The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC1
https://doi.org/10.5194/tc-2021-366-RC1, 2022

Comment on tc-2021-366

Anonymous Referee #1

Referee comment on "The effects of surface roughness on the calculated, spectral, conical–conical reflectance factor as an alternative to the bidirectional reflectance distribution function of bare sea ice" by Maxim L. Lamare et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-366-RC1, 2022

Within the manuscript, the influence of the surface roughness on the reflectance anisotropy of bare sea ice is simulated for different wavelengths, illumination conditions, and sea ice thicknesses. For this purpose, the authors employ the radiative transfer model PlanarRad. From this, the size and shape of the forward scattering maximum is discussed for different types of sea ice (first-year, multi-year, melting sea ice).

This study marks an important contribution for the remote sensing community, as different retrieval products from aircraft and satellites rely on an accurate knowledge of the multiangular reflectance of sea ice, which so far has been underrepresented in the literature. The retrieval products in need for a better representation of the surface reflectance anisotropy most notably include surface energy budget observations, but also include atmospheric retrievals that rely on surface reflectance corrections (e.g. cloud retrievals).

The manuscript is concise and the figures are generally of a good quality. However, there are some aspects that need further attention in my opinion. After some general comments, the more specific comments and suggestions for technical corrections follow below.

General Comments

(1) Nomenclature of reflectance quantities: The manuscript is missing a rigorous and consistent usage of reflectance terms. Even though BRDF and BRF are defined in Section 2.1, the usage throughout the text is inconsistent and, in some cases, erroneous. For example, the title indicates a study focused on the BRDF. Instead, due to the limited angular resolution in the viewing geometry, directional-conical results are presented

(compare Schaepman-Strub et al., 2006). Other instances include, e.g. P2L19 (BRF stands for Bidirectional reflectance factor, and BRF is not really an approximation of BRDF, it relates the reflected radiant flux of a sample surface to the radiant flux of an ideal Lambertian surface irradiated under the same conditions, as you define yourself in Sect. 2.1), P2L28 and P3L2 (the BRDF can never be measured), and P2L31(what is an isotropic albedo?). I understand that using BRDF is somewhat established in the literature, however I think the authors need to be more careful and follow the recommendations put forward by Schaepman-Strub et al. (2006) to make an effort to improve the usage of reflectance terms in the literature. This is not limited to the above-mentioned cases, and I suggest the authors check again their usage of terms throughout the manuscript (including the introduction when discussing other studies). You also need to mention that you are not simulating a BRF, but an approximation. If you call it a BRF you are assuming the reflected radiance is constant throughout the viewing quads.

(2) Introduction: In my opinion, the authors need to elaborate more on the references used in the Introduction. Long lists of references are given for a specific point of discussion, but simply mentioning the reference is not enough. For example, not just mention wavelength (….), but actually mention what the current state of the art is regarding wavelength-dependence of the BRDF/HDRF. So far, the introduction only mentions that different effects on the reflectance anisotropy have been measured before, but these effects are not described at all yet. For a proper state-of-the-art overview, more details need to be given already here. This will also help later to put the results of this study in perspective.

(3) Results: The structure of the results section needs adjustments, as currently the storyline is hard to follow as effects of roughness, thickness, wavelength and solar zenith angle are mixed throughout the different subsections. In addition, the separation of the Nadir BRF results complicates things in my eyes, as this could be discussed together with the 2D BRF. My suggestion would be: 3.1 Roughness and sea ice thickness (showing Figures 4-6, but maybe even switching the order of Figures 5 and 4), 3.2 Roughness and solar zenith angle (Fig. 7), 3.3 Roughness and wavelength (Fig. 8). That would make the structure easier to follow.

(4) Related to the last comment, Section 3.2.1 starts describing Figure 5 again as if it is mentioned for the first time in the text. I recommend to restructure and discuss the effect of the thickness already together with the effect of the roughness. First half of 3.2.1 actually discusses roughness again from earlier as well. The influence of the sea ice thickness shows the influence of the underlying surface, i.e. the ocean BRF. Thus, the values are lower as you clearly demonstrate. However, I think it is worth noting in the text that the shape of the BRF itself remains unaltered, meaning the shape and size of the forward scattering peak seems similar for all sea ice thicknesses independent of the sea ice type.

(5) The authors use the word ‘quasi-infinite’ in the figures, but ‘optically thick’ throughout the text. If the more commonly used term ‘semi-infinite’ would be used consistently, it would make the text much easier to read, and also put the text more in line with the Lamare et al. (2016) terminology. If the authors had any specific reason to name it differently, this should be emphasized more.

(6) I agree with the authors that studying the semi-infinite (optically thick) sea ice is important to understand the intrinsic surface BRDF of bare sea ice. However, I feel the study could benefit from including a figure looking at the spectral and solar zenith angle dependence of the BRF for another sea ice thickness that is closer to natural sea ice (as melting sea ice with a thickness of 20 m is more a theoretical consideration). I believe seeing the effects of wavelength and illumination angle on both the theoretical (semi-infinite) and the more realistic (e.g. 50 cm/100 cm) thicknesses would be of interest to many readers. Adding an additional figure could work (maybe restricting to only one roughness value at the other thicknesses), or maybe also adding another column for an additional sea ice thickness in Figs. 7 and 8 could be an option.

(7) Section 4.2: This is a very important section that puts the choice of the modeled roughness parameter in perspective. However, this should be included in the Methods section already, when mentioning the range of modeled roughness parameters at the end of Sect. 2.2. I also suggest the authors consider making the roughness considerations a separate subsection within the methods, e.g. after 2.2 Model description. As this is a vital part of the study, it should have a separate section in the Methods, and the choice of roughness parameters needs to be motivated already at this point. At the moment, 4.2 seems a bit out of place in the discussion.

(8) Please be consistent about the usage of the roughness parameter sigma throughout the manuscript. You first state it is unitless, but then give ‘m’ as a unit on several occasions, e.g. captions of Figs. 3 and 4, or on P6L23.

(9) When you mention specific values of the BRF or changes in %, you give values with 2 or even 3 decimal places, which is an accuracy not needed (with respect to the mentioned model limitations) and makes reading the numbers quite cumbersome. In my view, rounding the values to integer numbers seems to be more than sufficient.

Specific Comments

Title: In addition to using the correct reflectance term in the title (see general comments above), I suggest to include modeled/simulated in the title as well so that the reader immediately knows what to expect from the study.

Abstract: the mentioning of quads is not clear as this is not a commonly known term. You either need to define it or express the point you want to make differently. In addition, the angular resolution should be mentioned directly.

P2L8: please be more accurate with the definition of BRDF at this first instance, it does not describe the relation between illumination and viewing angles, but of the incident and reflected radiation of all sets of illumination and viewing angles.

P2L10: add Schaepman-Strub et al. (2006) reference already at this point

P3L14: please avoid statements of novelty in that way

P6L17: add 'between … nm wavelength', as otherwise it could sound like these are the chosen ice thickness values

Fig. 4: the different lines are hard to distinguish, especially for the different roughness values for the quasi-infinite cases

Sect. 3.1.1, strongest relative change in nadir BRF for melting sea ice. As far as I understand it, that is to be expected due to the much larger semi-infinite sea ice thickness of about 20 m. I think the authors should elaborate a bit on this and include more details on how these quantities were calculated in Lamare et al. (2016). P6L8 indicates that you calculated the e-folding depth times 3 or 5. If I compare to Table 1 in Lamare et al. (2016), it seems like you chose factor 5 for this study. If that is correct, I wonder why you chose 5 instead of 3, is this a wavelength consideration? I am not saying factor 5 is worse than 3, I just feel the authors need to elaborate a bit more, as this influences the discussion of the relative changes with respect to the sea ice thickness in Sect. 3.1.1.

P7L9: please also include a more recent reference for this statement, as this is the central motivation of this study.

P7L15: the second part of 3.1.2 needs rewriting, as it is very hard to follow how the authors describe Fig. 6b. In addition, what in the text is referred to as Figure 6 is actually Figure 5 (P7L9), whereas Figure 6b in the text should be Figure 6.

P8L22: 'however the intensity of peak increases with θi'. It is a bit difficult to follow this claim looking at Fig. 7, as at first glance the colors look the same, because the ranges of the color bars are not the same. I would suggest finding a different and consistent color bar. However, if the wide range of BRFs makes that too difficult, I think the authors should mention in the text already that the reader should pay attention to the varying ranges in the color bars of the respective sea ice types.

P9L6: 'However, the BRF does not decrease uniformly over the hemisphere with an increasing wavelength.’ I suggest introducing Fig. 8 only after this sentence, to use the first sentences as an introduction and then describe the results of the figure. This would help the reading flow, as currently after introducing Fig. 8 the authors describe earlier results from other figures again.

P9L13: decreases by 13.94% compared to what?

P11L1: Both Manninen et al. (2021) and Carlsen et al. (2020) study the influence of surface roughness on the BRDF of snow and the consequences for the calculation of the surface albedo and satellite retrievals, respectively. Manninen et al. (2021) from a modelling point of view, Carlsen et al. (2020) more from an observational side. Even though both studies investigate snow surfaces, the effects they are reporting are relevant for this study. However, the authors should elaborate more on how this relates to their results and give a bit more background when putting their study into perspective rather than just mentioning them. For example, the MODIS MCD43 product is never explained, and some readers might not know what it is.

Section 4.3: Thanks for having made the model output available. It would be of interest for future users if you could mention at this point the increase in computational time necessary to increase the angular resolution of the simulations.

Section 4.3: Second paragraph about the intrinsic surface BRDF. This is as a first approximation true, however, the authors mentioned earlier themselves that the model also only considers direct and no diffuse illumination. However, the scene observed by the satellite is illuminated by both direct and diffuse radiation. So only propagating the surface BRF to the TOA is not entirely sufficient. Please also mention it again at this part of the manuscript.

P12L25: The entire study focuses on the reduction in uncertainties for the retrieval of e.g. albedo products from remote sensing. The reduction of uncertainties in global climate models comes a bit out of nowhere at this point. Please back that up with a more thorough explanation or leave it out as in my opinion the study does not need that additional motivation, especially as it seems a bit far-fetched in the way it is mentioned right now.

Technical Corrections

P2L8: with

P2L18: sea ice (additional ‘sea’)

P2L22: snow kernels

P2L25: exist for sea ice

P4L4: ideal

Fig. 1: phi symbols in text and figure not the same

Fig. 2: color bar needs adjustments

Fig. 3: in the caption it says sigma = … m, but sigma is a unitless quantity

Figs. 5, 7 and 8: somehow the color bars are upside-down (including the labels)

Fig. 7: the roughness values at the top are flipped, highest is now on the left and lowest on the right (as compared to the other Figures), BRF color bar is also flipped, and not the same throughout the Fig., thus it is hard to compare the different plots.

P12L4: please define/explain HULIS

P12L14: please split that sentence up in two, it is currently very hard to read.

P12L18: same as above, please split that sentence up in two.