Dear Quentin Libois,

Thank you for your very comprehensive and careful review and the overall positive opinion. We will address your comments point by point below, comments are copied and replies are given in blue. Changes to the manuscript will be made available by track-change pdf.

Kind regards,
Quirine Krol, Henning Löwe

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:

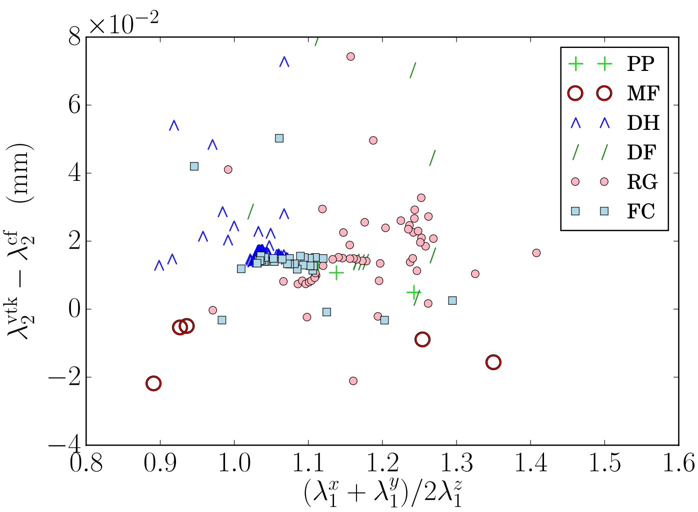
The theoretical framework presented in this paper strongly relies on critical assumptions that are not sufficiently discussed, although several important results largely depend on them.

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.

Reply:
We probably did not sufficiently elaborate on that point. To begin with, it is important to note that, strictly speaking, our analysis does not *assume* isotropy. We rather employ (wherever necessary) orientational averaging to reduce the information that is eventually used for the analysis. The geometrical interpretation of the involved quantities does not rely on isotropy. As an example, the relation between the slope of the correlation function via λ_1 and the surface area hold also (rigorously) for arbitrary, anisotropic systems, *after* orientational averaging (Berryman.1987). The same is likely true also for λ_2, namely that the orientational average of the third derivative of the correlation function of an anisotropic system is related to interfacial curvatures in the suggested way. We did not find a mathematical proof of the latter statement in literature, but our comparison of λ_2 (obtained from the correlation function, orientationally averaged) with λ_2 (obtained from direct computation of the interfacial curvatures) strongly suggests its validity. As an additional confirmation, we checked (plot below) that the remaining scatter is not caused by anisotropy, by plotting the residuals between the estimate λ_2^{vtk} (where anisotropy does not play a role) and λ_2^{cf}, which is not correlated with the anisotropy (R^2=.026). Accordingly, we also use the other length scales in the meaning of orientational averages, of arbitrary anisotropic systems. For the exponential correlation length this has been done similarly before. That said, none of the samples must be discarded.
Criticality of this procedure (not assumption) can only be revealed by measurements that will decide about the relevance of these orientationally averaged length scales for a measurement of anisotropic nature.

Changes to the manuscript: We add a part to the Discussion that discusses the anisotropy and retrieval of the parameters, however without showing this plot.



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.

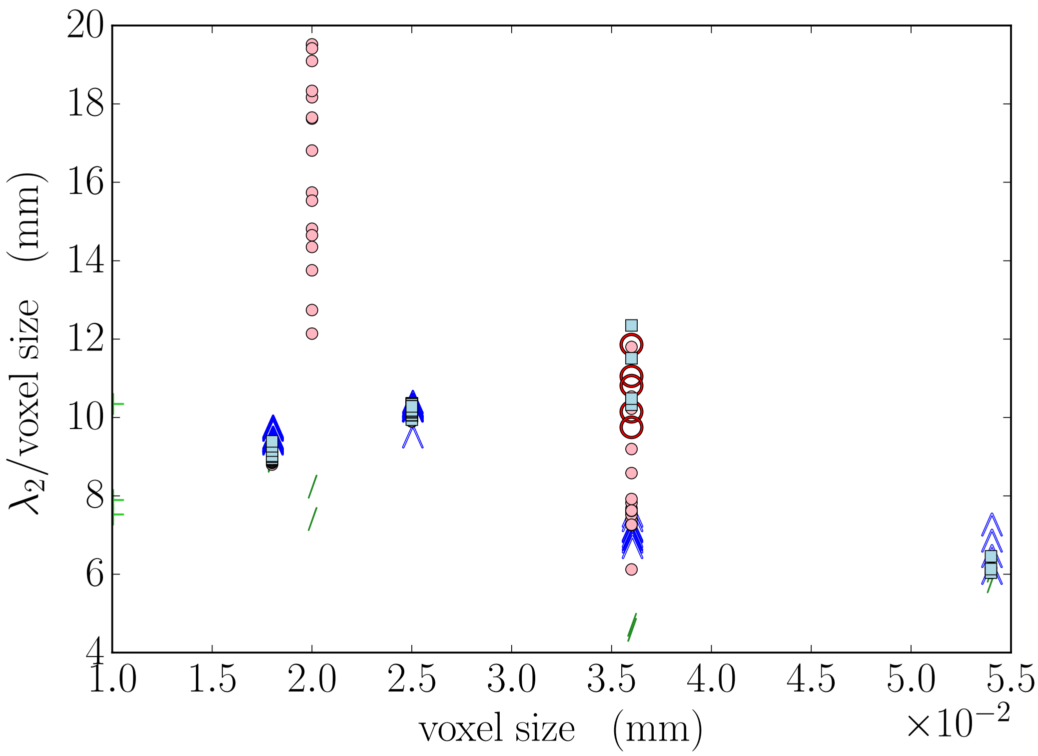
Reply:

We agree that for this step of deriving the shape parameter B from Libois 2013 by using Malinka 2014 involves a particular assumption about the independence of chords and adjacent surface normal orientations (note that our analysis of the statistical links between the length scales is however not affected by this) This issue was also brought up by the other reviewer, however rather pointing out the assumption of low absorption underlying Libois 2013 (which in contrast does not affect Malinka 2014). We are thus faced with the situation of linking two models/expressions that are based on two different, disjunct assumptions. That said, it is not entirely correct of using the closeness of the values found here to the values from Libois 2014 to confirm that the assumption of independent chords is not very restrictive. This aspect is now explained in more detail when deriving and discussing this connection. In the end (to produce Fig8) we evaluate B in the limit of low absorption (to cope with Libois). It is important to note that the assumption of independent chords (used in (Roberts and Torquato) already mentioned now in the paper) is slightly different from the assumption used by Malinka 2014. This will be also made clear.

Changes to the manuscript: The derivation of B is extended g^G, and the assumptions are discussed.

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.

Reply: We agree that the possible impact of the resolution could influence the results if the obtained quantities of interest are within a similar range. In general, the choice of resolution for CT images is made in accordance with the structure, such that the sample/resolution is statistically representative for the main quantities of interest (density and specific surface area). To assess the reliability of the obtained results we have compared them to the alternative VTK based method, for which we find very similar results. If the values for s are compared to the values that are obtained by the vendor software, we also see a good agreement with the VTK based method. To further confirm that the main quantity λ_2 is not systematically affected by image resolution we have plotted below the ratios of λ_2/voxelsize as a function of voxelsize, which are on average 9.8 with a standard deviation of 2.6. Only two samples have ratios 4.5 and the rest is 6.0 and higher. The correlation with the voxel size is R^2=-.20, but overall there is no systematic trend in λ_2/voxelsize for lower resolution (which would indicate a worse representation of the characteristic scales).



Changes to the manuscript:
We added a sentence on the spatial resolution of the data sets, its general importance and added the values for the characteristic ratios λ_2/voxelsize (the plot is however not included)

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 modelling are.

Reply: We agree that this extension to g (or g^G) is worthwhile for a comparison to Libois.2013. We replaced figure 8 by a plot of 1-g^G versus B (similar to libois.2013).

Changes to the manuscript: Table 1 is extended and Fig.8 is replaced by the a plot of B versus 1-g^G

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.

Reply: We agree, this is also in accordance with a suggestion of the editor. As suggested, the definition of the Euler characteristic in the theory section is left out left out, since it is not explicitly required. The Discussion section is restructured. It discusses first the methodology, including resolution, anisotropy, and the geometrical interpretation of λ_1 and λ_2. Afterwards, that the statistical models are discussed. We finalize it by discussion grain shape, including the connection to (Libois.2013,2014) and (Malinka.2014).

Changes to the manuscript: As indicated above.

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 attempts to justify this equation are unnecessary

   Reply: We agree. This is also in accordance with the other reviewer. These points are left for the discussion.

   Changes to the manuscript: The motivation for eq.15 is removed and this section is reformulated.

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.

   Reply: This was basically an attempt to stick to the names previously used in literature. But we agree, naming is now consistent and less ambiguous: λ_1 is named the Porod length, λ_2 is named the curvature length, the name for ξ, the exponential correlation length, remains. For μ_1 and μ_2 we stay with the first and second moment of the chord length distribution.

   Changes to the manuscript: The naming is made consistent throughout the manuscript.

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

   Reply: We agree, see comment 5).

   Changes to the manuscript: The discussion rewritten, taking all comments from both referees into account

Technical comments:

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"
We agree (maybe) to be discussed.

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 modelling 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"
Reply: We agree.
Changes: The sentence is changed to "the exponential correlation length is the relevant quantity in most snow microwave models".

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.
Reply: We agree.
Changes: "To facilitate forcing of microwave models by optical measurements" is replaced by "to derive input quantities of microwave models from optical measurements".

p.1. l.3: "the understanding of ξ" is vague. Simply say "To refine this relation between...]"
Reply: We agree.
Changes: the sentence is adjusted to "To refine the relation between…"

p.1 l.5: it is a statistical relation more than a prediction
Reply: We agree.
Changes: "Prediction" replaced by "relation".

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.
Reply: We agree.
Changes: Deleted**.**

p.1 l.10 : B is called the absorption enhancement parameter. Consider doing the same calculations with g.
Reply: We agree.
Changes: The parameter g, and therefore g^G, can be directly inferred from (Malinka.2014). This is added to the analysis and abstract.

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".
Reply: We agree.
Changes: Last sentence changed to say "Our results suggest that optically derived shape parameters can be used to refine the estimation of ξ".

Introduction

p.1 l.16-19 : maybe invert the order of the two sentences to keep chronological order
Reply: We agree.
Changes: The sentences are inverted.

p.2 l.4 : "with the MEMLS model" instead of "is used"
Reply: We agree.
Changes: Adjusted accordingly.

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.
Reply: We agree.
Changes: Changed.

p.2 l.16 : reference to Picard et al. (2009) might be relevant
Reply: We agree.
Changes: Reference is included.

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.
Reply: Thanks for pointing this out; we have not been aware of the paper.
Changes: Sentence on the measurement of B is added, including the citation. The discussion of B comes back to this point.

p.3 l.1 why "systematically?"
Reply: No specific reason.
Changes: Systematically is deleted.

p.3 l.12 : not clear what "images" you're talking about
Reply: We agree.
Changes: "images" is replaced by "µCT images".

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.
Reply: We agree.
Changes: Rephrased

Theoretical Background

p.3 l.21-22 : very redundant with p.1 l. 20-21.
Reply: We agree.
Changes: The sentence has been reformulated.

p.4 l.5 : why "in contrast"? Is the exponential approximation only valid for large r values?
Reply: We agree. The exponential approximation is of course based on a fit for *all* r.
Changes: We removed "In contrast" from the sentence.

p.4 l.14: use m2 kg-1 instead
Reply: We agree.
Changes: Adjusted accordingly.

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.
Reply: We agree.
Changes: Removed and included in the discussion.

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.
Reply: We agree. The reference to the Euler characteristic is however moved to the discussion.
Changes: Adjusted accordingly.

p.4 l.27: that the local. Why is local in parenthesis?
Reply: Local refers to the fact that the determination of this part of the correlation function is an average over nearest (or next nearest) neighbours (in the voxel images) which is commonly referred to as "local". This is contrasted non-local (i.e. long range) effect.
Changes: The sentence is moved to the discussion, and local is removed to avoid confusion.

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.
Reply: The text could indeed improve from reordering these sentences.
Changes: Reordered accordingly.

p.6 l.13 : to the theory of
Reply: We agree.
Changes: 'the' is inserted.

p.6 l.14 : it's 4π rather than 2π.
Reply: We agree.
Changes: Changed accordingly.

p.6 l.20: state here that the following sections investigate this issue and try to find a geometrical meaning of this second moment.
Reply: We agree.
Changes: A sentence is inserted.

p.7 l.2 : would it be useful to briefly define the surface-void correlation function? Otherwise
Reply: We won't go into the precise definition of the surface-void correlation function since it does not affect the understanding of the method. It seems however justified to mention it here since this part indicates the required effort to improve the relation between the two point correlation function and the chord length distribution to be valid not only for dilute systems (comment from the other reviewer).
Changes: No

p.7 l.4 : please clarify the meaning of "this is not a practical limitation"
Reply: This question is related to the more fundamental question about the validity of independent chords from point 2.
Changes: see point 2.

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.
Reply: We agree that we did not exploit this reference extensively. It is however crucial to comment on the assumption of the independence of consecutive chords.
Changes: The sentence is reformulated and used for a slightly different purpose (addressing point 2).

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.
Reply: We agree. (see also point 6). This point has been left out here since we come back to it in the discussion.
Changes: The section is cleaned up accordingly.

p.7 l.15: it seems that integrating by parts result in a factor [dA(l)/dl]. Why is it equal to 0? True for the exponential case. Idem for p.7 l.18
Reply: The two-point correlation (and thus its derivative) must go to zero for random systems for large r. Only in the presence of long range order (e.g. objects placed on a regular lattice) correlations persist to infinity (periodicity)
Changes: None.

p.7 l.20 : the expansion is only valid for small r values, while here the integration goes much beyond.
Reply: This equation is removed in the new manuscript, and therefore not discussed here anymore.
Changes: Revision of page 7.

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 $\lambda 2$ 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.
Reply: We agree. Thank you for this suggestion, which serves as the basis for the new formulation.
Changes: Revision of page 7.

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.
Reply: We agree that we could include more information on the samples that are used. Next to that, the isotropy (or rather absence of it) is mentioned. In the discussion session this is treated more extensively.
Changes: More information on the samples is given, and isotropy is shortly discussed.

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.
Reply: See answer to Comment 3.
Changes: We have discussed the effect of resolution in the methods and we come back to that in the discussion in more detail.

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.
Reply: see answer on point 2
Changes: see comment 2.

p.8 l.15 : Figure 1b does not really illustrate the exponential regression
Reply: In fact the formula that is used to create this figure is an exponential function. The illustration is a graph representing the involved parameters.
Changes: Figure is adapted with an illustration of the retrieval of λ_1 and ξ.

p.8 l.23 : the meaning of "in view of shape" is not clear.
Reply: We agree.
Changes: This sentence is changed to: "To confirm the geometrical interpretation of λ_1^{cf} and λ_2^{cf} we use an alternative and independent method to estimate these parameters by measuring the surface area and the local interface curvatures with a VTK-based image analysis. In short… "

p.8 l.23-25 : state more clearly that the section aims at validating the Eqs (6) and (8) by computing the interfacial area and interfacial curvatures.
Reply: We agree.
Changes: see previous changes.

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?
Reply: We agree. The smoothing parameter is a value for the number of times the Laplacian smoothing operation is applied. The smoothing has been discussed in (Krol.2016) and we adopted the same value for S here.
Changes: A short description of the filter is added.

p.9 l.4 : for S = 200, the interfacial area is larger, but the points seem also more spread, which is not discussed.
Reply: This is true. This is likely due to the fact that smoothing is filtering out small perturbations in the surface, reducing the area and increasing the values for λ_1. To which extend this happens is sample dependent, which causes the estimate for λ_1 to show a higher variance.
Changes: A sentence is added to clarify this.

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?
Reply: We partly agree. We removed the figure but we kept this small paragraph to elaborate more on the surface representation and smoothing. The factor of 3/2 has been the origin of quite some confusion in the past, and we would like to take the opportunity to mention and hopefully clarify this point.
Changes: Figure is removed.

p.9 l. 16 : one should be with superscript "cf"
Reply: We agree.
Changes: adjusted.

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?
Reply: In fact there are as many 'groups' of data as there are time-series present, which naturally show a pseudo- continuous deviation from the curvature estimates. The deviations from the 1:1 line are caused by the overestimation of the curvatures by the remaining steps in the triangulation from the underlying voxel-based data, and is thus anti-correlated with the size of the structures and correlated with voxel size. In the end we chose a smoothing parameter that is, on average, acceptable for all involved samples.
Changes: A sentence is added to the discussion to clarify this apparent grouping of samples.

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?
Reply: As explained above, these two sets of points are correlated since they are part of a time series. We will emphasize this when the samples are introduced in section 3.1. The depth hoar samples that show a higher deviation in Fig 4b and 4c, do not have particularly higher anisotropy values than the other depth hoar samples that do not have high residuals.
Changes: The data is introduced in more detail as well as the fact that some of them are part of a time-series. The anisotropy is discussed in more detail in the reformulated discussion.

Results

p. 11 l.11 : one extra "and"
Reply: We agree.
Changes: 'The' is deleted.

p. 11 l.11 : is it consistent to have a R2
less (0.731<0.733) for the regression with an additional
parameter?
Reply: Yes it is, since fitting eq.(18) includes two extra parameters which, if done correctly, should be accounted for in an adjusted correlation coefficient. Since a_0 is negligible to the fit this does not show in R^2 but it is however penalized in the reduced correlation coefficient.
Changes: we included "adjusted correlation coefficient".

p.13 l.1 : the name of λ1 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.
Reply: We agree.
Changes: The subtitle is changed from "Relating exponential correlation length to optical diameter" to "Relating exponential correlation length to the Porod length".

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).
Reply: We agree.
Changes: The order is reversed.

p.13 l.13 and 14: Eq. (14) instead of (16)
Reply: We agree.
Changes: Adjusted.

p.14 l.3 : Eq. (15) in stead of Eq. (14)
Reply: We agree.
Changes: Reference adjusted.

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.
Reply: We agree. In this section we changed the motivation for the statistical models involved.
Changes: We reformulated this sentence to "To motivate a statistical model we start from eq.(15) and test different expressions for $f(\varphi,\lambda_1,\lambda_2)$. Since f has dimension length a natural first candidate would be.. ". l.8: The sentence "Although not predicted from Eq.20…" is removed.

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 ($R2=0.295$) is small but including λ2 in the analysis further improves the fit".
Reply: We agree.
Changes: adjusted

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, λ1, λ2) 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.
Reply: See comment 7. To avoid confusion between the exponential correlation length and correlation length we stick to the term Porod length for λ_1.
Changes: Naming changed.

Figure 6 : remove "see". λ1 is not the optical diameter.
Reply: We agree.
Changes: Removed "optical diameter", changed to Porod length.

<u>Discussion</u>
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.
Reply: The smoothing parameter only influences the VTK-based parameters. The voxel size has an impact on the estimates of λ_1, λ_2, μ_1 and μ_2 and will be discussed in more detail.
Changes: Voxel size is detailed in the discussion.

p.17 l.5 : remind what grain size is because a1 is the coefficient for λ1 (which is optical diameter or grain size?)
Reply: We agree.
Changes: We adapted this sentence to "As a first step we have analysed the statistical relation between exponential correlation length and the Porod length. The latter is referred to as simply "grain size" or correlation length in Mätzler(2002)".

p.17 l.6 : again depth hoar could be removed from the analysis if it does not satisfy the conditions of the theoretical framework.
Reply: As discussed under point 1. Accordingly, we will argue in favour of keeping these samples.
Changes: None.

p.17 l.7 : this is not clear what is also shown by those data. That the coefficient is larger for depth hoar?
Reply: The results from Mätzler(2002) also distinguish depth hoar $\xi=.8\lambda_1$ and other snow types $\xi=.6\lambda_1$.
Changes: The sentence is changed to "Mätzler's model predicts a1 = 0.75, which is an average of $a_1=.8$ for depth hoar and $a_1=.6$ for other snow types. Comparing this to our result, $a_1=.79$, this is consistent since we have many depth hoar samples in the data set, which indicates an even larger influence of snow type or grain shape."

p.17 l.21 : Eq. (7) instead of Eq. (1)
Reply: We agree.
Changes: adapted

p.17 l.32 : there were attempts

Reply: We agree.
Changes: adapted

p.18 l.5 : why is "independent" in italic. Idem for p.18 l.15 "if"
Reply: We agree that it is not necessary to stress the words 'independent' and 'if'.
Changes: Adapted.

p.18 l.5 : where does this K/3 come from? It is K/24 in Eq. (8)
Reply: Yes, K/3 must be compared to $H^{2.}$
Changes: We adapted Eq(8) to 1/8(H^2-K/3) to make this obvious.

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.
Reply: We agree.
Changes: None.

p.18 l.32 : for such a system?
Reply: Yes.
Changes: Changed.

p.19 l.10 : wavelengths (in a single word?)
Reply: We agree.
Changes: wave lengths -> wavelengths.

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.
Reply: We agree. This is also consistent with the values we calculated for g and B shown now in Fig.7
Changes: Adapted.

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.
Reply: We will use consistently g^G and B.
Changes: notation adapted.

p.19 l.12 : it depends on shape rather than includes it
Reply: We agree.
Changes: include->depends

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.
Reply: Checked
Changes: None.

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 x 102.
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 absorptions

Reply:
We adapted the notation and description in Table 1.

We agree that Malinka involves a particular assumption on the independence of chords and adjacent surface normal orientations. This apparently leads to $B=n^2$ in the limit of very small alpha. This is now explicitly shown in the appendix. We also calculated there the next order correction in alpha that shows a slight dependence of B on shape if the latter would be defined only via moments of the chord length distribution. Accordingly, for visible wavelengths and corresponding alpha, no shape dependence of B would be predicted from A4, which is indeed not what is observed in nature. Thus it might be the case that, by using this independence assumption, some influence of shape on B is lost, in particular for for very low alpha (visible).

Changes: We included these points in the Discussion.

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 $\lambda 1$ and $\lambda 2$. This should be done to complete the analysis.
Reply: We agree. The analysis is extended to g
Changes: New figure with a plot of g versus B.

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?
Reply: This was done because eq.A4 is a function of $p(\alpha)$, and the ratio determines the relative importance of first and second order terms. However this figure is now replaced by a plot of g versus B. But the ratio can be also used as a simple proxy to assess the deviation of the snow chord length distribution from an exponential one (see comment 6 in the other review) Values are therefore given in the text.
Changes: Figure changed.

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.
Reply: We will emphasize the difference in the wavelength and discuss that in the weakly absorbing limit B is only depending on the real part of the refractive index. We will also point out that the apparent increased variation of B observed for visual wavelengths, may be due to the shadowing effect/density as discussed in (Libois et al (2014).
Changes:

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”.
Reply: we agree.
Changes: reference removed

p.20 l.15 : involved
Reply: We agree.
Changes: adapted

p.20 l.20 : this is the very critical assumption that should be further discussed
Reply: We agree. This assumption is indeed critical, but rather difficult to investigate. As explained above, we can only discuss this in reference to (Roberts and Torquato) who established an improved relation between the chord length distributions and the correlation functions. Their improved relation is still based on the assumption of independent chords. They tested this for level-cut Gaussian random fields, where successive chords are not independent from a rigorous perspective. The results however agree reasonably well, which is at least an indicator that this assumption is not so critical for this

aspect. As mentioned before, this independence assumption is however still slightly different from the independence assumption used by Malinka 2014.
Changes: This point is emphasized in the discussion which has been restructured.

p.21 l.l.1-16 : This part shows is partly redundant with previous parts of the text. This could be shortened.
Reply: This part of the text is replaced and rewritten to avoid redundancy.
Changes: Discussion is restructured and rewritten.

p.21 l.11 : why is this work mentioned here and not before? Could this help to establish the semiheuristical relations displayed all along the manuscript?
Reply: This relation is introduced in the discussion since it only explains that the slope in the origin of the chord length distribution is related to λ_2. While this shows yet another connection between chord lengths and the curvature lengths, worth mentioning, we were not able to put this on more general grounds which could be exploited earlier.
Changes: None.

p.21 l.12-14 : Why is the variance of the chord length distribution mentioned here for the first time?
Reply: Because it emerges only here in this argument to connects mu2 to lambda_12
Changes: In the reformulated discussion the variance of the chord length distribution is left out.

p.21. l.19 : remove parenthesis in reference
Reply: We agree.
Changes: removed

Conclusions

p.21 l.29 : extra "we"
Reply: Yes.
Changes: Changed.

p.21 l.29 : consider adding ($\lambda2$) after size metric
Reply: We agree.
Changes: added

p.22 l.9 : the meaning of "when compared to" is not clear
Reply: we agree.
Changes: rephrased.

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".
Reply: We agree,
Changes: Changed accordingly.

Appendix

p.22 l.28 : no parentheses for the references
Reply: We agree.
Changes: parentheses removed

p.23 l.8 : by the Swiss...
Reply: we agree.
Changes: The typo is removed.

References by the referee:

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.

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Libois, Q., Picard, G., Dumont, M., Arnaud, L., Sergent, C., Pougatch, E., ... & Vial, D. (2014). Experimental determination of the absorption enhancement parameter of snow. Journal of Glaciology, 60(222), 714-724.

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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.

References by the author:

Berryman, James G. "*Relationship between specific surface area and spatial correlation functions for anisotropic porous media*." Journal of mathematical physics 28.1 (1987): 244-245.