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

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We thank Bastiaan van Diedenhoven for his constructive comments on the manuscript. Point-by-point responses to the comments are provided below. The referee comments are written in *italic* font, and our responses in normal font.

Comment: *This paper aims to provide a parameterized set of single scattering properties for surface snow. Although the resulting model is rather ad hoc, availability of such a model would be of benefit to the snow radiation modeling community, as often still models based on perfect spherical grains are used. The paper is well-structured and clear and I recommend it for publication in The Cryosphere.*

I do have a few minor suggestions and questions for the authors to consider to improve the paper:

Page 881: line 19: It is noted that, since absorption is weak at 0.8 micron and particles are in the GO regime, the modeled phase function is only weakly sensitive to size. However, it should be noted that many of the habits in the Yang et al. database have geometries (i.e. component aspect ratios) that depend on size. This means that 1) some of the obtained fits in Figure 3 probably do somewhat depend on the chosen size distribution and 2) a combination of habits that provides a good fit given a certain size could be producing a poor fit (i.e. unrealistic phase function) when applied to another size because the different geometry. However, the droxtal that is used does not depend on size, and neither does the fractal by Macke et al. It is unclear to me from the Yang et al. papers whether the aggregates of 10 plates have geometries that depend on size. However, that appears not to be the case, as the asymmetry parameter for non- absorbing wavelengths do not appear to depend on size (for large sizes), as they would if the geometries would significantly change with size. This is then a (unintentional?) benefit of the authors' final choice of habits.

Response: This is true, and in fact not completely unintentional. The processes that determine the relationship between grain shape and size in snow are different from those in ice clouds, and therefore, size-shape relationships based on crystals in ice clouds might be misleading. We feel that at this point, it is for simplicity better to ignore the size dependence of shapes. Indeed, it also helps to make the parameterization simpler.

In the revised manuscript, it will be noted at the end of Section 3, that the geometry of some of the habits in the Yang et al. (2013) database (specifically, solid and hollow hexagonal columns, plates, and solid and hollow bullet rosettes) depends on size, while that for others (droxtals and the three aggragate habit types)

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does not. Furthermore, at the end of Section 4, it will be made explicit that avoiding such depedencies was one of our criteria when making the final choice of the OHC.

Comment: *Page 886: Equations 7 and 8: I assume the Beta's are a function of size parameter x here. Please add "(x)" for clarity.*

Response: In fact, the β:s depend not only on the size parameter x but separately on the particle size r_{vp} and wavelength λ (because ice refractive index depends on λ). This will be marked explicitly in the revised manuscript.

Comment: *Page 887: line 22: I think a reference to Macke et al. (1996) would be useful here.*

Response: This reference will be added in the revised manuscript.

Comment: *Page 890: Equation 12: I replotted Fig. 1 in van Diedenhoven et al (2014) and would like to confirm that this definition of absorption parameter also results in a better overlap between the single scattering albedos at several wavelengths calculated for a hexagonal crystal with aspect ratio of 1 as compared with the case using the definition of van Diedenhoven et al (2014). Thank you for this insight.*

Response: This is good to know! No change is required in the manuscript.

Comment: *Page 890: Equation 13: How are the parameters in this equation determined? Are these determined using a least-squares fit?*

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Response: The aim of Eq. (13) is to minimize the root-mean-square error in g. In practice, the functional form of the parameterization as well as the parameter values were determined by trial and error together with visual analysis of the data. However, we believe that the parameter values are quite close to optimal, that is, a rigorous search of the parameters would probably not improve the fit significantly.

In the revised manuscript, it will be noted that Eq. (13) is aimed at minimizing the rms error of g. A more detailed discussion of the fitting approach is, in our opinion, not warranted, on one hand in the interest of brevity, and on the other hand because the "real-world" uncertainty of the parameterization is very likely influenced more by the limitations of our dataset than the numerical inaccuracy of Eq. (13) (which is anyway quite small). Basically, this remark also applies to the other comments regarding the numerical fits below.

Comment: *Page 890-891: You state that g "increases slightly with increasing size parameter x_{vp} even at non-absorbing wavelengths (in the size parameter region where the geometric optics is not yet fully valid)." This is probably due to the fact the diffraction asymmetry parameter becomes increasingly less than 1 for decreasing size. I suggest adding that note if you agree.*

Response: Diffraction is certainly partly responsible for this. However, it is not necessarily the only explanation, in the case of the Yang et al. (2013) database, which utilized an "improved geometric optics method" with some refinements over the ordinary geometric optics. These refinements may (and probably do) influence how the asymmetry parameter g changes with size. An inspection of the values of g at weakly absorbing wavelengths suggested that g changes somewhat more with size than expected from diffraction alone; however, it is hard to be sure because g is not provided separately for diffraction.

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In the revised manuscript, this sentence will be formulated as: "... g increases slightly with increasing size parameter x_{vp} even at non-absorbing wavelengths, in part because the diffraction peak becomes narrower."

Comment: *Page 891: Equations 15 and 16: I suggest adding a reference to Macke et al. (1996).*

Response: The reference will be added.

Comment: *Page 892: Equation 19: Is there any reference for this approximation? How was it determined? Diffraction is mostly determined by the projected area of a crystal, so a parameterization in terms of x_{vp} is unexpected. It may be noted that an alternative approximation was given by Eq. 14 in van Diedenhoven et al. (2014).*

Response: No reference is given in the text, which implies that the approximation was developed by ourselves. The parameterization of g_{diff} was derived by attempting to minimize the rms errors in the logarithm of phase function $\ln P_{11}$ in near-forward directions (within a few degrees), although the choice is a bit arbitrary. The minimum in rms error is not very sharp, and furthermore, we were not able to cleanly separate the diffraction peak from the rest of the phase function because only the total phase function is provided in the Yang et al. (2013) database.

The parameterization of g_{diff} is provided in terms of x_{vp} to be consistent with the rest of the SSP parameterization. However, the size parameter defined with respect to the projected area x_p, which is physically more relevant for diffraction, is directly proportional to x_{vp} for the OHC: $x_p \approx 1.535 x_{vp}$, so that Eq. (19) may be rewritten as

$$g_{\text{diff}} = 1 - 0.60/x_{vp} = 1 - 0.921/x_p. \quad (1)$$

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Compared to Eq. (14) in Diedenhoven et al. (2014), our values of g_{diff} are somewhat smaller in the size range $x_p < 100$ considered by Diedenhoven et al. We found this beneficial, however, probably because it to some extent compensates for errors incurred by approximating diffraction with a Henyey-Greenstein phase function. The only way to improve the accuracy substantially would be to parameterize the shape of the diffraction peak more rigorously, but in this case, we consider simplicity more important. As stated in the text: *This treatment of diffraction . . . is a rough approximation, and clearly not ideal for studies of very near-forward scattering, but it serves well the current purpose. On one hand, it improves the accuracy compared to the assumption of a delta spike, and on the other hand, the HG phase function has a very simple Legendre expansion . . .*

In the revised version, the following changes/additions are planned: (1) It will be noted that the general aim in fitting the phase function parameterization was to minimize the rms errors in the logarithm of the total phase function (lnP_{11}), as the diffraction and ray tracing parts were not available separately (in this respect, the division expressed by Eq. (14) is conceptual rather than rigorous); (2) the expression of g_{diff} as a function of x_p will be provided; and (3) the parameterization of Diedenhoven et al. (2014) will be referred to.

Comment: *Page 892: Equation 22: Where is this form based on? How are all parameters in this equation determined?*

Response: When deriving Eq. (22), only the first two terms of Eq. (14) were included in the phase function parameterization, with diffraction treated as explained in the manuscript. In the first phase, a fortran program was utilized for an iterative search of the best value of w_1 separately for each wavelength λ = 0.199–2.7 μm and snow grain size r_{vp} = 10–2000 μm, where "best" is defined in terms of the rms error of $\ln P_{11}$ over the whole range of scattering angles (0–180°). In practice, this rms error is strongly

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dominated by the ray tracing part of the phase function. In the second phase, the values of w_1 were analyzed visually, and the functional form of the w_1 parameterization was determined by a "trial and error" procedure, where the goal was to minimize the rms error in w_1. We chose to exclude cases with very strong absorption (co-albedo $\beta > 0.3$) when doing this fit, since such cases are probably of little practical importance due to the low reflectance of snow. Thus, this is not a rigorous root-mean-square fit, but most probably, quite close to optimal.

Since explaining all of this in the paper would probably be distracting to the reader, we will note in the revised manuscript only the general aim of the phase function parameterization, which was to minimize the rms errors in $\ln P_{11}$.

Comment: *Page 894: Equation 27: How are all parameters in this equation determined?*

Response: First, the phase function residuals (P_{resid} in Eq. 14) were determined by subtracting the diffaction and ray tracing parts (the P_{diff} and P_{ray} terms) from the "exact" phase function P_{11} for the OHC. Second, the residuals were developed into Legendre series, for each r_{vp} and λ separately. Third, the Legendre coefficients in these series were parameterized by performing a root-mean-square fit with the LAPACK subroutine DGELS, which yielded the coefficients in Eq. (27).

In the revised manuscript, we will include the following statement: *The parameterization coefficients c_{mn} were determined by minimizing the rms errors of a_n with the LAPACK subroutine DGELS, and they are given in Table 1.*

Comment: *Page 894: line 6: Why is the Legendre expansion replaced by a polynominal? Could the terms b_n be directly determined from a_n or are they determined by a separate fit? Does the form of Eq. 28 also ensure normalization?*

Response: The ordinary polynomial form (28) was derived by writing out the Legendre polynomials in (26), which gives directly the coefficients b_n and d_{mn}. Therefore, it is completely equivalent to (26) and also ensures normalization. It is provided because in applications which do not require the use of a Legendre expansion (e.g., Monte Carlo models), an ordinary polynomial is simpler.

To make it absolutely clear that the two forms are equivalent, the following note will be added right after Eq. (29): *Here, the coefficients d_{mn} were obtained directly based on the coefficients c_{mn} in Eq. (27), by writing out the Legendre polynomials in Eq. (26).*

Comment: *Page 896 and further: The phase function was parameterized as simplified parameterization and a full parameterization. An even simpler parameterization would be just taking a Henyey-Greenstein phase function with the parameterized g. I think it would be useful to show the improvement that the additional terms bring compared to using a Henyey-Greenstein phase function. The HG results could be included in Figs. 10, 12, 13 and 14.*

Response: Thank you for this very useful comment! We have tested the use of the Henyey-Greenstein phase function, and found that it leads to substantial and systematic errors in the phase function: underestimation in the exact forward scattering direction, otherwise overestimation at forward scattering angles up to ≈40–80° (depending on the case), and underestimation at sideward and backward scattering directions.

In the revised manuscript, we will add the Henyey-Greenstein phase function to Fig. 10 and at least some of Figs. 12, 13 and 14 (all may not be necessary to make the point).

Comment: *Figure 2a: The images appear to show many rounded crystal edges,*

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which are signs of melting or sublimation. This can significantly affect the optical properties. Please add a discussion about the conditions of the snow pack at the time of the measurements in section 3. Can this be considered old snow?

Response: Discussion of the weather and snow conditions will be added to Section 3.

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.

It is quite possible that the rounded forms in Fig. 2a are related to sublimation, but melting is not plausible because the temperature stayed well below zero during the whole campaign.

Comment: *Appendix A: You might want to note that the Q_{ext} for fractals equals 2 for all sizes.*

Response: This is true, due to the use of geometric optics. We will add a short note about this in the revised manuscript (not in the Appendix but in connection to the discussion of Fig. 7, which shows Q_{ext}).

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