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The Cryosphere Discussions

Interactive comment on "Spectral reflectance of solar light from dirty snow: a simple theoretical model and its validation" *by* A. Kokhanovsky

Anonymous Referee #1

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Overview

This paper presents analytical expressions for snow albedo, and BRDF, taking into account light absorbing impurities. The albedo fomula is applied to recent studies of snow albedo measurements in which snow phyisical parameters (size and shape), as well as impurities contents, were controlled or measured. The formula compares well with more complex radiative transfer codes and is in relative agreement with the measurements. The author explains that such a formula could be used in the field to estimate straightforward impurity levels, instead of using radiative transfer codes that are more costly. Some suggestions are proposed that could help the manuscript being understandable by a majority of readers. In particular, the author should bear in mind

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Interactive Discussion
Discussion Paper

what his main objective is instead of presenting too many equations that are not always necessary for the overall understanding of the manuscript. Some points also have to be more detailed and more discussed for the conclusions to be more persuasive.

(1) As pointed out by the Editor in his initial review, the manuscript contains a lot of equations. Such a paper may interest a wide range of scientists, among whom a lot are not familiar with radiative transfer questions and definitions. For this reason, I'd recommend the author to state clearly at the beginning of Sect. 2 what expressions he aims at deriving, and what are the snow physical characteristics that are relevant (snow grain size, shape and impurity content). In particular, the initial decomposition of the albedo in terms of scattering events, and the discussion on random walk, though interesting, seem unappropriate and unnecessary in such a paper. Eventually, only Eqs. (12) and (13) are used. These formulae along with their derivations appear in several papers from the author (Kokhanovsky, 2004; Kokhanovsky and Zege, 2004; Zege et al., 2008). The main point here is not only how to get the equations, but to show that they are efficient and to highlight how to use them.

(2) The application of the analytical formulae to experimental studies is very interesting and essential to highlight the interest and validity of these formulae. Although explicit reference is made to the studies, it would be valuable to describe in more details the experiments, in particular detail which quantities were measured and in which conditions. This description should be systematic each time a new experiment is presented. This is particularly true for the data displayed in Figure 3 for which no information is given on the experimental method, nor on the formula that is used.

(3) The application of the albedo formula to experimental data seems straightforward, but few details are given on the applicability of the formula to these very experiments. As calculations provide very satisfying results, it seems that the formula is very efficient. In fact, a crucial question when impurities are at stake is their localization within snow. The analytical derivations assume an external mixture, meaning that the impurities are out of the snow grains. In Hadley and Kirchstetter (2012), it is precised

TCD
7, C193–C202, 2013

that the soot particles were hydrophilic, and thus were likely to be found within snow grains. In Brandt et al. (2011), the authors explain that the soot particles are also likely to be within snow grains, and they also discuss this point in the discussion. Thus it seems that there is some contradiction between the model assumptions and the physical reality. For these reasons, the localization of the impurities should be questionned, along with the fact that the theoretical formula seems in very good agreement with the experimental data, while the representation of impurities is likely to be inadequate.

(4) No reference is made to Zege et al. (2008) or Zege et al. (2011) whereas Eq. (30), which is the core of the present paper, is very similar to Eqs. (10) of Zege et al. (2008) or (11) of Zege et al. (2011). Hence the real novelty of the formula should be advanced in comparison with those existing formulae.

(5) Consistency of notations from previous papers of the authors is sometimes in default, which can be disturbing for the reader. In particular for k_{ext} and ξ which were σ_{ext} and B in Kokhanovsky and Zege (2004). Maybe this ultimate notation is the definitive one, otherwise it could be kept more consistent with previous studies.

(6) The section titles are very formal. Clarity would probably be increased if these titles were more descriptive, such as "2. Derivation of an analytical formula for the albedo of polluted snow" and "3. Validation from laboratory and field measurements"

(7) I found that the end of the manuscript, when dust is at stake, is not very clear. I find it hard to follow what are B_{d}, ϵ, $f(\lambda)$... It is puzzling to see that the dust concentration retrieved from the analytical model is almost twice inferior to the measured value of Painter et al. (2007).

Specific comments

P. 534, L2: "as a function of" instead of "as the function of"

P. 534, L6: I'd suggest "a new method for the determination"

P. 534, L15: Hadley and Kirchstetter (2012) is cited but not Painter et (2007) and Brandt et al. (2011). Maybe withdraw keep the first reference for later

P. 534, L19,22: Probably "multiple" instead of "multiply", or just "strongly light scattering media".

P. 535, L4-5: It is not clear wether they modeled it or measured it. Also, the dust is more likely IN the surface layer, rather than ON the layer. I would say something like "for a surface layer with dust concentration of 0.37 mg g^{-1}"

P. 535, L11: To what extent is it NEW compared to Zege et al. (2008, 2011)? What part of the paper/theory is NEW?

P. 535, L12: Maybe add a reference for the ART theory

P. 535, L14: These are maybe "expressions" rather than "equations"

P. 535, L26: "represented as a series"

P. 536, L11: The derivation of Eq. (4) from Eqs. (3) and (1) is not straightforward. I'd recommend the following derivation:

$$\begin{aligned} A &= \sum_{m=1}^{\infty} a_m (1-\beta)^m \\ &= \sum_{m=1}^{\infty} a_m \exp(m \ln(1-\beta)) \\ &\simeq \sum_{m=1}^{\infty} a_m \exp(-m\beta)) \end{aligned}$$

P. 536, L6: It's hard to see where Eq. (7) comes from, physically. Why talking in this

TCD

7, C193–C202, 2013

Interactive Comment

paper of the random walk theory? Moreover, is it 2D or 3D?

P. 537, L18: Eqs. (12) and (13) could be put at the beginning of Sect. 2, with reference to Kokhanovsky and Zege (2004) for instance, to avoid the complex derivation that is, in addition, different from that of Kokhanovsky (2004).

P. 538, L5: Equation (5) might be more clear and also consistent with Kokhanovsky and Zege (2004) if written $r(\mu_0) = \exp(-\alpha K(\mu_0))$

P. 538, L6: Inversion of μ and μ_0 can be puzzling. Equation (6) would be more clear if written $R(\mu_0, \mu, \phi) = R_0(\mu_0, \mu, \phi) \exp(-\alpha\nu)$

P. 538, L10: Keep a unique expression for $R(\mu_0, \mu, \phi)$. Avoid inverting the zenith angles.

P. 538, L18: Following Kokhanovsky (2004), the dense media effects are not really ignored, it just happens that they partly compensate in the case of weakly absorbing media(?). Or it depends what effects you are talking about.

P. 538, L22: It could be worth precising that Eqs. (16) and (17) correspond to the external mixture representation (with appropriate references maybe)

P. 539, L19: $C_{\text{ext}} = 2\Sigma$ comes also from Fraunhoffer diffraction, not only geometrical optics. And why use ξ while you used B in Kokhanovsky and Zege (2004) and ϕ was used in Zege et et al. (2008). This parameter is not widely known so if no definitive notation is found, it can be hard to follow for the reader.

p. 539, L20: of arbitrary shape

p. 540, L3: According to Kokhanovsky and Zege (2004), $\xi = 1.27$

p. 540, L6: It is not clear how ξ is defined. In Eq. (21), it looks like a definition and then ξ is given again. I'd suppress "and equals to $C_{\text{abs}}/\gamma V$".

p. 540, L22: The value of P depends on grain shape. It should not be in parenthesis. "the type of snow" can be suppressed since it is not detailed what link there is between snow type and grain shape.

p. 540, L23: I don't understand what "the first principles" are.

p. 540, L24: It is not clear what retrieval algorithms you're mentioning. What do you retrieve, P or a_{ef}? What does "report" mean here?

p. 541, L3: I have the feeling a_{opt} does not depend AT ALL on the shape. What is the use or the physical meaning of a_{opt}? Is it an important quantity? Is it necessary to talk about it to understand the following text?

p. 541, L4: You say "also" but I don't see the difference between Eq. (25) and the sentence "can be directly derived from the reflectance measurements". If you mean that knowing a_{opt} allows the prediction of the albedo, then how can you know a_{opt}? I have the feeling that Eqs. (24) and (25) are the same, with just a new quantity defined.

p. 541, L9: In terms of mathematical simplifications, "dominated" is not clear. I guess you mean that scattering and extinction properties of snow are not changed by addition of impurities

p. 541, L13: It might be worth to precise the unity (m^2 kg^{-1}) of ϵ for readers not used to this quantity

p. 541, L15: Instead of the reference, you could simply refer to Eqs. (20) and (21)

p. 541, L17: It seems that β_{soot} has become β_s

p. 541, L18: $\epsilon \sim \lambda^{-1}$: I understand it like ϵ^{-1} is a length, which is obviously not the case. Do you mean a relation of proportionnality? Also, why such a Rayleigh dependence? Maybe a reference or a short explanation about the small size of soot particles could help.

p. 541, L19: I think you mean "above" rather than "below"

p. 542, L7: What is the use of Eq. (32)? If you do not use it further, then it is probably

TCD

7, C193–C202, 2013

Interactive Comment

not necessary.

p. 542, L13: This sentence, though understandable, is not very clear. It is not clear which equation is used. Maybe say "In Fig. 1, plane albedo measurements of Hadley and Kirchstetter (2012) are compared to theoretical calculations according Eqs. (14) and (30), with $\mu_0 = 0$" (if this is indeed what was used). Maybe you should describe the experimental data before detailing the fitting method.

p. 542, L15: An excellent agreement

p. 542, L18: Are these theoretical calculations performed with ray-tracing models, Mie calculations, geometrical calculations?

p. 542, L19: According to Kokhanovsky and Zege (2004), $\xi = 1.27$ and $g = 0.89$ for spheres

p. 543, L1: Is it an assumption or is it the result of your fit? If it is a result, I'd say "implies" instead of "is consistent with the assumption"

p. 543, L1: The unity of ϵ seems wrong. It's probably rather m^2 g^{-1}.

p. 543, L10: I think it is interesting to see this formula. However, it could be written explicitely after Eq. (31) in the theoretical part. Or, if you keep it here, I think you should also write the formula used to compare with Hadley and Kirchstetter (2012) measurements (the plane albedo formula).

p. 543, L15: Isn't it 2.25μg g^{-1} rather than 0.25μg g^{-1}? (figure 3 of Brandt et al., 2011)

p. 543, L19: The deviations are maybe attributed too quickly to a single problem mentioned by Brandt et al. (2011).

p. 543, L23: The figures given come from calculations, not from measurements performed on natural snow. This should be precised.

p. 544, L2-3: I think this sentence is a good conclusion but not clear enough. Maybe

TCD
7, C193–C202, 2013

Interactive Comment

separate it into two sentences (e. g.) o "Clearly, the albedo reduction does not depend only on c. It also depends on snow grain shape and size."

p. 544, L4: What do you mean by "high accuracy" of an equation? Maybe you mean the sensitivity of the impurity retrieval to the albedo measurement?

p. 544, L7: The accuracy of Eq. (34) mainly depends on the values of g and ξ, that are not very well-known. Also it is stricly dependent of the external mixture hypothesis. Maybe these precautions with regard to Eq. (34) should be put forward.

p. 544, L12: Would you have a reference to support this extended formulation of the MAC? I don't really understand the function f. Do you replace B_s by $B_\mathrm{s} f$? whatever the impurity, $f(\lambda)$ including all the differences between soot and any other impurity considered?

p. 544, L13: The causal link between $f(\lambda) = 1$ and $\epsilon \sim \lambda^{-1}$ is not clear to me.

p. 544, L14: You mention the case of impurities for which scattering counts, but do not use it further for dust. In that case, the remark concerning scattering impurities can either be placed earlier in the text (at the beginning of the theoretical calculations for soot) or suppressed.

p. 544, L15: You don't mention in the text the study of Painter et al. (2007), while Fig. 3 is dedicated to this study.

p. 544, L17: if you now write B_d then what is the difference between B_d and B_s? Is $\epsilon(\lambda_0)\lambda_0$ replaced by $f(\lambda_0)\epsilon(\lambda_0)\lambda_0$, which is in fact independent on wavelength and where ϵ refers to soot characteristics?

pp. 544, L19: why is the constant b in f and not in B_d? I think a clear expression of B_d would help understand.

p. 544, L23: equal to 1.0

p. 545, L3: Shouldn't a value of MAC be defined for a specific wavelength? Or is it constant here? What quantity in the litteral expression of B_{d} do you deduce from this MAC value? Abreviation MAC could have been defined earlier when discussing the case of soot.

p. 545, L4: either "A good fit" or "the best fit" depending on what you mean. I do not understand what you want to validate using data from Painter et al. (2007). I think a bit contradictory to conclude that $c = 0.22$ mg g^{-1} gives the best fit while a different quantity was measured by Painter et al. (2007). You just highlighted that a MAC can be deduced from measurements, why don't you use Eq. (34) to determine dust MAC?

p. 545, L7: This theoretical expression for the case of multiple impurities should be put just after the function $f(\lambda)$ is defined. I would not say "the value of $B_{\mathrm{d}}fc$" but rather "$B_{\mathrm{d}}fc$" alone. In fact, I think it'd be better to finish Sect. 3 with a sentence that is more a physical result or conlusion, rather than a notation detail.

p. 545, L14: More than figures, I think calculations and measurements (or spectral albedos) are compared.

p. 545, L20: Such a simple formulation for the impact of impurities on snow albedo is certainly relevant for climate topics, but the last topic about turbulent heat exchanges is maybe a bit far from the direct applications of the formula. Also, are you talking about snow or air surface temperature? If snow, why not the whole snowpack temperature (hence melting...)?

Figure 3: The title is not clear because modeled and measured dust concentrations are not the same. In Painter et al. (2007), the authors say that the clean snow albedo has been calculated, not measured as I understand it in the legend. They also say that for both types of snow, grain size was the same, while you mention two different sizes. This is puzzling.

TCD

7, C193–C202, 2013

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