The Cryosphere Discuss., 9, C1849–C1854, 2015
www.the-cryosphere-discuss.net/9/C1849/2015/

The Cryosphere
Discussions
Open Access

Interactive comment on "Summertime evolution of snow specific surface area close to the surface on the Antarctic Plateau" *by* Q. Libois et al.

Anonymous Referee #2

Received and published: 9 October 2015

General comments:

This study addresses the seasonal and inter-annual variability of the near-surface specific surface area (SSA) at Dome C, on the Antarctic Plateau. SSA is derived from optical measurements, from high frequency microwave observations (89, 150 GHz), and is simulated with the snow model Crocus, which is forced by atmospheric quantities from ERA-Interim reanalysis. The topic is certainly very relevant, as the understanding and quantification of the SSA evolution allow the understanding, quantification, and better simulation of the snow mass and surface energy budgets. The SSA derived from observations is used to test the Crocus capability to simulate the various physical processes contributing to the daily, seasonal, and inter-annual variability of SSA. The paper is generally well written and well argued. The weakest point is the insuffi-

cient error analysis of the spectral albedo measurements and of the SSA derived from them. The SSA derived from albedo measurements was found to be in very good agreement with the SSA simulated with Crocus. However, a better error analysis of the measurements would strengthen the Crocus validation, as the Crocus simulations are in any case based on parameters specifically adjusted to the Antarctic environment. Also the uncertainties on the SSA derived from microwave measurements could be better assessed. In conclusion, I consider the paper well suited for publication in The Cryosphere after a minor revision, which can be done addressing the specific comment listed below.

Specific comments:

p.4501, lines 3-5: "SSA determines the albedo, especially in the near-infrared" is quite a rough statement. Although SSA has a first order impact on albedo, it does not entirely determined the albedo, as snow density, snow particle shape, and other microstructural characteristics have a second order impact on albedo. Perhaps instead of using "determine", the authors can write that "SSA controls", or "strongly affects" the albedo. In fact, in the following sentence the authors write "especially", contradicting the statement that albedo is solely determined by SSA.

p.4502, line 7-9: "when solar energy is absorbed deeper, it warms up the snowpack and increases temperature gradients, which in turn enhances metamorphism close to the surface and e-folding depth". I would remove from the sentence "and e-folding depth", as it is not certain that the e-folding depth increases when the surface layer becomes more absorptive due to the metamorphism (and for instance snow crusts form).

pp. 4504-4505, Sect. 2.1.1. As the instrument to measure spectral albedo is newly designed, it would be good to better quantify its accuracy, especially with respect to the deviation from the ideal cosine response (a plot with the deviation of the ideal cosine response as a function of incident angle would be welcomed, instead of just mentioning that the angular response was determined in the laboratory). The upward and downward looking fiber optics require two specific irradiance calibrations, which include some inaccuracy. Can the authors exclude an even small systematic error in the albedo calculated from the ratio between the two signals? What about the horizontal levelling of the cosine collectors, was it regularly checked? A misalignment of few degrees could well explain the observed excessively high albedo in the visible. Was the impact of the shadow of the whole measuring system accounted for in the albedo calculation? When the authors applied the correction for the angular response following the method of Grenfell et al. (1994) did they assume isotropic reflection from the snow surface? Also this assumption can be the source of a small error (see Carmagnola, C. M., Dominé, F., Dumont, M., Wright, P., Strellis, B., Bergin, M., Dibb, J., Picard, G., Libois, Q., Arnaud, L., and Morin, S.: Snow spectral albedo at Summit, Greenland: measurements and numerical simulations based on physical and chemical properties of the snowpack, The Cryosphere, 7, 1139-1160, doi:10.5194/tc-7-1139-2013, 2013). In conclusion, it would be important to estimate the error in the albedo that remains after the applied correction of the angular response (following the method of Grenfell et al., 1994), and calculate how this error propagates to the estimated SSA.

P 4505, line 23: For Eq. (2) the mentioned reference is not correct. A correct reference is for instance Negi, H. S. and Kokhanovsky, A.: Retrieval of snow albedo and grain size using reflectance measurements in Himalayan basin, The Cryosphere, 5, 203–217, doi:10.5194/tc-5-203-2011, 2011.

p.4506, lines 10-14: the authors introduce the coefficient “A” to deal with the uncertainty on albedo measurements. However, the definition and calculation of the coefficient “A” is quite confusing: is “A” wavelength dependent? The main problem here is the lack of a proper characterization and quantification of the errors in the albedo measurements. If the albedo cannot be further corrected, and the remaining error is partly attributable to the deviation from an ideal cosine response of the instrument, then the error in the albedo is wavelength dependent, as generally the deviation of the cosine response given by diffusers is wavelength dependent. Thus, a constant “A” through the

analysed wavelength range introduces an artefact. On the other hand, if “A” is wavelength dependent, it cannot be uniquely determined together with SSA using solely Eq. (3). Finally, if “A” is related to the error in the measured albedo, its value should be shown and commented (although, I think that the error quantification should be more directly and clearly expressed than through the coefficient “A”).

P4508, Sect 2.2: what is the estimated accuracy (from literature) of the ERA-Interim air temperature used as input to the DMRT-ML model to simulate the SSA? And what is the variability (std) of the snow density during the summertime? The air temperature accuracy and the snow density variability could be used to assess the sensitivity of the retrieved SSA to these uncertainties.

p. 4509, lines 4-5: here the problem is that the authors have not explained how accurate the spectrometry-based approach to retrieve SSA is.

p. 4509, line 22: rephrase as “It was reformulated in terms of SSA using Eq. (5) of Carmagnola et al. (2014)…”.

p. 4510, line 24: “here both were both fixed”.

p. 4512, line 9: rather than “Daily variations of SSA”, Section 3.1 describes “Seasonal variations of SSA in the uppermost 2 mm”.

p.4512, line 10-12: how many SSA values were used in the calculation of the mean SSA for each 1-m transect? Given that the ASSSAP was located 5 cm above the surface, were all the used SSA measurements independent (i.e, was the field of view of the ASSSAP smaller than the distance between two consecutive measured spots)? If the SSA measurements are not independent, then the standard deviation utilized in Fig 3a to illustrate the SSA variability has a questionable meaning. p. 4514, line 1: the title of Section 3.2 could be “Seasonal variations of SSA in the uppermost 2 and 10 cm”.

p. 4517, line 26: “…one year to another than in Crocus than in the observations”.

p. 4517, lines 27-29: there is some confusion in explaining SSA evolution in different snow layers and in different time scales. I would rephrase for instance as "Although the impact of snow precipitation seems moderate in Crocus simulations of SSA in the top 2 and 10 cm, snowfall occurrence and amount drive Crocus-simulated SSA variations in the top 2 mm, consistently with observations. While the deeper layers show a seasonal SSA evolution, the surface layer mostly reflects day-to-day SSA variations".

p. 4518, line 15: "... and makes complicated the comparison between punctual observations and simulations difficult"

p. 4518, line 21: if the spectral albedo sensors are placed at the height of about 2 m, then 50% (90%) of the received reflected irradiance comes from an area with radius of about 2m (6m) (see Schwerdtfeger, P. (1976), Physical Principles of Micrometeorological Measurements, 113 pp., Elsevier Sci., New York).

P 4518, lines 20-23: The sentences "..., which is more likely to be representative of surface snow at Dome C, even though larger-scale spatial variability exists" are quite ambiguous and unclear. It has been explained through the paper that the spectral albedo measurements in the wavelength range 700-1100nm mostly depend on the averaged SSA in the uppermost 2 cm of the snowpack, which also includes the 2-mm-thick surface layer monitored with the ASSAP. If the authors are now comparing the SSA in the two layers (top 2 cm and top 2mm), they cannot state that the former "is more representative of surface snow". What is "surface snow", the top 2cm or the top 2mm? Maybe the authors mean that the SSA derived from the albedo measurements represent a larger area, but of the top 2 cm of snow, not of the very surface (top 2 mm). I would like to remark that, even if albedo was measured at longer wavelengths (1300nm or larger) to get the SSA of the top 2mm from the same large area of ~6m radius, it not at all sure that the derived SSA would have been in better agreement with the Crocus-modelled SSA. This because the scale of spatial variability of the wind-compacted/eroded and snowdrift-accumulation areas has a quasi-period of 30-50m, as the authors found in another paper (Picard, G., Royer, A., Arnaud, L., and Fily,

M.: Influence of meter-scale wind-formed features on the variability of the microwave brightness temperature around Dome C in Antarctica, The Cryosphere, 8, 1105–1119, doi:10.5194/tc-8-1105-2014, 2014). This quasi-period is evidently larger than the footprint of the spectral-albedometer, which then does not necessarily exactly corresponds to the large-scale average snow surface SSA.

Fig. 3-6: in all the 4 figures is quite difficult (or impossible) to associate the dates to the plotted data. Perhaps the authors could remove the years from the date labels and mark them as titles of the subplots ("2012-2013" and "2013-2014"). Also, plots could have the grid (horizontal and especially vertical) on.

p. 4531, line 2 of Figure caption: "mat" should be "mast".

p. 4533, last line of Figure caption: after "era-Interim" please add "(right y-axis, dark grey columns)".

p. 4534, Fig. 4: in both subplots, it would be very useful to mark (maybe with a rectangle box?) the section of time series that correspond to Fig. 3. Otherwise, it is difficult to compare Fig.4 with Fig.3.

Interactive comment on The Cryosphere Discuss., 9, 4499, 2015.