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# *Interactive comment on* "Parameterization of single-scattering properties of snow" *by* P. Räisänen et al.

**P. Räisänen et al.**

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We thank Referee #2 for his/her constructive comments on the manuscript. Below, the referee comments are written in *italic* font, and our responses in normal font.

**Comment:**

*General remarks:*

*The authors provide a wavelength dependent single scattering parameterization based on a optimum habit combination OHC that matches observed light scattering* *properties at one specific wavelength. The approach is straightforward and represents a further step towards our understanding and application of light scattering at snow particles. However, the authors arrive too quickly to some conclusions where I see a need for more discussion. I therefore recommend acceptance after major revisions.*

**Response:** More discussion will be added, as outlined below in our responses to the specific points raised by the Referee.

**Comment:**

*Specific points:*

*page 876, line 11 - 24: I understand the approach to fit observed scattering properties to model results for certain particle shape habits at a given wavelength and to use this habit combination to calculate the scattering and absorption properties at all wavelengths. However, since the reference phase function is constructed at a non-absorbing wavelength (800 nm), the OHC is not or only to a small extend dependent on particle size (as the authors also state on page 881). It mostly depends on particle geometry. The situation is even worse since the polar nephelometer with its observation range between 15 and 162 degree scattering range excludes the forward and backward scattering region that contain the largest information on size.*

**Response:** We are not entirely sure if we understood this comment correctly. The phase function is very weakly sensitive to size because the size parameter is very large (geometric optics regime) and absorption is very weak. However, in our view, this insensitivity can also be viewed as a benefit: it implies that potential inaccuracy in the definition of the size distribution does not influence the choice of the OHC appreciably.

At the same time, it is true that phase function observations made at $\lambda = 800$ nm contain very little information on absorption. In this sense, it is indeed a clear limitation of our dataset that the observations are made at a single very weakly absorbing wavelength. However, we would like to emphasize that this does not make it pointless to provide a snow co-albedo parameterization (Eq. 11). Namely, as evidenced by Fig. 5b and 6b–d, there is a systematic difference in absorption between non-spherical particles and spheres, the co-albedo being significantly larger for non-spherical particles, for a given volume-to-projected-area equivalent radius $r_{vp}$. Therefore, although we are not able to constrain the co-albedo of snow precisely (as there is scatter among the non-spherical shapes), at least we can capture the systematic difference between non-spherical shapes and spheres.

In the revised manuscript, more discussion of this issue will be added to the end of Section 5, in connection to Fig. 6.

**Comment:** *879, 5: "In fact, this approach does not represent any specific roughness characteristics, but..." Very good! I appreciate this comment very much as the term "roughness" is often misused in the light scattering literature.*

**Response:** Thanks! No change required in the manuscript.

**Comment:** *879, 15: "blowing snow": Of course, details can be found in Guyot et al. (2013), however, it would be good to provide some information on how representative the observed phase functions are, i.e. homogeneity of the snow conditions, duration of the observations, ...*

**Response:** In the revised manuscript, discussion of the weather and snow conditions will be added to Section 3, as outlined below:

The blowing snow case on 23 March was preceeded by heavy snowfall on 22 March, ending during the night of 23rd. The last snowfall before the March 31 blowing snow case occurred on 29 March. Consequently, the case of 23 March represents essentially new snow, while on 31 March, some snow metamorphism had occurred, and the snowpack was probably denser (although snow density was not measured). The near-surface air temperature ranged from $-5$ to $-9$°C during the 23 March event and from $-18$ to $-20$°C during March 31. The wind speeds ranged from 1 to 9 m s$^{-1}$ on 23 March (median value 4 m s$^{-1}$) and from 5 to 8 m s$^{-1}$ on 23 March (median value 7 m s$^{-1}$). Mainly cloudy conditions prevailed on 23 March, while 31 March was cloud-free. The phase functions shown in Fig. 1a are averages over the entire blowing snow events, which lasted for approximately 10 hours on 23 March and 12 hours on 31 March.

**Comment:** *880, 19–26: The authors rather quickly dismiss the scattering peak at 145 degree scattering angle as an artefact and as quantitatively irrelevant. However, if this peak is caused by photodiode problems, how can we trust the rest of the observations? Why should this be limited to an angular region around 145 degree? The authors note that non of the considered particle geometries can reproduce this feature. I suggest to search the light scattering literature to identify which particle geometry could do the job.*

**Response:** Firstly, we have to withdraw the photodiode explanation given in the original manuscript. While it is true that reflections by photodiodes are a possible source of inaccuracy in polar nephelometer measurements, neither the paper cited in the original manuscript (Jourdan et al. 2003) or any other paper we have found provides direct evidence that this would be responsible for the 145° feature. We must apologize for not checking this properly when preparing the original manuscript. Regarding the reliability of the PN measurements, Shcherbakov et al. (2006) (their Table 1) report an accuracy of 3–5% between 15° and 141°, but degrading to 30% at 162°, for an experimental setup with low extinction. Thus the phase function derived from PN measurements is, overall, less reliable near the backscattering direction than in near-forward and sidescattering directions.

It is in principle possible that the 145° feature is caused by ice crystals, but unfortunately, we have not been able to identify an ice crystal geometry that would reproduce this feature. This feature falls between the icebow peak of spherical ice particles at 135° and a maximum that appears for many pristine hexagonal crystals at 150–155°. One might speculate that for rounded crystals (which seem to be present in Fig. 2a), the latter peak could be displaced, but it is not at all obvious (and indeed rather unlikely) that this would result in a clear maximum near 145°. Furthermore, for oriented crystals, it would most likely be possible to find geometries that produce a maximum near 145°; however the presence of a large amount of oriented crystals in blowing snow (where the conditions are typically turbulent) seems implausible. Finally, in general, the presence of a single halo peak near 145° would be surprising, when there is no evidence of other halos in the measured phase function.

One particle type that would produce a phase function maximum at 140–145° are liquid cloud droplets with a diameter of $d \sim 10\ \mu$m. However, we consider this an unlikely explanation due to the meteorological conditions. There was no cloud (i.e., fog) at the surface level, the air was subsaturated with respect to liquid water, and the temperature was well below $0°$C in both cases. In particular, the latter case (31 March) was quite cold ($-18$–$-20°$C), with a relative humidity of 79–87% and cloud-free skies. It is hard to imagine how a substantial amount of liquid droplets could exist in these conditions; yet the 145° feature was clearly visible in the measured phase function also in this case.

In summary, we cannot say with certainty whether the 145° feature is real or an artifact (although the latter is perhaps more likely, given that the PN data at these angles is in general less accurate than at smaller scattering angles). Further measurements, preferably using some alternative technique, would be needed to resolve this issue. Regarding the SSP parameterization, our original statement that the 145° feature has only a small influence still holds true. Note that we did not (and do not) screen out this feature when developing the parameterization.

A shorter version of the above discussion will be added to Section 3 in the revised manuscript.

**Reference:**

Shcherbakov, V., Gayet, J.-F., Baker, B. and Lawson, P., 2006: Light scattering by single natural ice crystals. *J. Atmos. Sci.*, **63**, 1513–1525.

**Comment:** *881, 23 - 882, 2: I respectfully disagree with the pragmatic approach to completely ignore the observed particle shapes and to adjust the optimum habit combination purely by minimizing a light scattering cost function. The observed snow grains should provide some constrains on the size dependent particle shape variation, see the work by Brian Baum. The authors correctly state on 884 "Thus, the potential dependence of snow crystal shapes on their size is not considered here". I consider this as an unnecessary simplification.*

**Response:** This is, at the same time, a well-justified suggestion and one that would be very hard to address satisfactorily. Consideration of how the shape of snow grains depends on their size would require a fundamentally different approach to the development of the parameterization. This would require (1) an analysis of how the shape of grains in (blowing) snow depends on size, (2) a parameterization of this size-shape distribution, (3) the computation of the corresponding single-scattering properties (SSPs), and (4) performing the numerical fits.

We agree that in principle, this would be the ideal solution, and this approach indeed represents the state-of-the-art for the parameterization of ice cloud single-scattering properties. However, to our knowledge, it has not been attempted for snow. The primary practical difficulty is that a very large fraction of the particles in snow are irregular. For the current samples of blowing snow, manual classification of shapes in CPI images by Guyot et al. (2013) (cited in the manuscript) suggested that more than 80% of the particles were irregular. It would be unfeasible to compute the SSPs of each irregular particle “exactly”, so in practice, one would have to associate them with some habit type in available SSP databases (e.g., some aggregate type in the Yang et al. (2013) database). However, there would be much ambiguity in such an approach, and it is not clear that it would, in practice, result in a better SSP parameterization. Also, carrying out such an analysis and reworking the parameterization from “scratch” would be very laborious (well beyond a typical major revision).

It may also be noted that our approach basically follows that in Kokhanovsky and Zege (2004), where the choice of Koch fractals for approximating the scattering by snow was likewise based on phase function data only. Furthermore, our approach may be considered analogous to the widely used practice of modeling the SSPs of irregular dust particles. Instead of considering the actual dust particle shapes, shape distributions of spheroids are used operationally in a variety of applications (Dubovik et al. 2006, 2011; Levy et al. 2007; (references provided in the manuscript)), as they have been found to mimic reasonably the scattering by dust.

Consequently, we will keep our “pragmatic” approach also in the revised manuscript. However, discussion of this issue (along the lines noted above) will be added to the beginning of Section 4 and to the end of Section 8 (see also our response to the last major comment).

**Comment:** *Discussion of Fig. 3: From the very interesting comparison of the observed phase function to those of the individual particle geometries I get the impression that the observed 145 degree peak somewhat resembles the 150 degree peak for hexago-*

*nal shaped particles.*

**Response:** This is true, but this peak is consistently at slightly larger scattering angles (around 150–155°) than the 145° feature in the observed phase function. We will note this in the revised manuscript: *The observed maximum near 145° falls between the "icebow" peak for spherical ice particles near 135° and a maximum seen for many pristine hexagonal shapes at 150–155°.*

**Comment:** *885, discussion of Fig. 5: Fig. 5a nicely shows that there is a single 3 habit combination that fits the asymmetry parameter best, and that this is not the case for the absorption parameter, because of the rather weak absorption. It looks like there is a set of 5 to 6 3 habit combinations, which provide cost $<$ 0.1 for the absorption parameter. Are those combinations very different from each other? In general, since absorption at 800 nm does not provide much sensitivity on particle habit, as the authors also state several times, I suggest to reconsider to remove the OPC exercise for the absorption parameter.*

**Response:** Figure 5a demonstrates that by using three habits, we can constrain the asymmetry parameter very well, but also that there are several three-habit combinations that yield nearly as small values of the cost function. As stated in the original manuscript, the best habit combinations include "severely rough" droxtals and strongly distorted Koch fractals, while the third habit varies from one combination to another. In the revised manuscript, we will add a table that lists explicitly the three-habit combinations with cost function below 0.1.

Regarding absorption, there are two points to make. First, the available observations indeed yield little/no information on absorption. Second, snow grains are distinctly non-spherical, and for non-spherical particles, the $\xi$ parameter and the co-albedo $\beta$ are, in general, systematically larger than those for spheres. There is some uncertainty because the values of $\xi$ differ between different non-spherical habits. This scatter implies that our parameterization (Eq. 11) cannot be expected to present the co-albedo of snow precisely. Nevertheless, we think that this parameterization is useful, as it is very likely that the actual values of $\xi$ and $\beta$ are larger than those for spheres.

We plan to remove Fig. 5b and the associated disussion in Section 4, as the above points can be made based on Fig. 6 too. Some more discussion of this issue will be added in connection to Fig. 6.

**Comment:** *887, 8: point number 4: I totally agree! But if this is so, why going through all the effort and provide a size/wavelength parameterization that may not be representative for snow particles in general? In my view, the authors too quickly jump from a case study to a general parameterization. Other researchers will happily apply this "DISORT ready-to-use" parameterization to all kind of snow conditions without questioning its applicability.*

**Response:** While the accuracy may not be equally good at other wavelengths or snow grain sizes, we do anticipate that it is better than that for spheres, most probably substantially better. This is an important point, as spheres are still widely used for radiative transfer calculations involving snow, due to the simplicity of using Mie theory. By providing an easy-to-use parameterization based on the OHC, we hope to improve upon this situation. To our knowlegde, no such previous parameterization exists. In this respect, the situation for snow is much less mature than that for ice clouds, for which there are numerous SSP parameterizations available. Thus, if we would not provide this parameterization, there is an obvious risk that some/many researchers will continue to use spheres for snow, happily or unhappily!

In short, most certainly our parameterization is not perfect (no parameterization is).

The key question is, whether it is *useful*. In the present situation, we believe it is. If a better parameterization based on a more comprehensive dataset is provided in future, then it is of course recommendable to start using that parameterization instead.

A description of changes planned to the text (in the concluding section) is provided below in connection to the last major comment.

**Comment:** *896: section 7: Don't you need to account for close-packed effects in the radiative transfer calculations?*

**Response:** In principle, yes, but we expect that their effect is rather small. As noted in the original manuscript (p. 876, lines 26–28): *For simplicity, close-packed effects are ignored in the calculations. It has been shown by Kokhanovsky (1998) that, at least as a first approximation, they do not have a pronounced impact on the snow reflectance.* In the revised manuscript, we will move this sentence to Section 7 (Radiative transfer applications), where this issue is more topical.

Furthermore, the same simplification has been made in most other radiative transfer studies involving snow, at least in the solar spectral region considered in this work.

**Comment:** *Conclusions: I think the authors did a good job with the technical set up of the snow light scattering parameterization, but the data basis that is used for that is simply not sufficient. Thus, the work should be more carefully treated as a case study on the effect of different shape assumptions on snow reflectance. The OHC constructed here should not get generalized (as the authors try to encourage the reader on page 902).*

**Response:** We agree that the data basis is rather limited. Note however, that this also applies to previous studies on the topic (e.g., the suggestion to use Koch fractals by Kokhanovsky and Zege (2004) was based on a comparison with a single phase function for laboratory-generated fairly small ice crystals — here we at least have direct phase function measurements for blowing snow).

To repeat our response above, currently many researchers still use spheres for computing the single-scattering properties of snow (as this is the easiest way to go, in the absence of parameterizations based on non-spherical snow grains). In spite of the limited data basis, we think that our statement (on p. 902, lines 14–16) *"it seems reasonable to assume that the OHC selected here provides a substantially better basis for representing the SSPs of snow than spheres do"* is justified. Therefore we think that providing this parameterization is useful.

However, in response to the Referee comments, some changes are planned in the concluding section.

- The paragraph regarding the limitations of the current work (p. 902, lines 4–13) will be converted into a bulleted list, to make it even more explicit.
- The wording of the (currently) last paragraph on p. 902, lines 14–20 will be moderated a bit.
- A new paragraph will be added, which states explicitly that the current parameterization should not be considered as the "final solution" to the representation of single-scattering properties of snow. Development of snow SSP parameterizations based on more comprehensive datasets, and ideally linking the snow grain shapes more directly to those observed, is encouraged.

A preliminary version of the text replacing the last paragraph in the original manuscript (p. 902, lines 14–20) is given below:

*In spite of the concerns mentioned above, it seems reasonable to assume that the OHC selected here provides a substantially better basis for representing the SSPs of snow than spheres do. Moreover, the parameterization equations provided in this paper are analytic and simple to use. A Fortran implementation of the snow SSP parameterization is available at https://github.com/praisanen/snow_ssp.*

*To conclude, this paper describes a first-of-its-kind parameterization for representing the SSPs of snow in the solar spectral region. The parameterization is provided in hope that it will be useful, especially to those researchers that still use spherical particles for computing the radiative effects of snow. Nevertheless, it should definitely not be viewed as the "final solution" to the treatment of SSPs of snow. We hope that the present work will inspire the future development of snow SSP parameterizations based on more comprehensive datasets. Furthermore, at least in principle, it would be desirable to replace the current approach (where the shape distribution of snow grains is selected based on scattering measurements only) with an approach that more directly links the snow grain shapes to those actually observed. This would require, first, the parameterization of the size-shape distribution of snow grains based on observations, and second, the computation and parameterization of their SSPs. The main challenge in such an approach is the treatment of irregular grains, which are very common in snow.*

**Comment**:

*minor:*
*882, 17: fractal geometry → tetrahedral geometry*

**Response**: This will be corrected.

**Comment**:
*885, 17: The differences in cost function, ... with lowest cost function values... ???*

**Response**: To avoid the repetition, this will be reformulated as "The differences in cost function . . . between the best habit combinations are very small . . . ".

Interactive comment on The Cryosphere Discuss., 9, 873, 2015.



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