the place of these measurements within the albedo research as suggested by the Referee. This new text is presented here above in our Reply #1 item iii), as well as in the revised abstract:

“We have measured spectral albedo, as well as ancillary parameters, of seasonal European Arctic snow at Sodankylä (67°22’N, 26°39’E). The springtime intensive melt period was observed during the Snow Reflectance Transition Experiment (SNORTEX), in April 2009. The upwelling and downwelling spectral irradiance, measured at 290 – 550 nm with a double monochromator spectroradiometer, revealed albedo values of ~0.5 - 0.7 for the ultraviolet and visible range, both under clear sky and variable cloudiness. During the most intensive snow melt period of four days, albedo decreased from 0.65 to 0.45 at 330 nm, and from 0.72 to 0.53 at 450 nm. In literature, the UV and VIS albedo for clean snow are ~0.97 - 0.99, consistent with the extremely small absorption coefficient of ice in this spectral region. Our low albedo values were supported by two independent simultaneous broadband albedo measurements, and simulated albedo data. We explain the low

albedo values to be due to i) large snow grain sizes up to ~3 mm in diameter; ii) melt water surrounding the grains and increasing the effective grain size; iii) absorption caused by impurities in the snow, with concentration of elemental carbon (black carbon) in snow of 87 ppb, and organic carbon 2894 ppb, at the time of albedo measurements. The high concentrations of carbon, detected by the thermal-optical method, were due to air masses originating from the Kola Peninsula, Russia, where mining and refining industries are located.”

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.

R: We have now changed the title to be: *“Spectral Albedo of Seasonal Snow during Intensive Melt Period at Sodankylä, beyond the Arctic Circle”*

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?

R: We agree that the sentence needs to be clarified. We have now removed the original sentence, and instead included the following sentence in the revised manuscript (Discussion):

“Our albedo results on the melting Arctic snow showed a rapid decrease in the albedo as a function of time. Thus, our data showed some indication of possibly SZA asymmetric albedo. SZA asymmetry in albedo could also be due to surface features like sastrugi, but in our case the changes in albedo were caused by melting snow, as the forward scattering nature of snow was detected by our measurements with the Sun shining from the southern directions. Hence, the albedo decline was found to dominate over the SZA dependent albedo signal. The main driver of albedo was intensively melting snow “

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.

R: We agree that this would need to be clarified. However, in the revised version we are more focused on the snow impurities (elemental and organic carbon) and these parts have been removed from the revised text.

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.

R: We agree that our study represents a boreal region beyond the Arctic circle, and this is now indicated in the manuscript as described in our replies #1ii) and #2 here above, and a photograph of the study area (new Fig. 1) was included.

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).

R: The measurement height was 2.5 m in order to allow the maximum distance from the spectrometer to the sensor holder (minimum loss in the optical cable length), and to be as close to the 2 m height recommendation by WMO as possible (although albedo can be measured at any desired height). This text is now also included in the Materials and methods.

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?

R: We refer here to our reply #5 above, and the text now included in the revised version due to this comment by the Referee:

“As the hemispherical global solar spectral irradiance is used for the calculation of the dose, the measured downwelling irradiance includes both of the direct and diffuse components, and the upwelling part consists of the hemispherically reflected global spectral diffuse radiance similarly to any non-weighted sensor used for an albedo measurement. “

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.

R: We have re-organized the revised manuscript and a more detailed description on the ancillary methods and the results and their significance is now included throughout the work.

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.

R: We have rewritten the RT model part, and we have included the research question into the Introduction:

“In RT calculations, the aim was to study how big effect (error) would a measured realistic change in diurnal albedo values cause on the modeled irradiance, if the observed diurnal albedo change of melting snow is ignored.”

Materials and methods:

“We used the Libradtran RT model (Mayer and Kylling, 2005) to calculate the up-welling and down-welling diffuse and direct spectral irradiances during a cloudless day (22 April). The measured spectral albedo (Bentham spectroradiometer data), total ozone (Sodankylä ozone

sounding data, <http://fmiarc.fmi.fi/archive/>, and Brewer spectrophotometer), and aerosol properties measured with a Precision Filter Radiometer / SunPhotometer (<http://litdb.fmi.fi/>), were used as main inputs for the RT calculations. Our hypothesis was the following: the measured diurnal albedo change is big enough to have an impact on the solar irradiance at the surface level. The albedo values used in the RT model were the ones measured with the spectroradiometer. Solar irradiances were calculated from the RT model using the morning albedo value and were compared with irradiances that were calculated using the albedo as measured at various times during the day. A difference in irradiance in [%] between 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 measurements with the model, to eliminate the effect of absolute calibration scale uncertainties of the measurement data. The absolute calibration scale should not affect the albedo measured by the Bentham spectroradiometer, as the same monochromator/light directing system is used for both the upward and downward sensors. “

Results:

“For the clear sky day 22 April, for SZA 55-70, the spectroradiometer measured albedo minimum was $A_{min} = 0.54$, the maximum $A_{max} = 0.65$ at 330 nm. The measured spectral albedo $A(\lambda)$ was dependent of the time t . These were used as input parameter values for the RT calculations. The irradiance spectra were modeled from 6 UTC to 14 UTC to produce the spectra $S1(t, A_{min}(L))$, $S2(t, A_{max}(L))$, and $S3(t, A(\lambda))$, where $S1$, $S2$ and $S3$ are the various types of modeled spectra (from 1 to 3), t is time, A is albedo, $A_{min} = 0.54$, $A_{max} = 0.65$, L is the Lambertian assumed reflectance, and $A(\lambda, t)$ is the actual measured spectral albedo. Instead of the Lambertian RT model assumption of an isotropic surface (independent of the direction), the actual measured spectral albedo $A(\lambda, t)$ is influenced by the forward-scattering nature of snow. A Lambertian albedo can still depend on wavelength. The values of the other measured input parameters for both types of RT calculations were: Ångström parameters $\alpha = 1.253$ and $\beta = 0.038$ (for the calculation of aerosol optical thickness $\tau_a = \beta \lambda^{-\alpha}$), and 347 DU for ozone. The maximum difference was observed when A_{max} was used for the model calculations, as in reality the albedo was decreasing as a function of time. For the same reason, the measured irradiance was expected to be closest to the case of A_{min} , as confirmed by our modeling results (data not shown). The differences were 2.5 - 4.5 % for wavelengths from 320 to 400 for this one day showing the 10 % change in the albedo (Fig. 9). The difference was calculated to be up to 9 % when using the results for the 4 days of the melting snow period. “

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.

R: There are small pine trees surrounding the open field of Bentham measurements. We agree that the diffuse irradiance and the shadowing by trees are important factors. The edge effect is studied in

our reply to the Referee#1: "A first estimate for the albedo error associated with the edge effect." by Meinander O. and Räisänen P. (<http://www.atmos-chem-phys-discuss.net/10/C11474/2010/acpd-10-C11474-2010-supplement.pdf>). The questions raised by the Referee are important but would go even beyond our reply cited here, hence it would become a subject of a separate article.

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.

R: We agree and have now corrected it to be "variable cloudiness"

Page 27081 line 25. The speculation regarding the frost in the night time could be avoided by producing data for night time air temperatures.

R: We agree, and the 6 UTC data have been used here to show that the air temperatures (at 2 m height) were below zero during 20-21 April, and over zero 22-24 April. Yet, snow surface temperature at the ground (Fig. 6 of the ACPD manuscript) is lower than air temperature measured at 2 m height.

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.

R: We would like to refer here on the Fig 3 (Fig. 4 in the revised version) and the new text now in the revised manuscript related to that figure:

"The spectral albedo results in Fig. 4 show the decrease of albedo as a function of time (the upper panel), and the relative wavelength dependent change (the lower panel). From the upper panel we can see the chronological order from highest albedo to lowest albedo (from 9 to 17 UTC). This decrease in albedo as a function of time is according to the snow grain size (Table 1), changing from 0.25 mm to 3 mm diameter grains as a function of time. These results agree with the Wiscombe and Warren (1980) paper, where albedo is expected to decrease with increasing grain size.

The lower panel shows that the spectral change (compared to the 9 UTC albedo) is first the bigger the shorter the wavelength (the 11 and 13 UTC values). This is consistent with the theoretical results of Warren and Wiscombe (1980), which show that absorption due to impurities in snow increases with decreasing wavelength. At 15 UTC this spectral behavior seems to disappear and the albedo values are 90 % of those in the morning regardless the wavelength. The SZA is then $\sim 70^\circ$. At 17 UTC, with SZA = 83° , the spectral behavior turns slightly toward the opposite, the difference from the morning values is larger for VIS than UV. At large zenith angles, the proportion of diffuse radiation is increased as the direct part then drastically decreases (e.g. Fig. 8 of Gardner and Sharp, 2010), and the snow albedo is known to decrease as a function of wavelength as the diffuse-to- direct radiation ratio increases (Fig. 12 of Wiscombe and Warren 1980).

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.

R: We refer to our reply to the previous comment here right above (on the same subject as the equations), and have also included a new sentence on the regressions:

“In our data, when calculating albedo in the visible from albedo at 310 nm, R^2 was only 0.6, indicating that in these data (snow with large snow grains and containing impurities) a linear model was not as good method as for UVA conversion from UVB ($R^2 = 0.97$). “

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.

R: We agree, and the size of this FMI operational albedo field is now reported (16 mx 16 m). A new Fig.1 was included to demonstrate the areas, and a new Table 1 was inserted to describe the measurements, too.

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.

R: We agree, and also refer to our previous reply. The Bentham field snow melted from 30 cm to partly totally melted during the measurement days, so it was a period of most intensive melt. Yet, there were differences between various fields. At the same time when the Bentham field was partially totally melted, the AWS showed a snow depth of 38 cm.

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.

R: The RT part has been clarified as presented in our reply here above. Also, in the revised version we have included new calculations on snow albedo with the SNICAR-model.

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

and

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.

R: We agree that more data at various locations should be used to have an Arctic-Antarctic comparison. In the revised version the focus is however in the low albedo and spectral behavior of

albedo as well as on snow impurities, and less on SZA asymmetry. Therefore the results and discussion on the SZA asymmetry have been minimized, and the Arctic-Antarctic comparison removed.

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.

R: We have removed the sentence. We have instead written more about the Fig.3 (i.e., Fig. 4 of the revised manuscript) as presented in our reply earlier.

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.

R: We use the grain diameter. We are also in the process of developing some additional methods for the grain size detection. We have now included impurity information in the revised manuscript. New revised text in Materials methods:

“The temporal changes in the snow grain sizes and shapes, according to Fierz et al. (2009), were estimated both visually with a mm-grid, and the snow grains on the grid were also macro-photographed to allow image analysis afterwards. An example of such a photo is in Meinander et al. (2008, Fig.4). “

“At the Sodankylä Arctic Center, snow surface (first 2 cm of snow) samples have been collected for impurity analysis since 2009 on a weekly basis, during snow time, from one location, protected with reindeer fences. Sampling and analysis follow the same general methodology developed by Forsström et al. (2009) and Aamaas et al. (2011), in which snow samples are melted in a microwave oven, and filtered through sterilized micro-quartz filters (55 mm diameter) using a hand pump attached to the filtering system to create a vacuum during filtering. The volume of melt water is needed for concentration conversions. Dried filters are analyzed with a Thermal/Optical Carbon Aerosol Analyzer (OC/EC) (Sunset Laboratory Inc., Forest Grove, USA) for their elemental carbon (EC) and organic (OC) concentration, following the NIOSH 5040 protocol developed by Birch (2003). The thermal-optical method was created by Birch and Cary (1996), where a detailed description of the method is presented.

One of these routine snow impurity samplings took place during the intensive melt days (24 April), one right before (17 April), and one after (30 April). The data from 2009 are used in our study related to the measured albedo. The data from 2009 - 2011 are used for the transport analysis to study the origin of the BC in snow. “

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.

R: We want to thank the Referee for pointing out the fact that this test was not earlier defined. It is now in Materials and methods:

“A simple „snow ball -test“was also made periodically in the Bentham albedo field. The test is in regular use in all Sodankylä snow research. This practical test tells if the properties of snow are such that one succeeds in making a snowball out of the snow on the ground. Snow balls can only be made when snow properties are suitable for making them; i.e. snow contains water but is not yet too wet. With the test, e.g., the start of snow melt can easily be detected, while corresponding snow property information would be hard to determine otherwise. “

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.

R: Now corrected.

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.

R:Now corrected.

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.

R: Now corrected.

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

R: Now corrected.

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

R: Now corrected.

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.

R: Now corrected.