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

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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.

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.

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.

Please include the sub-surface heat flux in Fig. 4. For instance, the daytime values of net radiation ($\sim$40 Wm-2) are only for $\sim$50% compensated by the sensible and latent heat fluxes, which implies a large value of sub-surface heat flux.

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.

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."

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

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

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".

p. 5973, l. 1: Likewise "In the visible..." -> "In the visible part of the electromagnetic radiation spectrum ($\sim$400-700 nm)...".

p. 5973, l. 14: 'M' is undefined.

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

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."

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.

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?

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

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').

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'.

Interactive comment on The Cryosphere Discuss., 7, 5971, 2013.