Third review of « A generalized photon-tracking approach to simulate spectral snow albedo and transmissivity using X-ray microtomography and geometric optics », by Theodore Lechter et al.

General comments

I thank the authors for the significant effort made to revise the manuscript, in particular for the critical discussion on the meaning of scattering coefficient for a porous medium, and for changing their method to compute this quantity from snow samples. I believe the updated discussion is very valuable for the overall quality of the paper and raises issues that would need to be considered in the future, even though I fully agree this is beyond the scope of the present study. The paper can now be published in *The Cryosphere*. Below some final technical issues are pointed out, that can be easily fixed. The most critical is about accounting or not for diffraction when computing the scattering coefficient and *g* (the choice should be consistent).

Technical comments (lines correspond to the track changes version)

l.87: do not change paragraph
Response: We agree and have updated text accordingly.

l.91: idem, include all this in the same first paragraph of the section
Response: Same as previous response.

l.104: "multiple scattering within the two-phase medium" is unclear here. Consider removing that, because independent scattering mostly refers to "no interference"
Response: Done, thank you for the suggestion.

l.107 the subscript "ext" should not be italic. True everywhere, and for "sca", "ice" etc.
Response: Thank you, we have fixed the subscripts throughout.

l.107-109: the variables for the physical quantities should not be in parenthesis
Response: These have also been fixed throughout.

l.139: weird to see again Snell's and Fresnel's laws (introduced a few lines earlier). Maybe move to a few lines earlier. If the manuscript is too long, these few formulas (known to most of the readers) could also be put in an Appendix.
Response: We have reworded such that the first paragraph is kept as a more general overview and the second paragraph explicitly states the laws and introduces the equations.

l.163: does d_i includes the lengthening of paths due to internal reflections (effect of B)? Or is it the length of the straight line between scattering events? The text suggests option 1, is it what you meant and what is done by Randrianalisoa and Baillis (2010)? I think the usual definition of extinction coefficient would rather use the straight line between scattering events (e.g. Eq. 7 of Malinka, 2014).

Response: Considering that there are several different methods for determining the scattering coefficient from microCT and ray-tracing methods, it's not clear that there is a "usual" or universally accepted method for determining the extinction coefficient. We choose to use the Randrianalisoa and Baillis (2010) method here because it a) fits best within the ray-tracing methods used in the study, and b) produced values most consistent with the theoretical rho*SSA/4 out of all the different methods we tested. Accordingly, we would like to retain the current method of computing the extinction coefficient and continue to pursue differences in various methods as part of ongoing research. To that end, this method would include the impact of path lengthening to a degree, as the scattering coefficient is equal to the TOTAL pathlength / number of scattering events, where each "particle" results in a single scattering event, whether it be reflection at the particle surface, or transmission through the far-side of the particle.

l.180: not clear how B and F_{ice} (why not just B and density?) are then used. Clarify here or later (in the RT code) when more relevant

Response: We think it is appropriate to include the path-length extension with the absorption in this framework due largely to the finding that the Randrianalisoa and Baillis (2010) method fits expected theory well. We also can show that for the examples in this paper, there is minimal impact to the results if a path-length extension is not used. For clarity, we have removed this specific reference to how B and Fice are used on l.180, and instead focus on a detailed description of how absorption is computed in the model in the section 2.2 where medium model is described.

l.212: the reference should be in parenthesis
Response: Corrected, thank you.

l.235: diffuse radiation means isotropic which is not random, it corresponds to a well-defined angular distribution
Response: Thank you for this clarification, we have reworded this to try and be more specific ->

"This initial direction can be prescribed as downward pointing with a uniformly random zenith and azimuth angles representing isotropic diffuse radiation, or ..."

Figure 2: panel a is more a cumulated distribution than a distribution, no? The suptitle is unclear, consider removing it. Also, maybe put the sample first (a) and the optical properties then (switch a and b). In the definition of g isn't the phase function missing? Important point: it seems that diffraction is included in the computation of g, hence considered as scattering. But it is apparently not included in the estimation of the scattering coefficient which relies on geometrical paths. Can you clarify this, and update the estimation of g (using only the geometrical part) if needed. Although it may not impact your results since you don't directly use g, but the full phase function instead.

Response: We have modified the figure according to these suggestions. THANK YOU for noticing that our definition for g was missing the phase function, we completely missed that mistake during proof reading. We chose to include diffraction in representing g for the following reasons: a) We can't recall ever seeing a geometric-only asymmetry parameter reported in the literature and we think that including diffraction in the computation places the values in better context with current literature, b) By including a "full" asymmetry parameter, the model optical properties could be more easily used in other two-stream RT models (e.g., TARTES), and c) As you alluded to in your comment, we do not use g in any of the other model components, so including diffraction as part of its computation does not improperly mix assumptions in the model. For clarity, we have modified our tables to include both the geometric and full "g" parameters.

l.261 : in snow with
Response: Done

l.430: should be “ext”, and SSA should not be italic
Response: Done

l.480: larger than
Response: Done

l.490: have a
Response: Done