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Discussions

Interactive comment on "Radiative transfer model for contaminated slabs: experimental validations" *by* F. Andrieu et al.

Anonymous Referee #1

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General comments

This paper presents an evaluation of an approximated radiative transfer model based on laboratory measurements. The spectral reflectance of pure ice slabs laid on snow was measured in the visible and near infrared (NIR) with a spectro-goniometer. A model for uncertainties propagation based on the bayesian inference is used to retrieve the probability density functions of ice slab thickness and snow grain size from these measurements. The paper is well written and relatively easy to follow. However, some rapid interpretations or inadequate discussions of the results do penalize the evaluation of the model and the results are not as convincing as expected. Also, the interest of the study from a geophysical point of view is poorly demonstrated and should be pointed out for TC readers. In particular, it would be worth replacing this study it in its

scientific context, highlighting more explicitly how it differs from previous works. Hence I recommend this paper be published only after the following points (some of them being critical) are addressed.

Specific comments

1) Although the text is easy to follow, it is hard to understand what the exact objective of the paper is.

The abstract and introduction suggest that the objective is to validate a radiative transfer model. Practically, two parts of the model are validated: 1) the representation of surface roughness and 2) the 2 media radiative transfer. From a geophysical point of view, it might be more interesting to evaluate the retrieval method, which constitutes, in addition, a consequent part of the study. The success of the method would suggest that this model can be applied to satellite data analysis, which is a real TC topic. This point does not imply significant changes in the manuscript, except in the definition of the objectives. More generally, the introduction is certainly too short and lacks of relevant references concerning the topic that is studied. For instance there are no references to studies of lake ice (e.g. Mullen and Warren, 1988) or sea ice spectral reflectance (e.g. Perovich 1996), that might be relevant. References to studies in planetary science for which the study might be most relevant are scarce, as well. Hence it is hard to know what is new and original with regards to the literature. Practically, do these laboratory experiments (a few mm of ice on snow) correspond to situations encountered on Earth, on other planets? Why is it relevant to study that kind of 2 layer media? Why are the surface roughness and ice thickness so important to study?

The abstract should detail the main results of the study. Instead, it essentially describes the content of the paper, without giving any quantitative results. It is somehow vague while it should be very straightforward and self-sufficient. Give numbers, percentages, wavelengths... to make it more concrete for the readers.

To sum up, the objective of the paper should also be clearly identified as soon as possi-

ble, rather than later in disparate places (see technical comments). It will substantially help the reader to follow the rest of the paper.

2) Since the evaluated model was published in another journal, it might be worth to detail more its mutliple scattering component, and in particular the meaning of the term isotropization (does it mean isotropic scattering?) which is not sufficiently defined. In fact most of the description is dedicated to the surface roughness and inclusions (the latter being unnecessary, see below), while ice thickness and snow grain size retrieval essentially depend on th treatment of multiple scattering. Concerning this point, as highlighted by the authors, the model used is approximated (p.5154, l.12). In particular I'm quite confused with the choice of isotropic scattering for snow (and probably inclusions) while it's been pointed out decades ago that snow, as any scattering particulate medium with particles large than the wavelenght, is highly anisotropic (e.g. Barkstrom 1972, Bohren 1983). Assuming isotropic scattering is thus a critical shortcoming (even more for BRDF than directional-hemispherical reflectance for instance) and can hardly be used to retrieve snow grain size. This shortcoming has a negative impact on the whole model, unless the ice thickness is such that the snow is not seen by the radiation (in which case the model is reduced to Fresnel reflection). Practically, it is not difficult (nor computationnaly demanding) to handle anisotropic scattering. The asymmetry parameter g has to be accounted for in complement to the single scattering albedo. In the adding-doubling model, it might be suffcient to define the albedo of the underlying snow layer, using for instance analytic expressions including g (e.g. Kokhanovsky and Zege, 2004; Libois et al., 2013). Anyways, the snow grain size retrieval (2 microns) is unrealistic, even in the case of hoar crystals formation. These results cannot remain as is (ie without more discussion) in a paper aimed at validating a radiative transfer model. Considering anisotropic scattering (although the flux will be nearly isotropic due to multiple scattering and weak absorption) may also improve the model and the comparison with experiments.

3) The choice of the title is very misleading because the reader expects a study on

impurities. In fact, the slabs (the nature of the slab is not detailed in the title) are made of pure ice as stated p.5140 l.9.l. As a consequence, many details about the ice inclusions add noise to the paper and should be removed to keep only the version of the model used in the present study (see also technical comments).

4) Several conclusions are drawn too quickly (eg surface roughness equivalent to average slope, 2 μm snow grain size retrieval, ice absorption masking the snow below, validation of isotropization...) and should be argued more rigorously (see technical comments).

5) The "Discussion and conclusion" part looks more like a conclusion only (or summary of results) because it does not bring much new physical insight. In case a Discussion is really wished, it might be relevant to elect one or two topics and really discuss them. Is the accuracy of the model enough for the objectives? What are its limits? How could it be improved? To which satellite data this could be applied?...

6) The figures captions are too detailed. They should be purely descriptive. Instead, most of the text should be placed within the main text (mostly in the Results part). This is the case for Figs. 3, 4, 5, 7, 8, 9, 10, 11. For instance, "the model reproduces the data well" is not supposed to be found in a figure caption.

Technical comments (italic indicates suggestions for replacement)

Title

The title is fuzzy and poorly describes the content of the paper. First because the kind of slabs is not precised (ice slabs on top of snow) and the "contaminated" does not detail if it means by air bubbles, dust, BC. Practically it is not contaminated at all in the study, making the term very inappropriate in the title.

Abstract

It is more usual (?) to have the abstract in a single paragraph, making it more consistent and self-sufficient. The objective should stand before the description of experiments

and model, not l.11. More generally, the abstract is too general and would be more appropriate for the last paragraph of the introduction aimed at describing the different parts of the paper. Details and reference l.4-5 are useless at this stagel. l.5-6 are not clear because it refers to a second interface while the snow substrate has not been ontroduced so far. It is not important to point out the isotropization there. Keep references for the introduction. l.10 : phase angles is not a standard term, would incidence angles fit (you use it later)? l.13 : the retrieved quantities appear in parenthesis, while this is to me the main result of the paper.

Introduction

p. 5139, l. 5-6 : ice snow covered don't lead to albedo changes. Changes in snow cover do. You do not mention ice at this stage, only snow, which sounds weird for a model dedicated to ice slabs. p. 5139, l.8 and 11 : snow grain size and specific surface area are essentially the same thing l. 13 : please provde references of snow properties retrieval (Zege et al. 2008, Negi and Kokhanovsky 2011) l.20 homogeneous surface is misleading because it may refer to surface roughness while you supposedly mean vertically homogenous. When the medium is not vertically homogeneous. l.28 "owing to" is awkward. As suggested by the long path lengths measured...

p.5140, l.1 : please provide reference for "several decimeters" p.5140, l.3 what does contaminated mean here? l. 5 : give some examples of satellite providing this kind of measurements l.7 : impurity content put forward but in fact not used. l.15 : remove "real"

Description of the model

p.5140, l.22 : aren't the inclusions assumed actually spherical in your model? p.5140, l.22-24 : the content of the experiment (pure ice slab overlying snow) should be presented in the introduction with relevant natural cases where this situation occurs. p.5141, l.8 : it's the specular contribution in the model (not measurement) that is more likely to be estimated from the roughness p.5141, l.10-15 : merge this with p.5140, l.24

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and avoid repetitions p.5141, l.20 : reaches p.5141, l.21 : is valid reaches p.5141, l.28 : how is the single scattering albedo of snow computed?

p.5142, l.2 : please provide reference for adding-doubling formulas p.5142, l.18 : what is the size (horizontal extent) of the sample? p.5142, l.19 : how thick is the snow substrate? Are you sure that it is optically thick at all wavelengths? p.5142, l.23 : what do you mean by the evolution of snow grain size? Increasing or decreasing? Is it a quantitative statement or was it actually measured?

p.5143, l.2 : what is the rotation speed? p.5143, l.6 : the title should be more explicit. Maybe Specular reflectance at 1.5 μm. p.5143, l.14-15 : it is not clear whether these measurements were obtained on ice only or with the snow substrate. p.5143, l.23 : To remain consistent from a title to another, maybe indicate for 3.2.2 on which material the reflectance was measured p.5143, l.24-25 : this objective should appear earlier, probably in the introduction if it is indeed a main objective of the paper

p.5144, l.1 : what is the reflectance factor? What is phase angle? p.5144, l.6-10 : how do these data support the isotropization hypothesis? Be more rigorous in your explanation p.5144, l.8 : what is an isotropic layer? Is scattering isotropic, or is the material geometrically isotropic (or both)?

Method

p.5145, l.10 : it might be useful to define here the actual model parameters to make the model more concrete p.5145, l.10 : reflectance observations p.5145, l.21 : the independency of the measurements is not straightforward for the reader. In fact, how are the measurements performed? One geometry, all wavelengths, or one wavelength all geometries? This could make a difference. p.5145, l.21 : please detail how potential bias is treated, or argue why measurements are unbiased p.5145, l.23-24 : it is confusing to say that the uniform prior distribution of the parameters is equal to that of the data (whose prior is actually not uniform).

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p.5146, l.1 : This is actually Bayes's theorem p.5146, l.3 : moving integrand at the end might be more standard p.5146, l.3 : the general formula seems useful given that you then assume a perfect model. Eq.3 could come directly, avoiding unnecessary details.

p.5147, l.9 : It is not clear why there is a specular lobe expected. Is it due to the surface roughness, to the scattering by inclusions, or to something else? p.5147, l.20 : Maybe precise the penetration depth of 1.5 μm radiation to support the statement.

p.5148, l.9 : please provide quantitative uncertainties for the reflectance measurements p.5148, l.12 : the original errors propagation is meant to propagate pdf, so you should not summarize the pdf by its mean and std, unless you show it is close to Gaussian, in which case this becomes relevant. Otherwise, why using such a complicated (but very worth) method?

p.5149, l.4 : here the reader discovers that there are no impurities in the ice slabs. This is quite contradictory with the title of the paper, and makes useless many details provided earlier in the study. Please give this essential detail in the introduction and remove all unnecessary details on inclusions and impurities. p.5149, l.5 : are you sure this spectral resolution is required? The spectral variations of ice optical index are not that strong.

p.5149, l.4-15 : this part is very similar to the one parameter model. Is it really necessary to provide all the details that are substantially similar to the previous model?

Results

p.5152, l.2 : I do not really understand your point. Is the sample perfectly smooth (no roughness) but tilted? If yes, then why do you observe a lobe? And why does your model (which assumes micro-scale roughness) fit your observations? p.5152, l.11 : the title should be more explicit p.5152, l.11 : it might be useful to add a short description of the content of this section because the reader is lost among the various inversion approaches. 1) example for individual geometries 2) results for 39 geometries 3) BRDF

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or something similar. Moving some text from the figures captions to here might help. p.5152, l.12 : if the roughness found for the previous sample is due to the average slope, how can it be used for the other samples that do not have the same slope? p.5152, l.16 : what quantity is shown in Fig.7? Which method is used for the best match? All angles together, or 39 angles individually, or a single angle? p.5152, l.23 : relatively sharp : be more quantitative p.5152, l.27 : indeed, it would have been useful to get snow grain size characteristics, especially because relevant instruments are used in other laboratories in Grenoble. p.5152, l.27 : you could get a rough estimate of the snow grain size from the BRDF measurements on snow using any analytical reflectance formula from Hapke or Kokhanovsky.

p.5153, l.1 : where do you see a decreasing trend in your data? Is this decreasing trend expected from snow physics considerations? Domine et al. (2009) may provide useful discussion. p.5153, l.18-19 : Again, this sentence is not easy to understand because the meaning of isotropization is not clear, hence the argument sounds weak.

p.5154, l.4 : above all, such snow grains (2 μm) do not exist. p.5154, l.5-8 : no physical process can produce such small grains, hence the problem is more likely due to the model

Discussion and conclusion

p.5154, l.12 : see remarks above about the isotropization p.5154, l.10-18 : this first paragraph does not provide new information (compared to the Results section) as expected in a Discussion. p.5154, l.27 : please justify "as expected". In fact, the extinction coefficient of ice at 800 nm is 2.1 m-1 (47 cm penetration depth), so that the influence of the snow substrate is major in your measurements. You could set a substrate with albedo varying from 0 to 1 in your model to show the sensitivity to the substrate.

p.5155, l.1 : please give quantitative facts to support your statement p.5155, l.7-9 : the snow impurities argument is not very convincing, in particular because in the NIR range the impact of most impurities is almost insignificant. You should support your statement

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with spectral analysis to identify other species than pure ice in the snow. p.5155, l.20-29 : it might not be appropriate to finish on such a technical note about scientific computing in a paper submitted to The cryosphere. An opening on the possibilities offered by the model for the interpretation of satellite data on icy planets would be for instance much more appropriate.

Figures

Fig.2 : in the surface medium Fig.2 : title above graphs is redundant with caption Fig.3 : what are reflectance factor and phase angle? Measures – measurements. Last 3 sentences should be moved to Results Fig.4 : title above graphs is redundant with caption Fig.5 : title above graphs is redundant with caption Fig.6 : title above graphs is redundant with caption Fig.7 : set all figures in a row rather than in one column Last 3 sentences should be moved to Results Fig.8 : Last 2 sentences should be moved to Results Fig.9 : measure - measurement Last 3 sentences should be moved to Results Fig.10 : title above graphs is redundant with caption Last 3 sentences should be moved to Results Fig.11 : the difference with Fig.8 is not clear. Precise that it is the result of all angles/wls inversion All caption (except first sentence) should be moved to Results

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