Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-58-RC2, 2020
© Author(s) 2020. This work is distributed under
the Creative Commons Attribution 4.0 License.

Atmospheric Measurement Techniques
Discussions

Open Access

Interactive comment on "On the retrieval of snow grain morphology, the accuracy of simulated reflectance over snow using airborne measurements in the Arctic" *by* Soheila Jafariserajehlou et al.

Anonymous Referee #2

Received and published: 25 August 2020

Within this manuscript, simulations of the bidirectional reflectance factor (BRF) are compared to airborne measurements of the BRF of snow surfaces with the Cloud Absorption Radiometer (CAR) at three different wavelengths in the visible and near infrared wavelength range. The measurements were part of the ARCTAS spring campaign over Alaska in 2008. In order to simulate the BRF correctly with the radiative transfer model SCIATRAN, many input parameters such as snow (e.g., snow grain size and ice crystal shape) and atmospheric parameters (e.g. aerosol optical thickness, ozone concentration) need to be measured or independently retrieved. Subsequently, the BRF simulations utilizing the carefully selected input parameters are compared to the CAR measurements, showing in general a good agreement. In comparing radiative transfer simulations of the combined snow-atmosphere system with actual measurements, this study presents an important contribution that is potentially relevant for passive remote sensing above snow surfaces. The authors efforts to carefully select the input parameters and presenting a retrieval algorithm for ice crystal shape and snow grain size from reflectance measurements should be acknowledged.

The figures are generally of good quality, which helps to convey the arguments of the authors. However, there are some aspects that need further focus in my opinion, After some general comments, the more specific comments and suggestions for technical corrections follow below.

General comments

I have a couple of general comments that need further attention: (1) about the scope of the manuscript, (2) about the reflectance terminology used throughout the manuscript, (3) the use of English, (4) the handling of measurement uncertainties, (5) the definition of the snow grain size.

(1) After reading the manuscript, its scope is still not entirely clear to me: (1) Is it about the description of a novel algorithm for the simultaneous retrieval of the snow grain size and ice crystal shape? In that case, it is a bit confusing to me that you moved the entire description of this algorithm to the appendix, while at the same time, the sentence 'we present a novel two-stage snow grain morphology [. . .] retrieval algorithm' is part of the abstract. If you want to focus more on the description of this algorithm, maybe it is worth to think about moving it to a more prominent spot within the manuscript. (2) Is it about the sensitivity of the radiative transfer model to the snow and atmospheric input parameters? (3) Or is the main focus the comparison of BRF simulations with the CAR measurements? To be clear, I do think that all three parts are important contributions. However, it is important that each part presented in the manuscript is investigated

Printer-friendly version

Discussion paper

thoroughly. And until now, each part is missing some pieces in my point of view and I will give more details on that further below in the specific comments. However, this lack of focus seems to already appear in the title, which reads very confusing and imprecise to me. The authors of course wanted to include all pieces, but this came at the cost of the readability and conciseness.

(2) Another important part is the terminology used throughout the manuscript. As you are referencing Schaepman-Strub et al. (2006) extensively in Sect. 2 'Theoretical background', I recommend you also stay consistent in the use of reflectance terminology. Equation 4 defines a reflectance factor according to Schaepman-Strub et al. (2006) and should be named 'reflectance factor' and not 'reflectance' as stated for example on Page5 Line143. Otherwise, this quickly becomes very confusing to the reader as it is very important to stay precise to differentiate between the different reflectance quantities. I mention some occasions where 'reflectance' should be replaced with 'reflectance factor' below in the Technical corrections. However, the authors should double check and change it in the entire manuscript.

(3) Unfortunately, some parts of the manuscript are quite difficult to read and the use of English should be improved to make the line of argumentation easier to follow. I gave some recommendations in the comments, but I think the authors should check the entire manuscript to foster reading comprehension.

(4) One of the most pressing aspects is the lack of accounting for measurement uncertainties. A detailed discussion of the measurement and retrieval uncertainties for the BRF measurements with the CAR is missing. Also, every time CAR measurements are shown, uncertainty bars need to be included (especially Figures 5 and 6, see also specific comments below). This also applies to the a priori estimation of the effective radius of the snow grains (Figure 7), and the scatter plots in Figure 10. I understand that adding uncertainty bars for the simulations in Figure 3 is not applicable as the plot is already very busy. However, as you even test the sensitivity of the simulations with respect to, e.g. how absorbing the aerosols are, at least some uncertainty estimates

CC BY

should be given within the text. The uncertainty discussion is especially important as it might influence conclusions drawn from the RMSE analysis: if all influencing factors are named and properly quantified, non-significant differences in the RMSE of 0.4 % (Figure 5) between two different ice crystal shapes should not be relevant and influence a decision for a specific ice crystal shape being used in the simulations.

(5) The snow grain size is given in terms of the maximum extent within the manuscript. Although the physical size of a snow grain is traditionally defined by the length of the largest extension of the crystal, in terms of radiative properties the optical-equivalent snow grain size is way more important. It is defined as the radius of a collection of spheres with the same total volume and surface area compared to the actual non-spherical snow grain (see e.g. Grenfell and Warren, 1999; Neshyba et al., 2003). Displaying the reflectance factor for different crystal shapes and sizes in Figure 3, one could assume that the same crystal sizes are comparable between the different shapes. However, from a radiative point of view, this is not true, as each size (largest extent in your case) is defined differently. I recommend using an optical-equivalent snow grain size in Figure 3 instead of the largest extension.

Grenfell, T. C. and Warren, S. G.: Representation of a nonspherical ice particle by a collection of independent spheres for scattering and absorption of radiation, J. Geophys. Res., 104, 31.697–31.709, doi:10.1029/1999JD900496, 1999.

Neshyba, S. P., Grenfell, T. C., and Warren, S. G.: Representation of a nonspherical ice particle by a collection of independent spheres for scattering and absorption of radiation: 2. Hexagonal columns and plates, J. Geophys. Res., 108, Art. No. 4448, doi:10.1029/2002JD003302, 2003.

Specific comments

Abstract: (1) The first 1.5 paragraphs (L10-L19) are too general for an abstract. SCIATRAN is the first really specific information about the study presented in this manuscript on Line 20. Please try to include specific information already earlier and leave some

CC BY

of the general motivation to the section ‘Introduction’. (2) L27: specify the used wavelength channels at this point. (3) L31: round the effective radius to an integer number as the two decimals imply a precision which is not achievable.

P2L41: please add the Arrhenius reference

P5L129: It is very important to list the different atmospheric contributions to the measured radiance. However, please be a bit more precise in the formulation: for example, the scattering by the atmosphere before and after reaching the surface is not removed. More precisely, ‘the contribution of light scattered by the atmosphere both before and after being reflected from the surface’ is removed (see Schaepman-Strub et al., 2006). Please specify the four contributions accordingly, referring to the different contributions of scattered radiation reaching the instrument’s field of view.

P5L134: please already give the CAR wavelength range at this point.

P5L138-149: This paragraph is very important to understand the quantities measured and simulated within this study. However, it is currently difficult to read. I recommend to reformulate the sentences and taking special care with regard to the sentence structure. This comment includes for example: P5L139: is applied to the measured radiances; P5L143: In the simulation [. . .]: this sentence is unclear, please reformulate. P5L147: We assume that the reflectance factor at flight altitude is a good approximation of the BRF just above the surface at infrared wavelengths where atmospheric scattering is negligible; Eq. 4: the subscript ‘0’ should be defined at this point.

Sect. 3: Subheadings would improve the readability considerably. I recommend to start with some more details about the ARCTAS spring campaign, adding a map with the flight tracks of the measurements used in this study, before giving details about the CAR instrument and the ozone and nitrogen dioxide data.

Figure 1: please add the position of the Sun in the caption of the figure to make it immediately clear where the forward and backward scattering directions are.

Figure 2: missing whitespace before 1.649 um in the figure caption. The y axis should be named ‘Reflectance factor’, as this is what you calculate from Eq. 4. Both Figure 1 and 2 should be described in more detail and not only mentioned in the text.

P6L173: You are giving an explanation for the decrease in inhomogeneities in the BRDF data. Please also discuss the increase of the BRF with altitude.

P6L182: In the paragraph describing the AOT data, a quick description of the representativeness of the aerosol conditions during the ARCTAS spring campaign with respect to the Barrow climatology would be helpful.

P6L183: please provide some more details about the spaceborne measurements of total column ozone.

P7L200: I guess the measurement location is sufficiently remote to justify this assumption. However, are there any measurements of black carbon on snow available for this region to further provide evidence for this?

P8L229: please provide more details about the ‘exponential vertical distribution’ used for the vertical profile of the aerosol number density. Are you assuming the aerosol number density is reduced exponentially with height? Is this not influenced by the boundary layer height? And why were 3 km chosen when the measurements were conducted at flight altitudes below 1700 m? Also: for the vertical profiles of pressure and temperature, did you use monthly mean profiles as well or could you make use of radiosonde launches in the vicinity of the study area?

Figure 3: (1) The ice crystal shapes presented in Figure 3 do not match the 9 morphologies introduced on P7L206: it seems you are presenting solid bullet rosettes in the figure, which are not mentioned in the text. On the other hand, you are not presenting the results for the fractal particles. Please clarify that as it is a bit confusing to me. (2) I assume this is still the calculated Reflectance factor, please name the y axis accordingly.

CC BY

P9L264: please specify ‘same size’, as in the sentence before you are talking about a size range between 60 to 10000 um.

P9L266: please clarify this sentence, because when I look at Figure 3, also for the plate shape the reflectance factor increases in the backward direction compared to the nadir direction.

P9L267: larger reflectance in all directions compared to what? The reflectance factor for the hollow bullet rosette seems to be at least equally high for some snow grain sizes compared to the aggregates of 5 or 10 plates.

Figure 4: (1) the green and blue lines and too similar and are hard to distinguish within the plot. (2) this is a reflectance factor again? Please name the y axis accordingly.

Figures 5 and 6: (1) this is a reflectance factor again? Please name the y axis accordingly. (2) The uncertainty of the CAR measurements needs to be included in the figure in the form of error bars. This also needs to be considered when calculating the RMSE. I assume a difference in RMSE of less than 0.4 % as visible between the chosen aggregates of 8 columns (98.8 um) and the columns (74.7 um) is not significant when considering possible measurement and retrieval uncertainties. This needs to be discussed in the manuscript. (3) The surface roughness clearly affects the CAR measurements at large viewing zenith angles. As I understand, the macroscopic surface roughness (in contrast to the ice crystal roughness) is not included in the SCIATRAN simulations? In this case, I suspect you are trying to fit the simulations to the measurements using different single scattering properties for the different ice crystal shapes, while more probably the macroscopic surface roughness is the underlying reason for the deviations between CAR measurements and SCIATRAN simulations. Macroscopic surface roughness enhances the backscatter by changing the effective angle of incidence, and reduces the forward scatter by casting shadows. Of course, this depends on the size of the roughness structures and their orientation, and I guess both parameters are unknown for the measurement conditions? Maybe some observations from

CC BY

within the aircraft with the naked eye or camera pictures could give an indication? At least the reduction in forward scattering of the CAR measurements compared to the simulations is visible for many different ice crystal shapes in Figure 5. Figure 6, however, shows an increase in the forward scattering as measured with CAR. In trying to choose the lowest RMSE for model simulations that neglect macroscopic surface roughness, it seems to me you can partly mimic the effect of surface roughness in choosing different ice crystal shapes (and single scattering properties). Thus, you are getting the 'right simulation', but for the wrong reasons in my point of view. Is there any way to test your simulations for different macroscopic surface roughness heights and orientation? Either way, this uncertainty needs to be discussed in detail within the manuscript.

P11L333-345: The justification of the ice crystal shape retrievals with the temperature-dependence seems dubious to me. One needs to be careful in differentiating the important temperatures here. It is true that temperature (and supersaturation!) strongly affect the shape of pristine ice crystals when the precipitating snow is formed within the cloud. If anything, the ice crystal shape should be connected to the temperature profile at the time of the last snowfall (excluding snow aging processes). However, the temperatures you are stating are temperatures measured in-flight, probably days after the precipitation event. This temperature is completely unrelated to the snow on the ground, especially as you report yourself that the snow surface consists of old snow during most days. After the snow has fallen to the ground, the vertical temperature gradient at the surface and within the snowpack is way more important for the ice crystal shape (influencing snow metamorphism processes). If you don't have in situ observations looking at the ice crystal shape on the ground, you cannot validate your ice crystal shape retrieval in that way.

Figure 7: This is way more illustrative and provides more information than Table 2, which becomes redundant in my point of view and can be removed from the manuscript.

CC BY

P12L359: it seems you are normalizing the RMSE somehow. Please provide the formula how you calculated the RMSE, as your description in the text seems to be imprecise.

P12L364: please round the effective radii to integer values. Providing two decimals is implying a degree of accuracy which is not achieved.

Figure 8: caption: ‘reflectance factor’

P12L367: I would recommend introducing Figure 10 only after Figures 8 and 9.

Figure 10: (1) caption: ‘reflectance factor’, x and y axis: ‘reflectance factor’, please state again in the caption which columns belong to the old and new snow cases. (2) I am interested in seeing a comparison of the correlation coefficients between new snow case and the lowest flight level of the old snow case as they have roughly comparable flight altitudes. This might make it easier to discuss a possible influence of surface inhomogeneities. At this point it would also help to provide more details about the differences in flight tracks between the two measurement days. Was the same area probed on both days? Otherwise of course, even the same flight altitude might not be comparable. This is connected to my earlier comment to provide more details about the actual flights performed during the campaign.

P12L377: I do not agree with the conclusion drawn here. The high correlation coefficient and small discrepancies do not justify the selection of this wavelength channel for the selection of the best ice crystal shape. The correlation coefficient and small bias is made ‘by construction’, as you selected the ice crystal shape based on the lowest bias between simulation and CAR measurements in the first place. The high correlation coefficients for this wavelength channel are therefore not surprising. In addition, the last sentence of this section (P12L379) seems a bit out of place and should be rephrased.

P13L388: This is an important point and should be included in this study already by looking at the correlation coefficient between measured and simulated reflectance fac-

Printer-friendly version

Discussion paper

tors and their dependence on the flight altitude for the case of old snow. I am interested to see whether there is a clear dependence of the correlation coefficient on the flight altitude.

P13L405: With regard to my earlier comment, the justification of the ice crystal shape retrieval with the temperature dependence cannot be mentioned here (and also not in the abstract).

P13L416: I wonder why the use of a vertically inhomogeneous snow layer in the model is only mentioned here and not in the discussion of the results already. It should not be mentioned for the first time in the Conclusions in my point of view.

Technical corrections

P1L30: Assuming that the snow layer consists [. . .]

P3L78: delete ';' after '2011'

P3L88: 'phenomenological', 'airborne'

P5L126: of the surface

P5L127: on the measured radiance

P5L128: scattering or absorption applying RTMs

P5L128: This removes the four atmospheric [. . .] from the measured radiance: i) [. . .]

P5L153: delete 'etc' or be more specific

P6L160: by a mirror – missing blank

P6L163: do you mean viewing zenith and azimuth angles?

P6L163: Please rephrase: The high [. . .] resolution [. . .] allows the estimation of the anisotropy of the reflectance in the snow-atmosphere system with high accuracy.

P6L167: RTM simulations

CC BY

P6L175: do you mean spatial inhomogeneity?

P7L208: eight ice crystal shapes/habits

P7L208: (referred to as fractal in this paper)

P8L227: ground-based measurements from AERONET

P8L228: selecting one of the aerosol types

P9L271: a priori knowledge

P9L272: to accurately reproduce measurements

P9L279: evaluate the impact of the atmosphere

P9L282: assuming the following properties

P12L362: described in the previous sections

P12L380: measurement of the reflectance factor

P13L390: reflectance factor

P13L397: reflectance factor

P13L408: reflectance factor

P13L411: reflectance factor

P14L429: comma misplaced

P15L466: do you really mean TOA reflectance? Or reflectance at flight altitude?

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2020-58, 2020.

Printer-friendly version

Discussion paper