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The Cryosphere Discussions

Interactive comment on "Quantifying bioalbedo: A new physically-based model and critique of empirical methods for characterizing biological influence on ice and snow albedo" *by* Joseph M Cook et al.

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This is well written and clear paper that presents a new optical property model/optical property look up table for biological contituents for the snow radiative transfer model SNICAR. I believe this is the novelty and strength of this paper, and the reason I am suggesting major revision is that I believe the authors should focus on this aspect as opposed to in length 'review' they give of general snow spectroscopy/radiative transfer.

Although it is true that field spectroscopy measurements (and the understanding of

snow radiative transfer) do present uncertainty, the authors themselves do not actually present any spectral (or broadband) measurements made in the field, which is puzzling. It is not clear to me if they tried, and felt they were subject to too much uncertainty to publish, or if they did not try because they assumed the uncertainties in these measurements were too large, or maybe the measurements didn't validate the model well enough- whichever it was, the jump from the theoretical radiative transfer modeling for bioalbedo to the general review to snow spectroscopy/optics/terminology feels like two different papers. They state this section is 'a critique of empirical bioalbedo studies' which is not really the case. I can appreciate the desire to point out to others in the research community these uncertainties, but it seems out of place, is not an exhaustive enough review as to act as an only resource for readers, and all of the information in sections 4.1, 4.2, 4.3, 4.8, and 4.10 can be found in a wide range of publications over multiple decades. I suggest the authors point to the numerous papers on general snow albedo and radiative transfer, and make the paper more concise and novel. I do think this is a good paper, but it could be improved by focusing the uncertainty discussion on those uncertainties that actually pertain to the adaption of SNICAR to model bioalbedo (sections 4.4, 4.5, 4.6, 4.7, 4.9).

I have a few more general comments, and minor line by line comments are below. -The authors should discuss in more detail how this paper varies from Cook et al. 2017, 'A predictive model for the spectral "bioalbedo" of snow'. -Since no field measurements are presented to validate the outputs, it should be made clear that this is a purely theoretical model. Thereby, I would be cautious in generically stating that this can extend ground measurements to remote sensing measurements. Remote sensing of reduction in snow albedo due specifically to particulates is difficult due to lacking spectral/spatial/and-or temporal resolution and has only been carried out successfully in very heavily contaminated regions from spaceborne platforms (I refer you to Warren, 2013(doi:10.1029/2012JD018476))- and even with those retrievals there are subject to high uncertainties. It is different with airborne imaging spectroscopy, but those flights (like the one used by Painter et al., 2001, Painter et al., 2013, and Seidel et al., 2016)

tend to be 'one-off' flights. -Is biological matter on snow and ice actually an impurity? This has been used commonly by the light absorbing community in the past, including myself, but it has begun to move away from it. I would consider using another term, like particulate or constituent.

Line by line:

P1, 16- Has this been recognized? I'm not sure. Also, I would generally avoid using the word significant unless there are statistics to back it up.

P1, 41- There are many more, and perhaps better, citations than the ones used- namely the 2007 and 2013 IPCC reports and Hansen and Nazarenko, 2004

P2, 6- High Sierra –> Sierra Nevada

P2, 7- The retrieval is an optically equivalent grain size.

P2, 37- Can you please provide an estimate of area to back up 'wide spatial coverage'? The Greenland Ice Sheet is massive, and the dark snow/transition zone where biological constituents are found, and impact albedo in a meaningful way, is only along ice sheet margins. (the albedo map here - http://nsidc.org/greenland-today/2013/06/springtime-melt-in-greenland-late-start-rapid-spread/ - comes to mind)

P3, 9- dusts –> mineral dust or simply 'dust'

P3, 12-14- Reword this sentence for clarity. The optical properties of the ice grains mostly matter in the NIR where ice is absorptive, in the visible wavelengths the vertical distribution matters because ice absorption is very weak and scattering is high- which means light can penetrate the snow pack. Still impurities only impact albedo in the near surface (maybe at maximum 30 cm, I have found it to be closer to 6-10 cm for aging snow in the presence of dust) because the imaginary index refraction increases across the visible spectrum.

P3, 18-20- Physical modeling of snow radiative transfer has been applied to remote

sensing retrievals over snow and ice for decades- I'm not sure what you are saying here.

P3, 41- A good citation here would be Kaspari et al., 2015 (10.1002/2014JD022676). But my response to this was- are they well-validated for contaminated snow? Or do they just decently approximate in a few instances? I would revisit these studies to see if they actually validate the model in the presence of black carbon and/or dust (Painter et al., 2007 estimated the partitioning between dust and black carbon, and the black carbon concentrations used were far far above what we have ever actually measured at that site)? The fact that SNICAR could not represent snow albedo in the present of dust prompted the development of a method to retrieve the optical properties of dust in snow, see Skiles et al., 2016 (doi: 10.1017/jog.2016.126).

P4, 8- This review (a generous term in regards to black carbon and dust) is over seven years old in a rapidly advancing field, certainly there are more up to date citations in addition to Gardner and Sharp? Just to point out they use black carbon concentrations from Hansen and Nazarenko, 2004 after which there has been an explosion of studies documenting black carbon concentrations, and the impact of dust and black carbon on snow albedo.

P4,12- Be explicit about what you mean here in terms of the optics

P5,5- Does this matter for snow albedo? Is not included because SNICAR is only over the solar wavelengths? If there is a reason you felt it important to mention this, support the statement.

P5, $\sim$14- I think it is worth noting at the end of the description here that similar methodology has been used to infer the imaginary part of refractive index of sea-ice sediment by Light et al (1998), for hydrated salt crystals in sea ice by Carns et al. (2016), and for dust in snow by Skiles et al. (2016).

Light B, Eicken H, Maykut G and Grenfell T (1998) The effect of included particulates

on the spectral albedo of sea ice. J. Geophys. Res., 103(C12), 27739–27752

Carns RC, Light B and Warren SG (2016) The spectral albedo of sea ice and salt crusts on the tropical ocean of Snowball Earth: II. Optical modeling. J. Geophys. Res.: Oceans, 121(7), 5217– 5230

P5, 21- I thought there were four options for spectral irradiance in SNICAR? Midlatitude clear or cloudy and then Greenland clear or cloudy.

P5, 25- These are different from the default look up table in SNICAR, which has the same range of radii? Can you give more details about how these were produced?

I am refraining from giving comments for section 4.1, the general theme was that you can find this information well documented elsewhere, namely in Schaepman-Strub et al., 2006

P7, 27- If you keep this section, again I think the information can be found in many other places, I think it might be worth mentioning here that TARTES offers a range of options for grain size/shape

P7, 35- I think you should explicitly make it clear that observable grain size is very different than optical grain size- observable grain sizes will lead to high errors because they represent what we see, not the absorbing path length

Section 4.3 I suggest removing

P9,28- Are these observations, or just model results? The plots in Warren and Wiscombe, 1980 are model results- and it makes sense yours would agree with those and with Flanner, 2007 because it is the same basic theory. I would suggest reading 3.2.2 of Skiles et al., 2016 where we found that SNICAR did model the snowpack this way, but observations do not show this effect (full spectal albedo time series can be found in Skiles and Painter, 2016 (doi:10.1017/jog.2016.125)), even under extremely heaving dust loading. If biological constituents are heterogeneously mixed among snow grains, ice absorption may still be dominant even with additional biomass.

P10, 35- Can you justify why it may be important to isolate the bioalbedo here, rather than later in the section? In the context of climate and hydrologic modeling it is the integrated impact of all impurities that matters, so it is helpful to point out why you may want to separate the unique reduction in albedo due to just biological constituents.

p11, 15- It is important to point out here that dust optical properties are highly variable, and SNICAR uses by default a 'global mean' dust optical property representation- you also only specified dust in the largest particle size bin in your runs, which is the least absorbing and probably an unrealistic representation of the dust particle size distribution anywhere... also, what about black carbon? It seems to be missing in this discussion, yet studies have found that in relatively high concentrations at the ice sheet edges.

Section 4.8 I suggest removing, information also found elsewhere. if it stays, it seems very out of place here and should be up with other radiative transfer stuff. Also, it is possible to set up an experiment to measure this if you have a spectrometer. If you are truly interested in this I recommend reading Tom Painter's dissertation (UCSB, 2000).

P11, 38- Clarify and give a citation for 'preferentially melt smaller grains with larger SSA's'

P11, 40- Albedo is also reduced in the NIR

Section 4.10 I would suggest removing, it is out place, especially since you do not present albedo measurements in the paper. And the gist of you paper is to model the spectral shape of snow albedo containing biological constituents, or in turn, inverting spectral reflectance to back out biological constituents, so it is unclear to me in what case broadband albedo would be sufficient in this context. You also seem to represent hemispherical reflectance as a 'decent' approximation of albedo, which is not the case, a model is required to go from a reflectance measurement to an albedo (this is described a number of places- I described it recently in Casey et al., 2017 (doi: 10.1002/2016JD026418) .

Figures-

Please make sure all of the information in figure captions is also in the paper (methods, etc.)

Figure 1: I would keep part B, and refer readers to papers containing other figures

Figure 5: Unnecessary, would be more appropriate if you had presented albedo measurements in this paper.

Metadata sheets would be more appropriately included if you had presented albedo measurements in this paper.

Please do not hesitate to get in touch with questions or comments- McKenzie Skiles, m.skiles@geog.utah.edu

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