Interactive comment on *"Relating optical and microwave grain metrics of snow: The relevance of grain shape"*, by Q. Krol and H. Löwe.

General comments

This paper addresses the relation between the grain metrics commonly used to model snow optical and microwave properties. At first order, snow microwave properties are governed by the exponential correlation length ξ while snow optical properties firstly depend on snow specific surface area (SSA). However, at second order snow grain shape also affects snow radiative properties. From this statement, statistical relations are derived that make the link between snow microstructure characteristics (curvatures) and snow physical properties. The relation between ξ and SSA is thus improved compared to previous empirical relations, by adding a contribution of snow grain shape. The general theoretical framework of Malinka (2014) is then used to show that snow optical properties depend on the moments of the chord length distribution. Based on this framework, another statistical relation is derived to express the second moment of the chord length distribution in terms of microstructure length scales. From this, a statistical relation between ξ and the first two moments of the chord distribution is derived. This suggests that shape parameters derived from optical measurements could be used as inputs for snow microwave modeling. This point is supported by comparing the values of the optical shape parameter B deduced from Malinka (2014) theory to values determined experimentally.

The paper is overall well written and pleasant to read, the objectives are well defined at the end of the introduction. The theoretical background is nicely presented and clearly outlines the problem. The approach is original and takes advantage of recent works in snow optics. It also applies the statistical properties of general random heterogeneous materials to the case of snow, thus linking rather theoretical studies and practical cases as illustrated by the use of μCT images of snow samples. The authors stress the need for a unified definition of grain shape and propose mean curvatures as such definition. They show that both microwave and optical properties can be expressed in terms of SSA and mean curvatures. Their approach is supported by the analysis of a large set of μCT images. They also provide valuable physical insight on the representation of snow microstructure as a particulate or heterogeneous medium. For these reasons, I recommend this paper be published in The Cryosphere. However, a number of critical points should be addressed before publication, related in particular to the fundamental assumptions underlying the presented theoretical framework.

Specific comments

The theoretical framework presented in this paper strongly relies on critical assumptions that are not sufficiently discussed, although several important results largely depend on them.

1) Throughout the text, snow is considered isotropic and the derivations significantly rely on this critical assumption. Although this assumption is clearly stated, several details are lacking to convince the reader that the results remain reliable. First, more details on the investigated snow samples should be provided. So far only 3 lines (section 3.1) present these critical elements of the study, which is not enough. Do these samples consist of sifted snow, natural snow samples taken in the field without perturbing the microstructure, snow samples resulting from metamorphism experiences in the laboratory...? It is clear that depending on the origin of the samples, the isotropic hypothesis is more or less acceptable. For instance depth hoar is known to be highly anisotropic and can hardly be investigated under this hypothesis. The authors should consider removing highly anisotropic snow samples if they do not fit in the theoretical background.

At the same time, the authors do have the necessary material to further discuss the isotropic hypothesis because the parameters are obtained from averages over the 3 directions x, y and z. Giving a hint of the actual anisotropy from the analysis of these 1-D parameters might help the interpretation of the data and estimate the associated uncertainties.

2) In this study, the successive chords in snow are assumed independent, which is a strong assumption not really defended by the authors. This same assumption was used by Malinka (2014) who considered a random medium, whose optical properties where then derived. However, this author clearly states in his conclusion that: "*The requirement of stochasticity is mandatory: the facets orientation and the ray path length inside solid or voids must be independent variables. [...] The question of applicability of the model to any particular medium should be considered separately based on compliance with the experimental data.*" Practically, one might expect light rays to be trapped in snow grains or selectively focused in preferential location, which would result in different chords having different realization probabilities.

A critical consequence of the random distribution hypothesis is that at low ice absorption, the optical properties of this random medium do not depend on the shape parameter (see e.g. eq. (25) of Malinka (2014) from which B can easily be derived). This is somehow contradictory with the definition of grain shape, which is expected to impact snow optical properties in the standard particular representation.

An alternative approach could be to validate this random medium assumption by comparing the values of B retrieved from Malinka (2014) to those determined by Libois et al. (2014), which are very similar. Once the random medium hypothesis is somehow validated, then the shape parameter only impact optical properties at more absorbing wavelengths. An important corollary of this would be that only optical measurements at relatively absorbing wavelengths would contain information about snow grain shape.

3) When it comes to the analysis of µCT images, the question of voxel size (ie resolution) is not enough discussed. In fact, the resolution varies from a set of measurements to another and is generally not that small compared to snow size metrics. This probably has an effect on the derived results and might explain why different subsets of points appear on several Figures (e.g. 2 bottom left and 4a). The smoothing parameter is discussed in sufficient details but resolution is probably an issue as critical.

4) Although snow optical properties equally depend on the parameters B and g, the paper is mostly focused on B. The analysis presented for B can very easily be extended to g. This would be more exhaustive because all parameters relevant to snow optics would be tackled, as all parameters (actually only ξ) needed for snow microwave modeling are.

5) The manuscript would benefit from a slight reorganisation of some parts because redundancy is found at several points and excessive details sometimes pollute the paper. Some elements are given too early (e.g. details about the Euler characteristic that should probably not be mentioned before the discussion section), some others should be provided in a different order (more details are provided along the technical comments). Also sections 3.3 and 3.4 could probably be merged.

6) The authors make their best to infer the shape of the statistical relations from theoretical backgrounds. However, this often adds noise to the paper because 1) the underlying assumptions are often very restrictive and not applicable to snow (dilute medium, random medium, use of Taylor expansion at 0 for estimating functions at infinity...) and 2) these statistical relations are eventually

revisited by adding terms. I think there is no problem assuming a certain type of relation, and then testing it with the available data. For sure, the type of relation can be suggested by a rapid analysis of existing formulae, but there is no need trying to justify it too much. In this context, I would suggest to remove the unnecessary calculations and reformulate the section around Eqs. (14) and (15). For instance the authors could say that they show the validity of Eq. (14) from images, even though initially this relation is only valid to restricted cases. All the attemps to justify this equation are unncessary.

7) The authors should give a consistent name to all important quantities ξ, λ_1, λ_2, μ_1, μ_2 and keep those names all along the manuscript. For instance, exponential correlation length and correlation length are sometimes used alternatively without a clear distinction. Porod length, optical diameter and curvature length are used sporadically as well.

8) At the light of the comments above, it will probably be necessary to rewrite the last section of the discussion (5.4).

Technical comments

Title

Could “snow grain size” be used instead of “grain metrics of snow”?
Alternative suggestions (these are only suggestions):
- “Relating optical and microwave snow grain size: The importance/relevance of using/considering grain shape”
- “Accounting for snow grain shape to improve the relation between optical and microwave snow grain size”

Abstract

p.1 l.1: rephrase to better compare the roles SSA and exponential correlation length play in determining snow optical and microwave properties.
Either from the physical point of view: “microwave emissivity/properties mostly depend(s) on the exponential correlation length”.
Or from the modeling point of view : “the exponential correlation length is the relevant quantity in most snow microwave models” or “the exponential correlation length is used to simulate snow microwave properties”

p.1 l.3: a microwave model is not “forced” by optical measurements, it uses quantities derived from optical measurements (e.g. SSA) as inputs. Forcing more generally refers to something external to the system (e.g. boundary conditions). This is correctly said p.2 l.9.

p.1. l.3: “the understanding of ξ” is vague. Simply say “To refine this relation between...]”

p.1 l.5: it is a statistical *relation* more than a *prediction*

p.1 l.8-9 : maybe remove this sentence because it does not provide additional information about the results. Also, it is somehow questionable in terms of applicability within the present theoretical framework. Keep it for the body of the manuscript.

p.1 l.10 : *B* is called the absorption enhancement parameter. Consider doing the same calculations with *g*.

p1. l.10 : the last sentence of the abstract is not clear. Maybe say “Our results suggest that optically derived shape parameters can be used to refine the estimation of ξ”.

<u>Introduction</u>

p.1 l.16-19 : maybe invert the order of the two sentences to keep chronological order

p.2 l.4 : “with the MEMLS model” instead of “is used”

p.2 l.14 : “though less significant...” is risky because the impact can actually be significant (errors up to 50%) for BRDF or light penetration simulations for instance.

p.2 l.16 : reference to Picard et al. (2009) might be relevant

p.2 l.17 : in this study the absorption enhancement parameter *B* and asymmetry factor *g* (name these factors) are equally important, except that only *B* can be estimated from optical measurements. Note that Libois et al. (2014) experimentally determined the parameter *B* for a variety of natural snow samples.

p.3 l.1 why “systematically?”

p.3 l.12 : not clear what “images” you're talking about

p.3 l.15-17 : maybe keep those last 2 sentences for the discussion and mention it more shortly at this stage because this is hard to understand without the whole paper in mind.

<u>Theoretical background</u>

p.3 l.21-22 : very redundant with p.1 l. 20-21.

p.4 l.5 : why “in contrast”? Is the exponential approximation only valid for large *r* values?

p.4 l.14: use $m^2\ kg^{-1}$ instead

p.4 l.24-28 : consider mentioning the topological dimension of the mean Gaussian curvature only in the discussion, because at this stage the reader does not understand the point.

p.4 l.26: the mathematical notation is not clear. Maybe use dS or dA to explicitly state that this is an average on the surfaces? This integration element could also be moved after the integrand.

p.4 l.27: *that* the local. Why is local in parenthesis?

p.6 l.10: detail why *z* is actually small and mention in which conditions this theoretical framework is valid. This in in fact detailed below, but inverting the order might be helpful.

p.6 l.13 : to *the* theory of

p.6 l.14 : it's 4π rather than 2π.

p.6 l.20: state here that the following sections investigate this issue and try to find a geometrical meaning of this second moment.

p.7 l.2 : would it be useful to briefly define the surface-void correlation function? Otherwise

p.7 l.4 : please clarify the meaning of “this is not a practical limitation”

p.7 l.1-7: since eventually the relation of Roberts and Torquato (1999) is not used, this part adds noise to the paper. Consider removing it (or mention it more concisely) if indeed it is not used.

p.7 l.12: not clear why you keep going while snow is clearly not a dilute medium. If the relation actually holds for snow (which seems to be the case as you show its consistency), state there that you demonstrate its validity for snow.

p.7 l.15: it seems that integrating by parts result in a factor $\left[l \frac{dA(l)}{dl} \right]_0^\infty$. Why is it equal to 0? True for the exponential case. Idem for p.7 l.18

p.7 l.20 : the expansion is only valid for small *r* values, while here the integration goes much beyond.

p.7 l.20-24 : This paragraph somehow adds noise to the flow of the paper. Would it be problematic to make it shorter and simply state that in Eq. (15) the integral is a function of λ_1 and λ_2 and must be of “length” dimension? I think this would not change the use of this equation later on (section 4.4). This approach would also allow the use of a constant term in the fit of Eq. (21) without further justification.

Methods

p.8 l.4 : More details about the preparation of the samples should be provided, and the isotropy of the prepared samples should be discussed. If for instance some samples obviously do not follow the isotropy requirement (e.g. depth hoar) they should be removed from the analysis.

p. 8 l.10 : the point regarding voxel size is very critical because the length scales are similar to voxel size, implying potential impact of voxelisation on the results. Can images at 18 and 50 µm be compared? See specific comment 3.

p.8 l.11 : before averaging, an evaluation of the anisotropy (or isotropy) should be given, because the whole theoretical framework is based on the isotropic hypothesis.

p.8 l.15 : Figure 1b does not really illustrate the exponential regression

p.8 l.23 : the meaning of “in view of shape” is not clear.

p.8 l23-25 : state more clearly that the section aims at validating the Eqs (6) and (8) by computing the interfacial area and interfacial curvatures.

p.8 l.30 : could this smoothing parameter be slightly more detailed, because it seems critical in the following section. What's the typical range, what values were used in the past? For what kind of applications?

p.9 l.4 : for S = 200, the interfacial area is larger, but the points seem also more spread, which is not discussed.

p.9 l.6-11 : what is the objective of this section? Does it serve the paper? Should it be used to support the isotropic hypothesis?

p.9 l. 16 : one should be with superscript “cf”

Figure 2 (bottom left) : there seems to be 2 sets of points, one consisting of RG. Could this observation help interpreting the limitation of S = 50?

Figure 4a : there seems to be 2 sets of points. Do they correspond to similar subsets of μCT images? The same 2 sets are observed in Fig. 6a
Figures 4b and c : DH is clearly an outsider here. Is it relevant to keep it in this study?

Results

p. 11 l.11 : one extra “and”
p. 11 l.11 : is it consistent to have a R^2 less (0.731<0.733) for the regression with an additional parameter?

p.13 l.1 : the name of λ_1 should be consistent between titles of sections 4.1 and 4.2. In section 4.1, optical diameter is not mentioned except in the title.

p.13. l.7 : I don't really understand this justification and don't think this is necessary. I would proceed the other way round instead. The figure 4b could be discussed at the end of section 4.1 with the aim of understanding the remaining residuals. This would naturally lead to the regression Eq. (19).

p.13 l.13 and 14: Eq. (14) instead of (16)

p.14 l.3 : Eq. (15) in stead of Eq. (14)

p.14 l.17 : here you try “heuristically” a regression, which is fine. This somehow contrasts with the previous regressions that were based on the derivation of equations. This could also be motivated by the form of Eq. (13) that includes the porosity factor. I think there is no problem assuming a relation, and then testing its validity with measurements. This is sometimes easier to understand than long inexact derivations.

p.14 l.12 : it is awkward to read that the benefit is small but to see the new regression, though. I would put it more positively: “The correlation coefficient (R^2=0.295) is small but including λ_2 in the analysis further improves the fit”.

p.14 l.24-25 : this is sometimes disturbing to read “correlation length” at some point and “exponential correlation length” later on. Please remain consistent throughout the manuscript, with each quantity (ξ, λ_1, λ_2) having its dedicated and constant name. Consider using “exponential” for the first part of the sentence, and “correlation length scales or Porod length and curvature lengths (for instance)” for the second part, to make the link with Eqs. (19) and (23) more obvious.

Figure 6 : remove “see”. λ_1 is not the optical diameter.

Discussion

p.16 l.2 : in complement to this discussion, this might be worth giving the sensitivity of Eq. (16) to the smoothing parameter, and possibly to the voxel size as well, if this makes sense.

p.17 l.5 : remind what grain size is because a_1 is the coefficient for λ_1 (which is optical diameter or grain size?)

p.17 l.6 : again depth hoar could be removed from the analysis if it does not satisfy the conditions of the theoretical framework.

p.17 l.7 : this is not clear what is also shown by those data. That the coefficient is larger for depth hoar?

p.17 l.21 : Eq. (7) instead of Eq. (1)
p.17 l.32 : there *were* attempts

p.18 l.5 : why is “independent” in italic. Idem for p.18 l.15 “if”
p.18 l.5 : where does this K/3 come from? It is K/24 in Eq. (8)

p.18 l.12 : this point is interesting, but puzzling as well. Indeed, from an optical point of view, a polydispersion of spheres will have the same “shape” parameters as a monodispersion in the geometrical optics approximation (and for low ice absorption), because B and g primarily depend and the shape, not on the size. Hence polydispersion would affect curvatures, but not grain shape as defined from an optical point of view. Said differently, a polydispersion of spheres will have optical properties similar to a monodispersion with same SSA, but different microwave properties.

p.18 l.32 : for such *a* system?

p.19 l.10 : wavelengths (in a single word?)

p.19 l.12 : the mentioned paper rather suggests that *g* for spheres is larger than *g* for snow, and that *B* for spheres is smaller than *B* for snow.
p.19 l.12 : the superscript *G* for the *g* refers to “geometrical”, that does not account for the diffraction contribution to scattering. This does not change the sentence but should remain consistent throughout the paper.
p.19 l.12 : it depends on shape rather than includes it

p.19 l.16 : it's 4π rather than 2π. By the way this quantity was already defined p.6. Then check the values for the following text and those shown in Table 1.

Table 1: Fraction of second to first rather than first to second order. Precise that mean and standard deviation are among all samples. Write 170 rather than 1.7×10^2. The values suggest no influence of shape at 0.9µm, which is consistent with the remark p.18 l.12. Note that eq. (5) of Malinka (2014) shows that at weakly absorbing wavelengths, *B* only depends on the real part of the refractive index.

This latter point should be further discussed to explore the validity of the random medium assumption used by Malinka (2014). In fact, this framework suggests that as long as the structure is random, shape has no impact on optical properties. This is contradictory to the fact that in the particulate representation of snow, different grain shapes result in different optical properties, even at low ice absorption.

p.20 l.6 : the authors decide to emphasize the parameter *B*, but in fact eq. (60) of malinka (2014) can also be used to express *g* in terms of λ_1 and λ_2. This should be done to complete the analysis.

p.20 l.7 : why is the parameter *B* shown in terms of this ratio? Is there supposed to be a visual correlation in Fig. 8? Why is the regression with respect to this particular ratio?

p.20 l.9 : Libois et al. (2014) experimentally determined the parameter *B* for a large set of snow samples and suggest *B* equals 1.6 ± 0.2. This comparison completes that with Libois et al. (2013). Note again that the range obtained in Fig. 8 results from the impact of shape at 1.3µm. This range can hardly be compared to that obtained by Libois et al.(2013,2014) obtained at visible wavelengths. The absolute values can on the contrary be compared.

p.20 l.9-12 : these sentences are not clear, and reference to Haussener et al. (2012) is very fuzzy, in particular the “remaining discrepancies”.

p.20 l.15 : *involved*

p.20 l.20 : this is the very critical assumption that should be further discussed

p.21 l.l.1-16 : this part shows is partly redundant with previous parts of the text. This could be shortened.

p.21 l.11 : why is this work mentioned here and not before? Could this help to establish the semi-heuristical relations displayed all along the manuscript?

p.21 l.12-14 : Why is the variance of the chord length distribution mentioned here for the first time?

p.21. l.19 : remove parenthesis in reference

Conclusions

p.21 l.29 : extra “we”
p.21 l.29 : consider adding (λ_2) after size metric

p.22 l.9 : the meaning of “when compared to” is not clear
p.22 l.9 : Maybe say : “The consistency between *B* values derived from the chord length distribution and those determined from optical measurements suggests such an approach is indeed possible”.

Appendix

p.22 l.28 : no parentheses for the references

p.23 l.8 : *by* the Swiss...

References:

Haussener, S., Gergely, M., Schneebeli, M., & Steinfeld, A. (2012). Determination of the macroscopic optical properties of snow based on exact morphology and direct pore-level heat transfer modeling. *Journal of Geophysical Research: Earth Surface*, *117*(F3).

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Malinka, A. V. (2014). Light scattering in porous materials: Geometrical optics and stereological approach. *Journal of Quantitative Spectroscopy and Radiative Transfer*, *141*, 14-23.

Picard, G., Arnaud, L., Domine, F., & Fily, M. (2009). Determining snow specific surface area from near-infrared reflectance measurements: Numerical study of the influence of grain shape. *Cold Regions Science and Technology*, *56*(1), 10-17.