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Discussions

Interactive comment on "Effects of snow grain shape on climate simulations: Sensitivity tests with the Norwegian Earth System Model" *by* Petri Räisänen et al.

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We thank Anonymous Referee #1 for his/her constructive comments on the manuscript. Point-by-point responses to the comments are provided below. The referee comments are written in *italic* font, and our responses in normal font.

Comment: *Recommendation: Accept after minor revision. This is an interesting study which is suitable for publication in The Cryosphere. It is well-written.*

lines 8-9. Say that the nonspherical grains are compared to spherical grains with the same specific surface area.

Response: The effective radius is defined in our study using the ratio of volume to projected area rather than volume to total surface area. Therefore, even if the difference might in practice be small for snow, it is in principle more correct to use here "specific projected area" instead of "specific surface area".

Change in the manuscript: In the abstract, it wil be said: "Therefore, for the same snow grain effective size (or equivalently, the same specific projected area), the snow broadband albedo is higher when assuming non-spherical rather than spherical snow grains, typically by 0.02–0.03."

Comment: *line 32. Cite also Dang et al. (2015)*

Response and change: We will cite this paper in the revised manuscript.

Comment: *line 47. If the snow grain contains concavities and hollows, then the projected area is not the appropriate measure, because internal surfaces also deflect photons. See Grenfell et al. (2005). Admittedly, although cavities are present in atmospheric ice crystals, they are uncommon in surface snow.*

Response: In fact, we agree only partially with this comment. It is certainly true that the single-scattering properties of non-spherical particles can be influenced by concavities and hollows, and it is also true that no definition of effective radius (including our Eq. 1) is *always* optimal. Hovever, it is not at all clear that defining the effective radius in terms of the total surface area A

$$r_{VA} = 3\frac{V}{A} \tag{1}$$

rather than in terms of the projected area P (which is what we use in the manuscript, although with the notation r_e)

$$r_{VP} = \frac{3V}{4P} \tag{2}$$

would be an improvement, even in the case of concave particles. It should be noted that Grenfell et al. (2005) only consider r_{VA} and do not compare its performance to r_{VP}, so their paper does not yield direct information on which choice actually works better. However, for concave particles $r_{VA} < r_{VP}$, and as noted by Grenfell et al. (2005) themselves, this leads to an overestimation of optical depth. This might not be a major issue for snow (for which the optical depth is usually very large), but in the case of cirrus clouds, it would necessarily lead to an underestimation of direct solar radiation. Furthermore, according to our (admittedly limited and unpublished) comparisons, r_{VP} appears to be a better predictor of the single-scattering co-albedo of nonspherical particles than r_{VA}. That is, when plotted as a function of r_{VP}, the differences in co-albedo between different concave and convex particle shapes tend to be smaller than when plotted as a function of r_{VA}. The problems with using r_{VA} to represent co-albedo can in fact also be seen from Fig. 3 of Grenfell et al. (2005). The values of co-albedo are systematically and even substantially higher for concave ice crystals than for ice spheres with the same r_{VA}. This is indeed what one would expect to see, because (i) for a given value of r_{VA}, r_{VP} is larger for concave particles than for spheres and (ii) it is well known that the co-albedo generally increases with increasing particle size, when the particles are large compared to the wavelength.

Therefore, we adhere to our view that it is, in general, better to use the projected surface area rather than the total surface area in the definition of the effective size of nonspherical articles, but we also note that concavities and hollows do play a role.

Change in the manuscript: The following will be stated in the revised manuscript: "While the SSPs of non-spherical particles (including snow grains) can be influenced by concavities and hollows (Grenfell et al. 2005), in general, the most relevant measure of their size for radiative transfer is the volume-to-projected area equivalent effective radius (e.g., Mitchell 2002) . . . "

Comment: *line 49, eq. 1. Point out that re is inversely proportional to specific surface area (SSA), a quantity that is commonly used in snow radiation work.*

Response: The r_e is inversely proportional both to SSA and to the specific projected area (SPA). For completeness, it is perhaps best to add an equation showing these relationships.

Change in the manuscript: We will note the following: "The r_e is inversely proportional to the snow specific projected area (SPA; projected area per mass) and the specific surface area (SSA; total surface area per mass):

$$r_e = \frac{3}{4\rho_{\text{ice}}\mathrm{SPA}} = \frac{3F}{\rho_{\text{ice}}\mathrm{SSA}} \tag{3}$$

where ρ_{ice} is the density of ice and the fluffiness parameter $F = \mathrm{SSA}/4\mathrm{SPA}$ (Grenfell et al. 2005) is $F = 1$ for convex particles such as spheres and $F > 1$ for concave particles."

Comment: *line 89. Change "retuning the snow grain size" to "increasing the snow grain size (of the nonspherical grains)"*

Response and change: This will be reworded according to the suggestion.

Comment: *line 96. "model model" is redundant.*

Response and change: This will be corrected.

Comment: *line 183. "abundant snow cover … in parts of Tibet … ". What does NorESM predict for snow cover and snow depth in Tibet? In reality, Tibetan snow is patchy and thin, with average depth peaking in February at only 2 cm (Flanner and Zender 2005, Figure 3b).*

Response: It is indeed true that NorESM overestimates the amount of snow in Tibet. The snow cover fraction in February is close to 80%, as compared with 30% in the NOAA SCE CDR (aka. Rutgers University) data, resulting in the distinct overestimate seen in Fig. 8a-b. The area-mean snow depth in February for the region considered by Flanner and Zender (2005) is about 25 cm in SPH and 31 cm in NONSPH. Incidentally, while overestimated snow cover likely exaggerates the "radiative forcing" associated with changed snow grain size, it is not obvious that overestimated snow depth works in the same direction. In fact, Fig. 1c of our manuscript indicates that snow grain shape has a larger effect on snow broadband albedo when the snow layer is relatively thin.

Change in the manuscript: We will add the following in Sect. 4.1 (as a footnote, to avoid the disruption of the flow of the main text): "The RF in Tibet may be exaggerated by NorESM's overestimation of snow cover in Tibet (see Fig. 8a–b below)." In addition, the snow depth will be mentioned in Sect. 4.3.1 where snow-related quantities are compared with observations. "… overestimation in Tibet (Fig. 8a–b), where snow depth is also overestimated (the February mean snow depth for the Tibetan Plateau region (30–40°N, 80–100°E) being 25 cm for SPH and 31 cm for NONSPH, as

compared with roughly 10 cm for satellite microwave-derived data and only 2 cm for in situ data, see Fig. 3b in Flanner et al. (2005))".

Comment: *line 185-186. "in the southern parts of northern Eurasia … the change in snow albedo is largely masked by forests." This is also seen in a band of forest across North America at 50-60N between the Great Plains and the tundra.*

Response and change: This is true and will be noted it in the revised manuscript.

Comment: *line 209-210. Define "Q-flux".*

Response: The physical meaning of Q-fluxes is explained in connection with the description of the mixed-layer ocean model in Sect. 2 (lines 105–106 of the original manuscript): "The Q-flux (representing the implied horizontal and vertical heat flux into/out of the local mixed-layer column)… ", so there is no need to repeat this explanation in Sect 4.2.

Change in the manuscript: For clarity, we will add a reference to Sect. 2 at the point where Q-fluxes are mentioned in Sect 4.2. "… especially because the Q-fluxes employed in the slab ocean model are based on a preindustrial simulation (see Sect. 2)".

Comment: *line 296. Change "Figs. 8 and Fig. 9" to "Figs. 8 and 9".*

Response and change: This will be changed as suggested.

Comment: *line 303. Change "or" to "of".*

Response and change: This will be corrected.

Comment: *line 357. Change "NONSPH" TO "SPH". This is important.*

Response and change: Thanks for spotting this! It will be corrected.

Comment: *line 372-373. "2 W m-2 in eastern Greenland (mainly due to BC)". This is probably excessive. The BC content at East Greenland AWS stations is only 2-4 ppb (Table 6 of Doherty et al. 2010).*

Response: We compared simulated surface-layer BC concentrations in the SPH experiment to the observations listed in Table 6 of Doherty et al. (2010). This comparison indicated a slight overestimation for the spring measurements (simulated BC concentrations 2–10 ppb, observed 2–7 ppb) and a more pronounced overestimation for the summer measurements (simulated BC concentrations 7–23 ppb, observed 1–20 ppb but mostly 1–4 ppb). However, it may be noted that the summer measurements are mostly at different sites than the spring measurements, and in fact no measurements are available in the region where the NorESM aerosol radiative effect is maximum. In that region, the simulated BC concentrations in surface snow in summer were as high as $\sim$30–40 ppb, which is very probably too much, even if some enrichment of BC in the surface snow is likely to happen during the snow melt season also in reality.

Change in the manuscript: In the interest of brevity, we will only add the following sentence regarding this issue in the revised manuscript: "The RE in Greenland may

be excessive, as comparison with observed BC concentrations in Greenland (Table 6 in Doherty et al. 2010) suggested that NorESM likely overestimates the BC in surface snow especially in summer."

Comment: *line 441. Change "in lack of information" to "because of the lack of information".*

Response and change: This will be corrected as suggested.

Comments: *line 447. "indistinguishable" is misspelled.*

line 516. Change "report" to "reports".

line 603. Change "run" to "ran".

Response and change: These typos will be corrected.

Comment: *Figure 3 caption line 3. Change "limit" to "threshold". Also on captions to Figures 5 and 6.*

Response and change: Yes, "threshold" is a better word here. This also applies to captions to Figs. 7 and 12. These will be corrected as suggested.

Comment: *Figure 7a. Give units on scale bar (probably micrometers).*

Response and change: We will add the units (μm) to the panel title of Fig. 7a (it is technically easier, and more consistent with our other figures).

Comment: *Figure 7b. A ratio (rather than percent-difference) might be easier for the reader to interpret. Also in Figure 11a.*

Response and change: We will modify these figures as suggested and update the wording accordingly.

Comment: *Figure 13 caption last line. These numbers will be easier to compare if they are given in the same units: "(24 ppb for hydrophilic BC and 8120 ppb for dust)".*

Response and change: This will be changed as suggested.

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