**The Cryosphere
Discussions**

Interactive comment on "Vertical profiles of the specific surface area of the snow at Dome C, Antarctica" *by* J.-C. Gallet et al.

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Response to A. Kokhanovsky

A. Kokahnovsky's comments are reminded in italics for clarity

This is an important and interesting paper aimed at the snow vertical profiling (snow specific area, grain size). The comments given below can be used by authors to improve the final version of the manuscript.

Comments

p.1650. I disagree that the complex shapes of snow crystals can adequately be represented by spheres for radiative transfer modelling. There is an extensive evidence

that this is not the case (see, e.g., Mishchenko et al., JQSRT, 1999; Kokhanovsky and Zege, Appl. Optics, 2004; Xie et al., JQSRT, 2006). The works cited by you address the question if the results of modelling (say, specral albedo) can be adequately represented using spherical grains. Yes, it is possibble to find such models. However, they do not provide accurate estimations of the size and the specific area of the snow. The results derived on the use of spherical models are highly biased. I advice that you study this question in detail using, e.g., hexagonal crystals as input in DISORT (see, e.g., Xie et al., 2006).

Thank you for bringing up this crucial point. You are right, the bidirectional reflectance of snow depends on grain shape. This has been amply demonstrated in the work you cite and in others, including work by Warren and others. We have also recently done work that demonstrates that snow hemispherical reflectance, i.e. albedo, depends on grain shape (Picard et al., 2009). However, all those studies were theoretical and only apply to a given and single crystal shape. Natural snow is almost never made up of a single shape. It happens that the result is that the mixture of snow crystal shapes present in natural snow behaves as spheres. An indication of that appeared in (Gallet et al., 2009), who studied snow albedo at 1310 nm for snow types such as depth hoar, rounded grains, fresh snow of many types of crystals, and observed that, within experimental error, the SSA-albedo relationship fell on a single curve and that crystal shape did not need to be taken into account. Subsequently, (Arnaud et al., 2011) modeled the SSA-albedo relationship and showed that this could be done using the b value of (Kokhanovsky and Zege, 2004) that corresponds to spheres. We therefore believe that the albedo of natural snow can be modeled by representing snow as spheres of equal SSA. Of course, this is not true for bidirectional reflectance (Dumont et al., 2010). We will change our text accordingly to explain the limits of validity of our statement.

p.1650. Eq. (1) is valid not only for spheres (see Domine et al., 2008).

Sure. We will reword that part to clearly define reff, the optical radius, as a function of SSA.

p.1650. What is the penetration depth (PD) at 1300nm? What vertical depth you need to make useful measurements of SSA? I guess, the PD must be larger than several grain diameters. Is there water vapour absorption at 1300nm? How do you correct for corresponding effects (including absorption by liquid phase)?

This is a very useful question. Using DISORT, we calculated the flux as a function of depth for 2 typical snow types to deduce the e-folding depth, which we equate to the penetration depth. For recent snow, (density = 120 kg m-3, SSA= 40 m2 kg-1), PD=7.5 mm. For Antarctic depth hoar, (density = 230 kg m-3, SSA= 12 m2 kg-1), PD=6.8 mm. So the PD is of the order of 1 cm. We will add that in the paper. Given the optical path length, absorption by water vapor is completely negligible. The snow studied here, in Antarctica, never had any liquid water, which will be mentioned in the revised version.

p.1652. The accuracy of reading of the is 2mm. This is not a small number. The extinction coefficient is snow is about 1 inverse cm. So the error in optical thickness is about 2, which is not a small number as far as snow optics is concerned.

The accuracy of 2 mm is dictated by natural conditions: the boundaries between snow layers is often not sharp, especially in Antarctica, and layer boundaries fluctuate so that layers thicknesses vary, often at the 10 cm horizontal scale. This being said, we agree that this should not stop us from evaluating the resulting error on spectral albedo. To that end, (i) we will mention in the methods section that the resulting error will be evaluated in the modeling section; (ii) in the modeling section, we will compare the spectrum of C3 with that of the same pit, but with the layer boundary at 1 cm depth shifted down by 1 mm and the boundary at 2 cm shifted down by 2 mm. In fact, we found that the difference in albedo is completely negligible. The maximum difference is 0.05

p.1653. How stable is your calibration curve? You need to prove that your calibration curve can be used for any snow sample.

This has been proven in (Gallet et al., 2009). The reference was mentioned at the start

of the paragraph, but we will mention it again after the relevant sentence to make the point clear.

p.1661. You may mention that it follows from Eqs. (2), (4) that the optical thickness tau is just SAI/2. Therefore, you may use measurements of tau to estimate SAI. In Eq. (2) you need to wtite 'rosnow, i0.

We will remove the SAI section. SAI has not convinced referees and we will take that into account. SAI is relevant for chemistry, and we agree that this is mostly a physics/ radiative paper so there is little room for a chemistry aspect in this paper.

p.1661. You write that the spectral albedo depends on density. Please, prove it refering to measurements or references. In case of a semi-infinite snow, there are two parameters, which influence the reflectance : the single scattering albedo and the phase function (Mishchenko etal., JQSRT, 1999). Both do not depend on the snow density. However, both are influenced by the shape of grains. Please, estimate the influence of crystal shapes on your results (the determination of SSA and the average grain size).

Thank you for bringing up this point. We fully agree that for a semi-infinite snow layer, albedo does not depend on density. However, for a natural snowpack made up of layer each having a different SSA and a different density, changing the density of a given layer will change the albedo of the snowpack. For example, dividing the density of the top 1 cm layer (which often has the highest SSA) by 2 will decrease its optical depth and will reduce the albedo of the snowpack. We will change the wording and say "The spectral albedo [….] only depends on the SSA, density and thickness of each snow layer."

p.1662. You write that clouds enhance the diffuse radiation. This is not always the case. Actually it could be quite dark under clouds (almost no radiation penetrates).

You are right. We meant that "clouds enhance the diffuse fraction of radiation." This will be corrected.

p.1663. I am not sure that it is useful to introduce Eq. (6). The radiative transfer theory enables the calculation of albedo for any atmospheric state numerically not using approximations like Eq.(6). Why yo do not make direct numerical simulations instead?

What you are saying is correct, it is possible to calculate the albedo for any atmospheric state using radiative transfer calculations. However, using data given in Tables 5 to 7, it is easy to calculate albedo for any diffuse light fraction using equation (6) (changed to 5 in the new version). Not all readers are familiar with the use of radiative transfer models, while anyone can use equation (6). We will move the equation to section 4.5, where we bring up the data, and will explain how the data and equation (6) allow the calculation of albedo for different fractions of diffuse radiation.

p.1671. You state that the thin high-SSA layer was never observed. This is too strong sentence.

We will completely rewrite the section on this thin high-SSA layer, also in response to the comments of referee 1. We will remove this sentence, even though it is true that it has never actually been observed. Our aim is to reexamine constructively the interesting hypothesis of (Grenfell et al., 1994) in the light of new data, not to criticize it. In any case, since we cannot reach a firm and definitive conclusion on its existence, we will refrain from making sharp statements. We also became aware of the thesis work of Peter Kuipers Munneke (University of Utrecht) who studies radiation and snow physics at Summit, Greenland, and made some interesting observations on the surface layers. Although this work has not ben published, and even though it deals with Greenland and not Antarctica, we feel it is relevant and will briefly mention and discuss it in the revised version.

Table 2. Please, include the geometrical thickness of layer here and also average grain size (related to SSA).

Since we will remove the SAI section, this table will be removed.

Table 3. Could you give grain size here.

Again, since we will remove the SAI section, this table wil be removed.

Fig.17. The DISORT results depend on the grain size and shape. How did you select these important parameters for Fig.17?

We used our stratigraphic data (SSA and density as a function of depth) given in Tables A1 and A2. This was mentioned in section 4. To make sure this will be clear to readers, we will mention in the captions of Figs 17 and 18 that DISORT calculations use the data of those Tables.

References

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