Response to reviewer 1

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:

In this paper, the authors present temporal evolution of summer time near surface snow specific surface area (SSA) at Dome C, Antarctica estimated from two types of in-situ measurements (performed during two summer campaigns: 2012–2013 and 2013–2014), and satellite remote sensing in the microwave region (obtained during 2000–2014). In addition, they investigate whether the Crocus snowpack model forced by ERA-Interim reanalysis data can be used as a useful tool to understand observed changes in near surface SSA. In conclusion, they state that observed variations of near surface SSA were successfully reproduced by Crocus; however, effects of wind on the snow compaction and SSA evolution can be overestimated in the model. Overall, this manuscript is well written and easy to follow. Provided information are valuable for TC readers who are interested in not only physics of SSA but also surface energy balance in Antarctica. In addition, snow modelers might also find this manuscript interesting. Therefore, this reviewer recommends its publication once the authors attend to the following comments. My major concern is whether the authors have confirmed the adequacy of ERA-Interim data (note that this is not observation) in Antarctica. If the accuracy of input data for Crocus (ERA-Interim) is insufficient, the reliability of presented model performance in this study can be somewhat lowered. In the following part, this reviewer gave specific comments. Please note that page and line numbers are denoted by “P” and “L”, respectively.

We are grateful to this reviewer for the encouraging comments, and tried to emphasize the adequacy of ERA-Interim data. This point has been addressed in more details by Libois et al. (2014a). Here, the paragraph dedicated to ERA-Interim data has been largely expanded as explained below in our response to the specific comments.

Specific comments:

P4501, L7-9: SSA also controls the e-folding depth as well.
This was added in the text:
Snow specific surface area [...] ***strongly affects*** snow albedo ***and light e-folding depth***, especially [...].

P4502, L22: Please describe more why detailed snowpack models are not fully adequate for polar environments.
We added the following sentence:
“***In fact,*** such models are usually not fully adequate to polar environments (Dang et al., 1997, Groot Zwaaftink et al., 2013). ***Their semi-empirical parameterizations for snow metamorphism, compaction and fresh snow characteristics are indeed often based on observations made in alpine environments (e.g. Marbouty, 1980, Guyomarc'h et al., 1998), and do not necessarily perform well in colder and drier areas. In addition,***”

P4505, L3-5: Does it mean that the authors used data obtained only under clear-sky conditions? Please clarify.
The method based on the clear-sky parameterization was applied to all data independently of the sky

conditions. It means that cloudy conditions are included in the retrieved SSA time series. This point is now clearly specified:

P4505, L2:
"Since this information is not available from measurements, the direct/diffuse ratio was supposed to depend only on SZA, ***and was thus treated in the same way for clear-sky and cloudy conditions***."

P4506, L17:
"This procedure is applied ***every day independently of sky conditions. It is repeated every year from 18 October to 27 February***, when SZA at noon remains lower than 67°."

P4509, L1-6: Please indicate expected accuracy of this remote sensing technique.

The accuracy of this remote sensing technique was not mentioned by Picard et al. (2012) and has not been addressed since then. However, it is likely that the largest source of uncertainty is the density value chosen in the inversion procedure. Here snow density was assumed equal to 320 kg m^{-3}, which corresponds to the average value observed at Dome C, but we tested the inversion for 300 and 350 kg m^{-3} as well, which essentially corresponds to the variability observed in the field. We believe that the obtained SSA range 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 kg m^{-3} (respectively 350 kg m^{-3}) instead of 320 kg m^{-3} yields SSA differences up to +20% (respectively -40%), which gives a broad estimate of 40% for the accuracy of the method***."

P4509, L13: This reviewer could not understand why the authors listed "sphericity" here. This is a "virtual" parameter.
In fact the listed variables are ***prognostic*** variables (not diagnostic, updated P4509, L12). Although sphericity may be considered a variable with loose physical definition, it evolves from a time step to another and is used to compute snow compaction by the wind for instance. Here it is mentioned to point out the difference with the former version of Crocus (Brun et al., 1989, 1992) which also included the ad-hoc variables dendricity and grain size (replaced by SSA by Carmagnola et al., 2014). Hence sphericity was maintained in the list of variables.

P4510, L3-6: The explanation provided here is a bit difficult to follow. Please describe in more detail.
This explanation was reformulated to be more understandable P4510, L1:
"***SSA decrease is computed from the formulation F06 of snow metamorphism (Carmagnola et al., 2014) which is based on a fit of the semi-empirical microphysical model of SSA decrease rate proposed by Flanner et al. (2006). Because of working in the mid-latitude context, the fit in Carmagnola et al., 2014 was computed over a period of 14 days, as in Oleson et al. (2010). Here we use the same approach but extend the period to*** 100 days to account for the slower metamorphism resulting from the low temperatures prevailing at Dome C."

P4510, L24: typo: "both were both . . ."
corrected

P4510, L27: The authors introduce ERA-Interim to drive Crocus in this study. Have the authors confirmed its accuracy in Antarctica? In case systematic biases were found in some properties, did the authors correct them?
ERA-Interim data accuracy was investigated by several authors and more specifically to run Crocus in Libois et al. (2014a) and Fréville et al. (2014). Only the precipitation rate was corrected because it showed a significant negative bias at Dome C. Based on in situ observations of 10 m wind and 2 m air temperature, the latter ERA-Interim data seem adequate at Dome C. At least they do not show any significant bias and were thus used as is. This is now detailed as follows:

"Crocus was forced by ***3-hourly*** ERA-Interim atmospheric reanalysis for 2 m air temperature and specific humidity, surface pressure, precipitation amount, 10 m wind speed, and downward radiative fluxes. ***ERA-Interim data were already used by Fréville et al. (2014) to simulate snow surface temperature on the Antarctic Plateau. As detailed in Libois et al. (2014a), precipitation rate was multiplied by 1.5 to ensure that simulated annual snow accumulation matches observations at Dome C. On the contrary, ERA-Interim wind was found in good agreement with measurements performed on the 40 m high instrumented tower at Dome C (Genthon et al., 2013). Libois et al. (2014a) also pointed that drift events observed at Dome C could satisfactorily be predicted from ERA-Interim wind time series, further supporting the consistency of wind data. As for air temperature, it does not show any significant bias during the summer from 2000 to 2013 compared to Dome C II automatic weather station (http://amrc.ssec.wis.edu/aws). It does show a positive bias of about 2K during the winter, but this is not critical for our study because snow metamorphism barely operates in winter.*** "

P4510, L27 – P4511, L1: Please indicate time intervals of ERA-Interim and output data from Crocus simulations.
ERA-Interim data were prepared at 3-hourly time step by composing analysis and short term forecasts. This was added (see previous comment).
Crocus is run at a 15 min time step but output data for this study are considered every 12 hours resolution because our main focus is on snow metamorphism, which operates at the scale of several days.

P4511, L4:
"Then, Crocus was run from 2000 to 2014 ***and the full state of the snowpack was recorded every 12h,*** yielding the reference simulation that is analysed in the following."

P4511, L1: Please indicate how many model layers were set in the 12 m snowpack. In addition, it might be informative to list model layer thicknesses set in this study.
This information was added as follows P4511, L11:
"The snowpack was first initialized with a~depth of 12 m [...]. ***It comprised 25 layers***."

We also precised that the number of snow layers is variable in Crocus in case it was not clear for the readers (P4509, L11):
"The number and thickness of numerical snow layers evolve with time."

and P4509, L13:
"Crocus was adapted to the specific meteorological conditions prevailing at Dome C [...]. ***In particular, the optimal thicknesses of the 5 topmost layers were set at 2, 3, 5, 5 and 10 mm, to ensure that surface processes are accurately represented***."

P4512, L9: It seems to me that the title of Sect. 3.1 "Daily variations of SSA" is not suitable, because

data intervals presented in Fig. 3 are several days (not a few hours or less).
The title was changed into "***Seasonal variations of SSA in the uppermost 2 mm***"
To maintain consistency from a section to another. The titles of Sect. 3.2 and 3.3 were also changed:
"Seasonal variations of SSA ***in the uppermost 2 and 10 cm***"
"Inter-annual variability ***of SSA in the uppermost 10 cm***"

P4513, L14: Does this explanation mean that the top most model layer thickness of Crocus is less than 2 mm?
The thickness of Crocus layers evolve depending on compaction, precipitation, sublimation, etc. Layers can also be merged or split depending on their properties. The model always tries to match an optimal thickness profile. Here this optimal profile is 2, 3, 5, 5 and 10 mm for the uppermost 5 layers as mentioned above. It means that the uppermost layer thickness tends to be around 2 mm. After a light precipitation event, the topmost layer can be 1 mm thick. In this case, the computation over the topmost 2 mm implies that at least 2 layers are accounted for. In the case the topmost layer is more than 2 mm, the average SSA over the topmost 2 mm is simply that of this layer. The information regarding layer thickness has been added before.

P4513, L23-26: Please discuss why Crocus could not simulate the effect of soft snow removal by the wind, and the formation of surface hoar.
Such processes are indeed currently not simulated explicitly by Crocus. The last sentence of the conclusion clearly states that these processes could be simulated by Crocus and should be regarded as potential improvements.

P4521, L8:
"Other physical processes not yet simulated by Crocus should also be regarded as potential progress for simulating snow properties on the Antarctic Plateau, such as the formation of hoar crystals, and the mixing of the topmost layers of the snowpack due to snow drift."

Currently, the only way Crocus can loose mass to the atmosphere is via sublimation, which in the model does not lead to any change in snow physical properties. The effect of the wind is essentially to compact snow and possibly to increase SSA as a result of smaller ice crystals falling last after a drift event. In the model, wind does not physically remove snow, it can only foster sublimation, which is a very different process than that observed at Dome C. As for surface hoar, condensation can occur on top of the snowpack, but the newly deposited snow has the same properties (density and SSA) as the uppermost layer, which is quite different from what is observed in the field and called surface hoar. In addition, surface hoar resulting from vapor transfers within the snowpack is not simulated at all because such mass transfer is not simulated so far, although work is in progress on this critical question.

To make it clear we slightly modified the sentence P4513, L23:
"The effect of soft snow removal by the wind as well as the formation of surface hoar are ***currently*** not simulated by Crocus [...]."

P4514, L20-25: This reviewer could not follow what the authors intended to explain here. It might be better to reformulate.
There are 2 ways to compute the surface SSA. Either to compute a linear average, or an exponential decay "average". The latter accounts for the fact that the uppermost layers (few mm) contribute more to the albedo than layers below. The exponential decay correspond to the light e-folding depth in snow. The paragraph was reformulated as follows:

"***Since solar irradiance decreases exponentially with depth, the uppermost mm of the snowpack contribute more to the albedo than the snow below. As a result, the SSA retrieved from albedo measurements is the result of a convolution of the actual SSA profile by an exponential to a first approximation. To account for this effect, the simulated SSA was also computed using*** a 2 cm exponential decay (Mary et al., 2013) rather than a linear average. ***This resulted in slightly higher SSA (less than 5%). Likewise, since the choice of 2 cm is to some extent arbitrary, the average was also computed over the topmost 1 and 4 cm***. ***It resulted*** in less than [...]"

P4515, L1-2: The contrasting feature of summer SSA decrease between 2012–2013 and 2013–2014 is interesting. Could the authors discuss the reason of this difference by referring to meteorological conditions during these two summers?
The analysis of ERA-Interim precipitation shows that during the winter 2012, the total amount of precipitation has been 45% more than during the winter 2013. This information was added in the discussion:

"The summer decrease was thus more significant in 2012-2013 than in 2013-2014, which is reproduced by Crocus (Fig. 5). ***More precisely, the main difference between both summers is the initial value of SSA. This can be explained by the fact that ERA-Interim precipitation accumulated from March 1st to November 1st was 45% larger in 2012 than in 2013***."

P4520, L6-15: Before discussing the impact of wind speed on the topmost 7 cm SSA evolution, the authors should demonstrate accuracy of wind speed obtained from ERA-Interim (related to "P4510, L27"). If wind speed from ERA-Interim is overestimated, this discussion has no meaning.
We believe that wind speed at 10 m is sufficiently well simulated by ERA-Interim, and does not show any particular bias. This was detailed previously (response to "P4510, L27"), and is much more detailed in the study by Libois et al. (2014a) dedicated to the impact of wind on snow properties.

P4520, L20: The validity of meteorological forcing used in this study has not been confirmed (related to "P4510, L27").
See previous comment and "P4510, L27".

Figure 1: "mat": typo?
Corrected

Figure 3a: Two hatched areas are difficult to distinguish from each other.
We replaced the hatched areas by shaded areas .

Figure 3b: What do the authors mean by the "dark line"? In addition, what are the dark dots? This is not explained in the caption.
In fact what you call dark dots were referred in the caption as "clear dots" and the dark line referred to the line linking the white dots. This was indeed misleading, and was clarified in the caption as follows:

"**The grey circles indicate single measurements and the white circles highlight the median value for each day**."

References:

Genthon, C., Six, D., Gallée, H., Grigioni, P., & Pellegrini, A. (2013). Two years of atmospheric boundary layer observations on a 45-m tower at Dome C on the Antarctic plateau. *Journal of Geophysical Research: Atmospheres*, *118*(8), 3218-3232.

Guyomarc'h, G.., & Mérindol, L. (1998). Validation of an application for forecasting blowing snow. *Annals of Glaciology*, *26*, 138-143.

Marbouty, D. (1980). An experimental study of temperature gradient metamorphism. *Journal of Glaciology, 26*, 303-312.