Comment on tc-2021-366
Anonymous Referee #2

Referee comment on "The effects of surface roughness on the spectral (300–1400 nm) bidirectional reflectance distribution function (BRDF) of bare sea ice" by Maxim L. Lamare et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-366-RC2, 2022
The authors present a well-defined model study that investigates the effect of sea ice surface roughness on the directional reflectance of bare ice as a function of wavelength, sea ice thickness, and solar zenith angle. The work represents a further important contribution for the understanding of radiative transfer in a polar environment, even though the study shows its limitations. But these limitations are well addressed by the authors.

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Thank you
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General Comments
The study is about bare ice reflection properties. Therefore, I suggest to elaborate a little bit the meaning of this specific surface type in a meteorological context. In the current version, the introduction is mostly related to technical aspects. What is the seasonal contribution of bare ice in polar regions? Introduce the three ice types which are used in Sec. 2. Also give some more background on the surface roughness. What are typical scales?
An entire paragraph is listing snow surface studies without giving any details. However, in my opinion this part could be removed. The authors should focus on a review on bare ice studies dealing with surface reflection properties and their dependencies. A comparison with findings related to snow surfaces should be given in the discussion section.

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Fixed: The introduction will be expanded to introduce the three bare ice types, the scales and surface roughness. The section on snow surface studies will be moved to the discussion and expanded.
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I'm wondering if Section 3 could be restructured. In the current version it's no easy reading. Starting with the roughness and wavelength dependence might help (Fig. 8), without showing the nadir BRF plot (Fig. 4). The main message becomes clear from the contour plots already. Then show and discuss Fig. 5 (roughness and thickness). Based on Figs. 8 and 5 I would introduce the shift and broadening of the scattering peak by presenting Fig. 6. At the end show and discuss the roughness - SZA relation (Fig. 7).

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Fixed: section 3 will be restructured following the outline suggested by referee 1.
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Specific Comments

P1l12: “Different types of sea ice...” A lot of numbers quantifying the roughness effects are given at this part of the abstract. Try to reduce the information since it is a way too much.

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Fixed: The amount of numerical information in the abstract will be reduced.
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P2l2 and other: Omit the term “light” when you talk about solar radiation which is not within the visible wavelength range (400 – 700 nm)

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Fixed. The term light will be removed when not discussing visible electromagnetic radiation, but UV and NIR electromagnetic radiation are still light.
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P2l10: “The BRDF is a directional description of albedo...” The relation is more complex than stated here, be more precise. Also the HDRF should be introduced better than just saying that it is a proxy of the BRDF.

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Fixed. Please see our reply to reviewer 1: A more in-depth discussion about biconical system will be given.
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P2l33: “high sensitivity” – please elaborate

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Fixed: A fuller explanation of what is sensitive to what will be given.
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P3l2: “13 bands” – spectral bands, p3l7: “300 and 4000 nm” add wavelength afterwards, same as on p3l19 to make sure that wavelength is meant here

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Fixed: These typos will be corrected.
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P3l17: “... is required” Because it has not been done yet is not a convincing reason. What kind of consequences do you expect to derive from this study?

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Fixed: Reasons will be included in the manuscript. We note here that the first reviewer gave excellent reasons: “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).“
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Last paragraph of the introduction could be improved to reflect the outline in a better way.

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Fixed: The last paragraph of the introduction will be reworded.
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P3l32 / Fig. 1a: Do you really need Fig. 1a?

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Figure 1a is a quick, clear, and unambiguous method to define the geometry and variables used in our paper. Whilst it is not absolutely essential it does add huge amounts of clarity to the work. Since no reason is given to remove it we have decided to keep it
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Eq. (2) add “=pi*BRDF”

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Fixed.
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When I have understood correctly, the PlanarRad model was designed for aquatic radiative transfer. What justifies its use for calculating sea ice reflection properties? Is it a numerical model? Later (p5l9) Monte Carlo ray tracing is mentioned. Please explain a little more how this is related.

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The description of the model will be reworded to be crystal clear which calculations are performed with the numerical model PlanarRad and which calculations use the ray tracing (roughness calculation). PanarRad was originally written for marine aquatic environments and sea-ice is a marine aquatic environment. The numerical part of the radiative model works in the same way as the model Hydrolight which is a very well-known model in hydrological optics.
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P4l10: “calculation of the BRDF” Actually, the model allows rather the simulation of the biconical reflection. So it’s not a real BR(D)F which is shown in the following plots.

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Fixed: As outlined in the reply to review 1. A fuller more accurate description will be given of our biconical set-up.
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P4l17+Fig. 1b and P4l28+Fig. 2: You could combine both figures as Fig 1a and Fig 1b. P5l3: Here the roughness parameter is without unit, but later and on p4l29 sigma is given in meters. Also in Fig. 3 its given without and with unit. Further, I suggest to move the definition of the roughness parameter to section 2.1.

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Fixed: The roughness parameter should not have units, this will be explained and corrected throughout the manuscript. The roughness definition will be moved as suggested. The figures will not be combined as this will create a weird right-angled figure and occupy more journal space.
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P5l11: “using 10 rays per quad” Could you explain, how is this number selected?

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Fixed: the justification will be explained. It is a balance between reproducible simulation and computational effort. Further increasing the number of rays per quad did not increase the precision of the calculation.
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P5l12: A scale height between 0.1 and 10 cm is chosen here. How do these numbers relate to real roughness features?

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Fixed: The roughness parameter should not have units. A detailed description of the following explanation will be given: Sigma is the elevation (height) standard deviation relative to the horizontal distance between two points on the surface and is unitless. Note the statement at page 5 line 4 is incorrect and will be removed "For example, if σ = 1, the slope between points located 1 mm apart has a standard deviation of 1 mm".
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P5l14: The three sea ice types come somewhat out of the blue. Some more background should be already given in the introduction (see my first general comment).

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Fixed: Our justification for the three sea ices will be introduced and strengthed.
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P5l19: “mass-ratio of 1 ng/g” Did you see any effect of black carbon for this low number? From Marks and King (2014), Figure 3, I don’t expect any significant contribution.

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Fixed: A black carbon ratio of 1 ng g(-1) does has a small effect that is hard to discern on a graph.
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P5l25: “increases by 29.5%...up to 630.7%” I would give only integer numbers (here and elsewhere)

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Fixed: The precision of such numbers will be reduced.
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P7l8: “...calculate the energy budget of the sea ice...” The sentence tries to motivate the direction of investigation. However, you should make this point already in the introduction, where the relation between satellite-based observations of the directional reflection, BRDF, BRF, ice albedo, and energy budget should be given.

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Fixed: Referee 1 requested this argument be removed and it will be.
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P7l32: “moves lower on the hemisphere” – maybe better say that it is shifted to higher viewing zenith angles

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Fixed: The phrase will be reworded as suggested.
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P9l27: “to a larger zenith angle” – better write “viewing zenith angle” here

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Fixed: The phrase will be reworded as suggested.
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P11l1: The comparison with findings for snow surfaces should be extended. Give also quantitative results.

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Fixed: The section on snow surface studies will be moved to the discussion and expanded.
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P11 Section 4.2: I would shift this subsection to the model setup section. Statements related to the surface in general should be part of the introduction.

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Fixed: This text will be reworded and moved to the introduction.
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Technical Corrections
P2l8: “rwith” – with

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Fixed.
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P2l12 and other: insert a space before references “roughness(e.g. Manninen, 1997)” – roughness (e.g. Manninen, 1997)

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Fixed.
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P2l22: “snow kernals” – kernels

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Fixed.
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P2l31: “isotopic” – isotropic; Do you mean a Lambertian assumption here?

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Fixed.
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P3l1 and other: “dependance” – dependence

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Fixed.
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P4l4: “idea” – ideal

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Fixed.
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Figure 3: Please increase the font size of the axis labeling.

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P6l3: “thickness” – ice thickness

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Fixed.
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Figure 4: Please increase the font size of the axis labeling.

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Fixed.
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P6l24: "... due to the large absorption in the ice dominating the signal ..." Sounds strange.

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Fixed. Reworded.
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5: please check color bar labeling

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Fixed.
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7: roughness (sigma) is in wrong order, also check color bar labeling

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Fixed.
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8: roughness (sigma) is in wrong order, also check color bar labeling

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P7l31: "as shown in Figure 6" – wrong reference

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P8l6: "inter-dependant" – inter-dependent

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P8l20: "inter-dependant" – inter-dependent

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P10l12: Start a new paragraph with "Miao et al. (2020) ..."

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