Interactive comment on "The growth of sublimation crystals and surface hoar on the Antarctic plateau" by J.-C. Gallet et al.

Anonymous Referee #1

Received and published: 27 January 2014

Review of 'The growth of sublimation crystals and surface hoar on the Antarctic plateau' by Gallet and others

Summary
This paper describes possible formation mechanisms of hoar/sublimation crystals at the surface of the Antarctic Plateau near Dome C. Over a period of several days, SSA measurements are performed to determine typical crystal sizes while near surface density is determined using conventional methods. A daily cycle is found in both. Next, a 1D snow model is used to study energy and water vapor exchange at the surface; because this model is not designed to simulate SSA changes, theory from frost flower formation on sea ice is used to qualitatively explain the phenomena at hand.

Finally, the impact on surface albedo is determined using a radiation transfer model. Scientific and technical quality This is an interesting paper dealing with an important topic, namely the interaction of heat and mass exchange at the surface of vast stretches of the Antarctic ice sheet interior. The paper is generally well written, but at some points the authors need to formulate more accurately, see specific comments below. The figures are generally of good quality, although here also some points need to be addressed, see below. All in all, I recommend the MS be accepted for publication in The Cryosphere with minor revisions.

We wish to thank reviewer#1 for his/her comments. Answers are inserted in blue below.

General comments

Towards the end of the Introduction (e.g. page p. 5974) please explain more explicitly the difference between hoar and sublimation crystals and their proposed formation mechanisms.

We will add a couple of sentences to explain shortly to main differences between the mechanisms responsible for the formation of hoar and sublimation crystals. Briefly, surface hoar forms by the deposition of atmospheric water vapor while sublimation crystals form from an upward flux of water vapor coming from the warmer snowpack into the cooler air.

Section 3.2: the RMSE of >2K between modeled and 'observed' surface temperature comes across as rather large, and could influence the results given the subtle processes at play here. Please allow the reader to form his/her own opinion about model performance by showing both modeled and observed surface temperature in Fig. 2.

Thank you for that remark. We will add the modeled temperature in Fig 2. Moreover, considering the potential uncertainties on the snow surface temperature measurements, we have been using only the modeled data of Crocus so that the temperature gradient, which drives the vapor water flux and therefore the formation of sublimation crystals, will not be affected by these uncertainties.

Please include the sub-surface heat flux in Fig. 4. For instance, the daytime values of net radiation (_40 Wm-2) are only for _50% compensated by the sensible and latent heat fluxes, which implies a large value of sub-surface heat flux.
Thank you for spotting that. We will add the sub surface flux in Fig 4.

As indicated, uncertainties in observed (near) surface densities are large. In section 3.2 surface density is modified in an arbitrary fashion to see whether sublimation regimes are sensitive to this parameter. This can and should be pursued in a more quantitative manner, e.g. by prescribing various combinations of surface and subsurface densities in CROCUS in order to minimize the error in modeled surface temperature. This then gives a first order quantitative estimate of uncertainty in surface density, which could then serve as justification for the different values for surface density chosen later in the paper.

As already mentioned before, the uncertainty on the temperature does not affect the temperature gradient. The uncertainty on densities measurement is claimed to be 20% in our paper and the densities that we ascribe as “arbitrary” is of the order of 40% lower than the measured one and in the range of measured densities of fresh snows (Domine et al., 2007b). The objective of changing the density is in fact to change the thermal conductivity, calculated using Yen’s equation in our model (Yen, 1981). We agree on the fact that another run considering this time higher densities and therefore higher thermal conductivity would be helpful to estimate the effect of the temperature gradient on the possibility to form sublimation crystals at the surface of the snowpack. However, Yen’s formulation calculates thermal conductivity based on density only, while it is known that other variables such as snow cohesiveness comes into play (Domine et al., 2011). Testing various densities will affect the theoretical thermal profile of the snowpack but would not give any indication on our density uncertainty, especially when considering the weak cohesiveness of the snow surface during our experiment. If anything, it would give us information on the validity of Yen’s equation, convoluted with our density uncertainty. This would not be easy to exploit in a useful manner.

Specific comments

Abstract: the sentence "On the Antarctic plateau, the budget of water vapor and energy is in part determined by precipitation, but these are so low that the dynamic of snow crystal growth and sublimation at the surface can be important factors" is unclear; I suggest to revise into e.g.: "On the Antarctic plateau, precipitation quantities are so low that the surface mass budget is for an important part determined by snow crystal growth and sublimation at the surface."

Indeed your version is clearer than ours, thank you very much.

p. 5972, l. 8: very likely due -> very likely formed due

Thank you, we will then modify as follow: …. “we conclude that the formation of theses crystals was very likely due to the …..”

p. 5972, l. 13: a 10 W m-2 forcing in net shortwave radiation, I assume.

The minus sign is assigned to the snow surface. As the snow SSA increases at noon, the snow albedo increases and therefore we calculated that 10 W m^{-2} more are reflected due to the presence of sublimation crystals, resulting in a drop of 0.45 K of the snow surface temperature. We will modify as follows to avoid misunderstanding: “….the specific surface area variations of the surface layer can induce an instantaneous forcing at the snow surface up to – 10 W m-2 at noon, ….”

p. 5972, l. 16: Please be more precise in your formulation. For instance, "Snow is the most reflective surface on Earth" is better written as "A closed cover of fresh snow is among the most reflective surfaces on Earth".

We will modify it as follows: “Snow is one of the most, or perhaps the most reflective surface on Earth”.

p. 5973, l. 1: Likewise "In the visible..." -> "In the visible part of the electromagnetic radiation spectrum (_400-700 nm)...".

Thank you for that clarification, we will modify it.

p. 5973, l. 14: ’M’ is undefined.

Thank you for spotting that, M is the mass in Kg and we will define it.

p. 5973, l. 27: What is ’critical’ about the solar zenith angle?

Indeed our wording was not very good. We only intended to say that the solar zenith angle is a very important parameter that has to be taken into account when studying the daily variations of the snow albedo. We will change critical by crucial.

p. 5976, l. 20: The paragraph "Solar radiation penetration is computed as a function of snow optical radius and age, as a surrogate for snow darkening due to the deposition of aerosols, a process which is irrelevant at Dome C. Snow darkening was thus not accounted for in our simulation." is unclear. Snow darkening can be caused by aerosols or by snow metamorphism, and the latter is taken into account. So please reformulate latter sentence into e.g. "Snow darkening by aerosols was not accounted for in our simulation."

We will reformulate as follows: “Solar radiation penetration is computed in the Crocus model as a function of snow optical radius and age, i.e. number of days since snowfall. The sensitivity to age is a surrogate for aerosol deposition. Due to the very low level of impurities on the high Antarctic plateau, Crocus simulations in this study were done without accounting for the effect of age (i.e. aerosols deposition) on the solar penetration. ”

p. 5976, l. 20: Please elaborate on how solar radiation penetration in snow is modeled, i.e. number of wavelength bands, which incoming spectrum and direct/diffuse fraction is chosen, vertical resolution in the snow etc.

In the current version of the Crocus model, solar penetration is modeled over 3 spectral bands: (0.3-0.8) , (0.8-1.5) and (1.5-2.8) µm. The solar irradiance provided by ERA-interim is a broadband value. It is separated across the 3 bands using fixed coefficient and radiation is entirely diffuse. The vertical resolution in the snow is not fixed since it corresponds to the layering of the snowpack controlled by the model (Vionnet et al., 2012). Despite these rather simplistic assumptions, as demonstrated in (Brun et al., 2011), Crocus allows a good simulation of the vertical sub-surface temperature profile at Dome C. We will specify this in the revised version.

p. 5977, l. 5: first you state: "In this study, we used the meteorological variables provided by ERA-interim reanalysis (Dee et al., 2011)." But in line 12 you continue: "Consequently, for January 2009, we used wind speed, humidity and temperature measured (ventilated sensors) 4m above ground at the tower instead of ERA-interim reanalysis." It is unclear what you did exactly: did you first force CROCUS with ERA-Interim, and with AWS data only for Jan 2009? In that case, would this not result in a jump in the forcing, given that ERA-Interim is biased?

Thank you very much for that remark. Indeed we use ERA-interim data to initialize properly the temperature of the snowpack from January 1st 2007 until January 1st 2009 (and not 13th as written in the first version). The field data were not available over the two years needed to initialize the snowpack temperature. Then, we replaced ERA temperature, wind speed and humidity by measured values from January 1st until the end of the experiment. A small jump indeed appears on January 1st but its effect disappears within a few days and our experiment starts on January 18th. We will mention this in the revised version.

p. 5979, l. 10: reliable -> reasonable;

Thank you, we will modify it.

Table 2: 'Condensation' and 'Evaporation' should be 'Deposition' and 'Sublimation' for the cold conditions at Dome C. Same for y-axis labels in Fig. 7 ('Condensation' -> 'Deposition').

Thank you for that remark. We do agree that the term sublimation is more appropriate than evaporation for the conditions at Dome C. We will modify that in the text and in Table 2. Deposition and condensation are both used to describe the change from a gas phase to the solid phase but intuitively we do think that condensation is more appropriate.

In Figs. 4, 7 and 8, please shorten x-axis labels to improve readability. In general I suggest to change the time axes in all relevant figures into a single number, e.g. 'Day of Jan. 2009'.

This has been taken into account in the revised version of the figures.

References cited:

Brun, E., Six, D., Picard, G., Vionnet, V., Arnaud, L., Bazile, E., Boone, A., Bouchard, A., Genthon, C., Guidard, V., Le Moigne, P., Rabier, F., and Seity, Y.: Snow/atmosphere coupled simulation at Dome C, Antarctica, Journal of Glaciology, 52, 721-736, 2011.
Domine, F., Taillandier, A. S., and Simpson, W. R.: A parameterization of the specific surface area of seasonal snow for field use and for models of snowpack evolution, Journal of Geophysical Research-Earth Surface, 112, F02031, 10.1029/2006jf000512, 2007b.
Domine, F., Bock, J., Morin, S., and Giraud, G.: Linking the effective thermal conductivity of snow to its shear strength and density, Journal of Geophysical Research-Earth Surface, 116, F04027 10.1029/2011jf002000, 2011.
Vionnet, V., Brun, E., Morin, S., Boone, A., Faroux, S., Le Moigne, P., Martin, E., and Willemet, J. M.: The detailed snowpack scheme Crocus and its implementation in SURFEX v7.2, Geosci. Model Dev., 5, 773-791, 10.5194/gmd-5-773-2012, 2012.
Yen, Y.-C.: Review of thermal properties of snow, ice, and sea ice, United States Army Corps of Engineers, Hanover, N.H., USACRREL Report 81-10, 1-27, 1981.