Second 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

Much effort have been made to improve the scientific quality and the clarity of the manuscript, which has been significantly revised compared to the initial version. New figures have been appended, and many others were updated. This is appreciated and certainly makes this study more convincing. However I believe an additional effort is still required to make it an even more significant and scientifically robust contribution. In particular, issues that have been partly tackled in the response to reviewers should be further tackled and included in the paper. These issues are detailed below.

Specific comments

1) The most questioning remaining issue concerns the definition of extinction coefficient for a porous medium. It is explained in the text (and in more details in the response to reviewers) that the computed extinction coefficient does not perfectly match the expected dependence (from theory on particulate media) on SSA and density. This is attributed to the way extinction coefficient is computed, with photons launched from anywhere in the medium, and not only in the air (which is what is implicitly done when launching photons on an isolated ice particle in air). I believe this raises a very important question about the definition (if only it means anything) of the extinction coefficient for a porous medium. In particular with this method the authors find for a collection of spheres a value of the extinction coefficient different than the commonly accepted value. What is the meaning of having different extinction coefficients, when such a quantity should be intrinsic to the medium? This critical question cannot be overlooked, otherwise your method could be erroneously replicated by others. My guess is that defining extinction coefficient and phase function (and probably absorption along extinction paths along with those two quantities) for a porous medium is not univocal, and definitely not trivial. Clearly there exist alternative strategies that result in different values. However I think that for radiative transfer what matters is the product $\gamma_{ext}\cdot(1\text{-}g)$, which is probably the same for any strategy (also explaining why the computed albedos satisfactorily match). Hence I would recommend to estimate γ_{ext} in the common way (launching photons from the air phase only) and to compute the phase function using the deviation between entrance angle (into the ice phase) and final escape angle (after some internal reflections). Applying this to a collection of spheres would help check whether accepted values are retrieved for the phase function (or g) and extinction coefficient, and whether both strategies are indeed equivalent for radiative transfer. Actually you can already check whether $\gamma_{ext}\cdot(1\text{-}g)$ obtained with your method for the collection of spheres matches the “particle approach” value. This is certainly beyond the initial scope of this paper, but the strategy proposed cannot really be accepted before it is proved that it replicates exactly (regarding this, care should be taken to ensure that the obtained match is convincing, which was not the case in the new figure of spectral albedos provided in the response to reviewers) what is expected for a collection of spheres. Tackling this issue would strongly support the presented strategy, and would be a major contribution to radiative transfer in snow (actually in any weakly absorbing porous medium).

2) Some figures (e.g. spectral albedo in Fig. 13 (now Fig. 10)) and values (e.g. Table 1) have changed between both versions, while they apparently correspond to the same measurements, which is puzzling. It would be helpful to explain these differences (at least for the reviewers).

Technical comments (lines correspond to the track changes version)

l.16: the RMSE value might not be meaningful here, because it depends on the number of samples considered, on the characteristics of snow, on, the spectral range of the measurements… which are not detailed. Also, reflectance is rather “measured with a spectroradiometer” than “estimated”.

l.92: this long paragraph is surprising because it already contains much information, especially about the second component of the model. Shouldn’t a large part of it (after l.98) be moved to the subsections?

l.95: should the term “ray-tracing” or something equivalent appear here? Actually both models are somehow based on ray-tracing

l.113: could you expand on why “the semi-quantized approach described here reduces the number of photons required to achieve a statistically robust result”

l.116: “in this model”: is it in the first or second component that phase and diffraction are ignored? I’d rather say in the first component, since the second component just takes as inputs statistically representative single scattering properties, no matter where they come from

l.136: isn’t “a photon of light” redundant?

l.140: ice-path fraction or mean path fraction traveled within ice? Maybe chose a single consistent term.

l.143: the choice of starting anywhere in the medium (ice or air) seems (according to the author comments) to be the reason for not matching the expected relationship between extinction coefficient, SSA and density. Starting the paths only in the air would definitely change the obtained extinction coefficient (and the phase function accordingly). So it raises a fundamental question about how to define the extinction coefficient of a porous medium, which may be an ill-posed question. See specific comment 1).

l.215: consider providing here (or in the caption of Figure 2) the number of photons used to compute these statistics. Also the exponential fit does not seem very convincing. Discussing errors in this fitting procedure (fitting for instance the log of the POE) would be useful. To which extend could this uncertainty of the fit explain discrepancies with the usual dependency on density and SSA?

Figure 1: a) ke in the legend should have a unit. By the way what is ke? In the Figure caption refer to a, b, c, d. c) what is the dashed red line? d) 1000 nm should be in the suptitle rather than in the legend

l.260: still, I think this Eq. (17) implies that the total physical length traveled by the photons is the sum of the s segments, while due to the internal reflections quantified by the B parameter a longer total distance is traveled, so that s should be scaled accordingly

l.280: Figure 2

l.339: one tenth rather than 10 times?

Figure 5 caption: different types of what?

Tables 1 and 2: could the g values (of the computed phase function) be indicated as well?

l.405: is it obvious what an exponential increase means? Maybe provide the functional form of the dependence

l.412: the definition of transmittance is ambiguous here. “Within a snowpack” suggests that the snowpack is thick and that the downward flux is estimated at an intermediate depth, rather than at the bottom of the snowpack. Please clarify this, because both quantities (e.g. flux below a 5 cm layer and flux at 5 cm depth in a thick layer) are very different.

Table 2: depth should start at 0 at the surface no?

l.474: it would be worth commenting the fact that the scaling of Eq. (21) is not 0.5 as would be expected from the studies cited above

l.477: not clear why rounded grains are supposed to have highest B values. Spherical particles have a low B compared to fresh, supposedly fractal snow

l481: as both γ_{ext} and F_{ice} depend on density, what is the meaning of varying them independently?

Figure 13: extinction coefficient should have units mm^{-1}. What do the points correspond to?

l.496: was it clearly stated how F_{ice} depends on B (when density is fixed)?

l.531: it should be made clear that the refractive index is taken constant only for the optical properties computations, not for the absorption modeling

l.532: another point regarding the MIR is that radiative transfer should not be applied this way to dense media (shadowing effects are ignored while they are obvious when snow grains touch each other). It’s actually a chance that the dilute media theory applies that well to snow (see Kokhanovsky (2004) for more details) but this holds only as long as snow is weakly absorbing, which is not the case anymore beyond 1400 nm. Likewise, surface features (the topmost few mm) matter in this spectral range, while they are probably not captured by manual measurements of snow.

l.546: the term Monte Carlo is ambiguous because to me the estimation of the optical properties is also based on a Monte Carlo method (ray-tracing with various possible paths)

l.556: it’s not clear why this work would help with subnivean hazards. In the introduction or here it would be nice to clarify what kind of features could be seen (or not) through the snow, and why knowing the snow transmittance can help

Reference:

Kokhanovsky, A. A. (2004). *Light scattering media optics*. Springer Science & Business Media.