Response to reviewer 2

The reviewer's comments are in black and our answers are in red.
Modifications of the manuscript are reported in bold and italic.
The pages and lines reported here correspond to the original pdf.
New references can be found at the end of the document.

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 insufficient 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.

We thank the reviewer for the attention paid to our study and modified the manuscript to better account for measurements errors. Here, we do not pretend to develop a thorough error analysis of the albedo measurement and derived SSA, because such an exercise requires a dedicated study. The latter is work in progress and should be submitted in a few weeks. We have however added new elements that enable us to derive an estimation of the albedo error and retrieved SSA. It is based on a better analysis of the scaling coefficient A, whose meaning was more detailed according to the reviewer's recommendations. We also stress that our main interest is in SSA variations rather than SSA absolute values. All these points are addressed in details in the specific comments.

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.
We used “***strongly affects***” instead of “determines”.

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

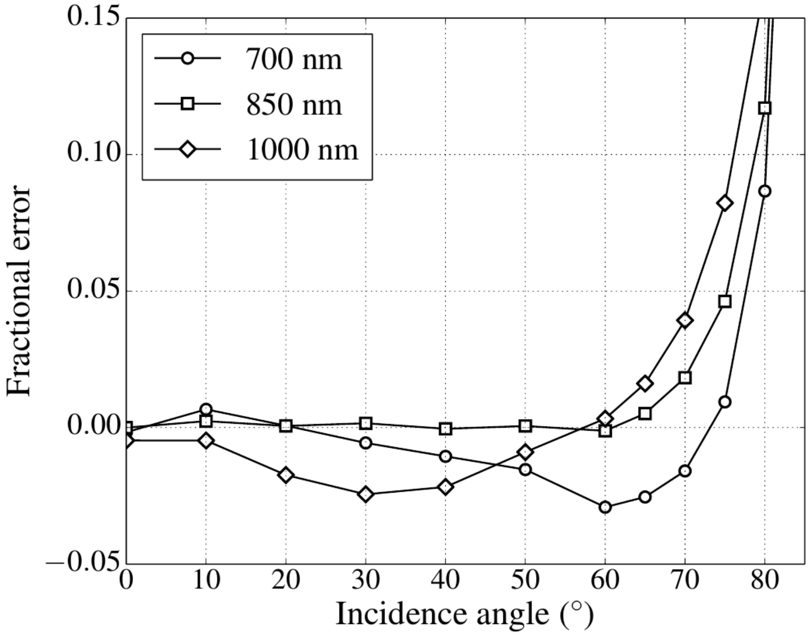
snow crusts form).
In fact the feedback loop mentioned here requires the e-folding depth to increase with metamorphism. Otherwise, only the more usual albedo feedback should be invoked. The formation of snow crusts as a result of dry metamorphism is not well understood at Dome C and there is no evidence to our knowledge that it leads to more or less absorptive layers. Here metamorphism implies SSA decrease, so this detail was added to avoid erroneous interpretation:

"[...] it warms up the snowpack and increases temperature gradients, which in turn enhances metamorphism close to the surface. ***As a consequence, SSA generally decreases and e-folding depth increases.***"

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 detailed analysis of the instrument accuracy will be presented in another study to be submitted in a few weeks. For the present study, more details are however added.

The figure below shows the deviation of our collectors response from the ideal cosine response as a function of incident angle for 700, 850 and 1000 nm. This is similar to Fig. 2 of Grenfell et al. (1994):



We do not think that the plot is worth being added to the manuscript because the detailed analysis of the collector response will be investigated in the furure study. However, the quantitative information about the collector response was added as follows P4504, L26:

"To this end, the angular response of our collectors was determined in the laboratory. ***The deviation from the perfect cosine response is less than 4% for angles below 70°, but increases beyond 80° due to the dome geometry of our collectors that capture a significant amount of light at grazing angles (Bernhard et al., 1997).***"

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?
Although we use the same angular correction for all the collectors because they were all designed in the same way, it is necessary to inter-calibrate the upward and downward looking fibers because overall transmittance of the optical system is not the same for both lines. To do this, the two collectors of the same measurement head were consecutively positioned in the downward-looking position, by simply flipping the arm (Fig. 1) of 180°. Horizontality was carefully checked for this procedure and we assumed the upward flux remained constant during this ~1 min experiment. The ratio of the spectra obtained with both lines was used to rescale the albedos analysed in the study. This inter-calibration step is now detailed in the text P4505, L3:

"It was calculated at all wavelengths with the atmospheric radiative transfer model SBDART (Richiazzi et al., 1998) for typical summer clear-sky conditions at Dome C. ***Although the upward- and downward-looking fibers are assumed to have the same angular response, the transmittances of both optical lines are different. To account for this effect, both lines were inter-calibrated. For this, the two collectors were set in the downward-looking position, by simply flipping vertically the fibers of 180°. The procedure lasted less than 30 s and was performed on a clear-sky day, so that the upward flux could be assumed constant. The ratio of both spectra was used to rescale all the albedos analysed in this study***."

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.

The system horizontality was checked at installation with embedded electronic sensors. At this time it was 0.4° and is accounted for in the procedure to correct the incident radiation. Unfortunately the inclinometer did not work in the cold and no continuous measurement was available. It has not been measured since then. However, the horizontality was estimated from the symmetry of the incident radiation around local noon. There is so far no reason to believe that a significant tilt is present in the system. The manuscript was modified as follows:

P4504, L12:
"It has 2 similar measurement heads looking to the surface and to the sky (Fig. 1). ***The horizontality of the heads was checked at installation with an electronic inclinometer and was better than 0.5°***."

Was the impact of the shadow of the whole measuring system accounted for in the albedo calculation?

The shadow of the instrument is not accounted for, essentially because measurements are taken at noon and at that time the thin shadow of the mast is far from the head (Fig. 1) and its impact likely to be minor. It is now detailed in Fig. 1 caption that the picture was taken at ***11:00 local time***. In addition, the question of this shadow is mentioned in the discussion about the scaling coefficient A below.

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).

Indeed, we assumed the reflected radiation was isotropic. According to Carmagnola et al., (2013), this assumption results in an error of 0.2-0.4%, and again is added in the factors affecting the value of the scaling coefficient A. It is now mentioned explicitly P4505, L3:

"It was calculated at all wavelengths with the atmospheric radiative transfer model SBDART (Richiazzi et al., 1998) for typical summer clear-sky conditions at Dome C. ***Contrary to incident radiation, reflected radiation is assumed isotropic."***

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.

The remaining error after all corrections are applied depend on : the uncertainty on collector response measurement, the variability of direct/diffuse irradiance, the instrument horizontality, the effect of snow anisotropy, the assumption of surface flatness, the impact of shadowing... Since the objective of the present study is not to derive an accurate measurement error using forward error modelling, we use an empirical approach to get an estimate of the precision. In fact, the scaling coefficient A somehow includes all the remaining errors, and should equal 1 if measurements were perfect. Practically the time series of the coefficient A shows that it essentially varies from 0.98 to 1.03. Hence an upper bound error is chosen for the estimated precision of albedo measurements: 3%. To convert this albedo error into SSA error, we use Eq. (1). For the spectral range and SSA of interest, the accuracy is expected to be better than 25%. This information was added in the manuscript P4506, L23:

"The SSA retrieved with this algorithm roughly corresponds to the SSA of the top 2 cm of the snowpack [...]. ***A rigorous forward estimation of the accuracy of the algorithm would require a thorough analysis of several factors including the uncertainty on the collector calibration procedure, the effect of snow anisotropy (Carmagnola et al., 2013), the shadowing of the surface by the instrument, the potential tilt of the sensor, the validity of the semi-infinite snowpack assumption, etc. Taking into account all these factors and their inter-correlation is beyond the scope of this article and will be addressed in future work. Here the accuracy is estimated using a global approach based on the analysis of the coefficient A obtained during the retrieval. Over the period of observations, it varied in the range 0.98 - 1.03, while ideal measurements would have yielded A=1. The deviation of A from 1, that is -2% to +3%, gives an estimation of the albedo measurement accuracy. Hence we assume that the albedo accuracy is 3%. The corresponding accuracy on SSA estimation is then derived from Eq. 1. For the spectral range of interest and the SSA values encountered at Dome C, the estimated accuracy of the SSA retrieval is better than 25%***."

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.

Reference to Negi et al. (2011) was added here.

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").

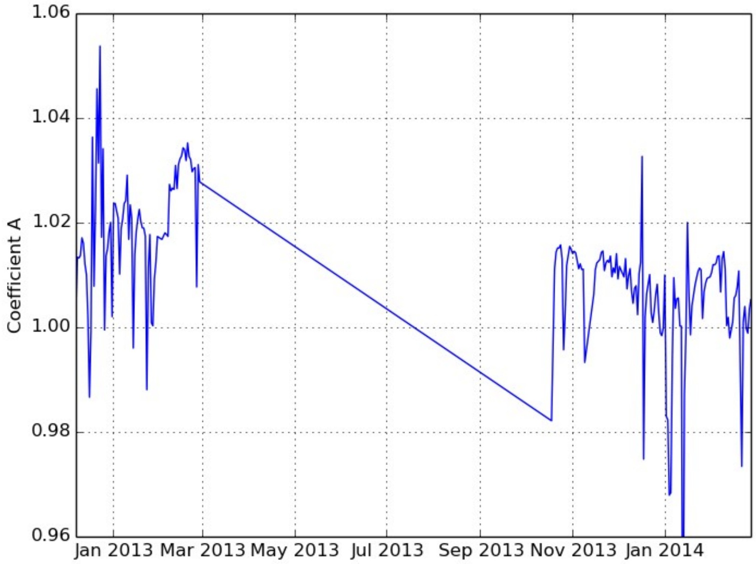
We clarified the definition of A. In fact, A somehow includes all the errors that were not accounted for before. A is indeed wavelength independent here. We believe that A mainly results from geometrical errors that are less wavelength-dependent than the deviation of the cosine response, the latter already being corrected. The text was modified P4506, L10:

"***To account for remaining uncertainties in the albedo spectra, a scaling coefficient A*** is optimized along with SSA, so that the optimized function α is actually given by:

Eq. (3)

where r_λ^{diff} gives the proportion of diffuse light. ***A is meant to compensate for all the factors affecting in a wavelength-independent way albedo measurements, that were not explicity corrected by the previous processing steps.***"

The evolution of A for the two seasons 2012-2013 and 2013-2014 is shown below. However, we think this figure is unncessary in the paper.



As it seems unclear to the reviewer that the deviation from the ideal cosine is already accounted for, it is now more clearly stated:

P4504, L25:
"Despite our effort to build highly diffusing collectors, ***small remaining deviations from the ideal cosine response need to be corrected***."

P4504, L26:
"To this end, the angular response of our collectors was determined in the laboratory, ***and used to estimate the true incident and reflected fluxes from the measured ones (Grenfell et al., 1994).***"

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.

Although recent studies have shown a warm bias of ERA-Interim temperature on the Antarctic Plateau (e.g. Fréville et al., 2014), we are not aware of any thorough evaluation of ERA Interim temperatures at Dome C. On the contrary, we have performed many measurements of snow density at Dome C, and this parameter essentially varies between 300 and 350 kg m^{-3}. For the inversion it was assumed equal to 320 kg m^{-3} which corresponds to the average value observed, but the inversion was applied for 300 and 350 kg m^{-3} as well. The obtained variability gives an estimate of the accuracy of the method. Although it depends on SSA, it is estimated to be roughly 40%. This information was added in the text as follows p4509, L4:

"As a result, this SSA time-series is not expected to be as accurate as the spectrometry-based approach described in Sect. 2.1.1. ***The most critical assumption is probably that of constant density. Assuming a density of 300 or 350 kg m^{-3} instead of 320 kg m^{-3} leads to SSA differences of maximum 40%, which we consider to be a rough estimate of the accuracy of the method.***"

We have not estimated the effect of 2 K in our particular case but it corresponds to a variation of emissivity of 0.01 which yields a smaller variation of SSA than the 10% change of density (e.g. Fig. 3, Brucker et al. 2010 for example at 37 Ghz).

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.
This accuracy is now explicitly mentioned

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

p. 4510, line 24: "here both were both fixed".
corrected

p. 4512, line 9: rather than "Daily variations of SSA", Section 3.1 describes "Seasonal variations of SSA in the uppermost 2 mm".
Corrected. The titles of Sect. 3.2 and 3.3 were also changed for consistency:
"Seasonal variations of SSA ***in the uppermost 2 and 10 cm***"
"Inter-annual variability ***of SSA in the uppermost 10 cm***"

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.
ASSSAP measures the snow reflectance every 10 ms. The number of measurements acquired depend on the speed at which the instrument is moved on the horizontal rail by the operator. Practically, the instrument was passed two times along the rail for each transect and about 1000-1500 points were acquired. Afterwards, the measured transect was divided in 1-cm long intervals. The median for all values taken in the same interval was then computed. The dots in Fig. 3a correspond to the mean of these medians. The std shown in Fig. 3a is that of all median values. The footprint of the laser beam on snow is less than 1 cm because light penetration at 1310 nm is only 2 mm, so that the std is a relevant quantity here. The text was modified P4511, L10:

"For each 1 m long horizontal transect taken with ASSSAP in 2012-2013, the average surface SSA in the range 0.25-0.75 m was computed. ***For that, the measured transect was first divided in 1 cm intervals over which the median SSA value was taken. These medians were then used to compute the average value and standard deviation for the transect,*** from which the temporal evolution of SSA at the two locations was deduced (Fig.3a)."

p. 4514, line 1: the title of Section 3.2 could be "Seasonal variations of SSA in the uppermost 2 and 10 cm".
Changed, see p. 4512, line 9 comment

p. 4517, line 26: ". . .one year to another than in Crocus than in the observations".
First "than" removed.

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".
The reviewer's suggestion was incorporated in the text except for the end that wsa replaced by:
"the surface layer mostly reflects day-to-day variations of weather conditions."

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

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).
This quantitative information and the reference were added as follows:
"Conversely, the spectral albedo measurements cover ***an area with radius of approximately 6 m (Schwerdtfeger, 1976)*** and probe deeper into the snowpack."

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.

The surface snow mentioned here is indeed quite confusing, because it refers to 2 cm, but seems to be compared to that of the transect, ie 2 mm. It has been improved P4518, L21:

“[...] and probe deeper into the snowpack. ***Hence they are more likely to be representative of the average snow SSA in the topmost 2 cm at Dome C***, even though larger-scale spatial variability exists (Picard et al., 2014).”

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.

As recommended by the reviewer, the years are now indicated as titles for each plot, the time labels are now simply 01/12 for January 12th. Grid is on for all Figures 3-6.

p. 4531, line 2 of Figure caption: “mat” should be “mast”.
corrected

p. 4533, last line of Figure caption: after “era-Interim” please add “(right y-axis, dark grey columns)”.
Done, added for Fig. 4 as well.

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.
We've added horizontal double head arrows to highlight the periods corresponding to Fig. 3. It is detailed in the caption as:
“***The horizontal arrows highlight the periods of measurements shown in Fig. 3.***”

References:

Bernhard, G., & Seckmeyer, G. (1997). New entrance optics for solar spectral UV measurements. *Photochemistry and Photobiology*, *65*(6), 923-930.

Brucker, L., Picard, G., & Fily, M. (2010). Snow grain-size profiles deduced from microwave snow emissivities in Antarctica. *Journal of Glaciology*, *56*(197), 514-526.

Negi, H. S., & Kokhanovsky, A. (2011). Retrieval of snow albedo and grain size using reflectance measurements in Himalayan basin. *The Cryosphere*, *5*(1), 203-217.