Dear Aleksey Malinka,

Thank you for your detailed and careful review and the your generally positive opinion about the work. We will address all your discussion points in the following, comments are copied and replies are given in blue. We also included the additional comment we received by email. Changes to the manuscript will be documented by a track-change pdf.

Kind regards,

Quirine Krol, Henning Löwe

The article presents a study, important for optics and physics of snow. It improves our understanding of snow microstructure. The authors attract our attention to the importance of the third term of the expansion of the correlation function, related to the curvature of the air-ice interface. One of the achievements of the work is the correlations between the microstructure parameters, both short-scale and long-scale, which are established experimentally by investigating the snow samples.

There are some points to discuss.

1) The authors state that the second term in A(r) expansion (and therefore p(0)) is equal to zero and explain that this is a direct consequence of the interface smoothness. However, the widely used (e.g., by Debye) exponential function for A(r) has the obviously nonzero second term. At the same time, there are interface models, such as the Switzer model, that provides strictly exponential correlation function. Particularly, in the Switzer model the space is dissected by a set of random planes into random polyhedrons and the resulting polyhedrons are assigned to ice with the probability 1-φ, and to air with the probability 1-φ. This interface is not smooth: it has plane facets and sharp edges. Obviously, it doesn't match the morphology of aged snow, but fresh snow seems to be much closer to the Switzer interface than to smooth one, because of the facets and edges of ice crystals. With this in view and taking into account the importance of the exponential correlation function, it would be extremely desirable to discuss the facet-edge interface and its relationships with the smooth one

Reply: It is true that the second order appears theoretically if discontinuities in the structures such as edges and corners are present. Fresh snow, as we know, contains many of these features. The ability to detect this second order term and relate it to discontinuity features is however difficult due to image resolution and noise in the data. A theoretical sharp edge would be treated practically as a rounded edge, which likely shifts weight from the second to the third order term. The resolution of our snow samples is raised by the second referee (see comment 2) As discussed there, we only find a very weak bias of image resolution on the third order term. A second argument is given by the shape of the chord length distribution that tends to zero for small chords which is a direct consequence of the absence of the second order term in the correlation function by virtue of eq.(14).

Changes to the manuscript: In the theoretical section we have added a sentence that mentions the role of sharp edges of the fresh snow samples. We also added the discussion on image resolution in the discussion session.

2) The motivation of Eq. (15) looks invalid. In general, the integral of a function from 0 to ∞ is not determined by its behaviour at 0. More precisely, the authors say that "A(r) depends at least on two independent length scales, λ1 and λ2" and further "In the absence of other relevant scales…" But „at least" doesn't mean „only". It is obvious that, as λ1 and λ2 are the coefficients of expansion at 0, there are other terms and, hence, other independent length scales at the interval (0, ∞). Figure 1b clearly demonstrates the idea that the integral is not determined by the behaviour at 0, because the contribution of the function tail can be of any value. This note doesn't affect the further results of the work, because the authors show that short-length and tail scales must correlate and try to explain why. However, at the stage of Eq. (15) this statement looks ill-founded. Let me suggest the idea.

As the value of the correlation length ξ is derived from the fitting the correlation function by the exponential at the whole interval, the estimation

$$\int A(r)dr = \xi$$

looks much more reliable. Partially, this implication is confirmed by the fact that, when considering the correlation between ξ, μ_1 and μ_2, the obtained correlation coefficient at μ_2 is higher than that at μ_1. (Minor: the differential dr is missing in the integral).

Reply: We acknowledge the ambiguity in motivating Eq. 15 in the present form, and for that reason we abandoned this argument, as also suggested by the second referee (see his comment 6).
The proposed idea is an interesting alternative to define and measure ξ. For correlation functions that are strictly exponential this definition is equivalent. This is however more in the direction of the length scale required for the microwave scattering coefficient, where the relevant scale (raised to the third power) is the zero mode of the Fourier transform of the correlation function, i.e. the volume integral over the correlation function. We will however stick here to the more "traditional" definition and estimate ξ by fitting the correlations function as done in (Vallese 1981, Mätzler 2002, Calonne 2015, Proksch 2015, Löwe 2011,2013,2015)

Changes to the manuscript: We have changed the motivation of eq.(14).

3) Page 14, line 25: "In the previous sections we found a statistical relation between the exponential correlation length and the chord length moments on the other hand." I guess the authors wanted to say "between the geometrical scales λ_1 and λ_2 and the chord length moments," because the relation between the exponential correlation length and the chord length moments is considered just below.

Reply: That is correct.

Changes to the manuscript: We have changed the sentence accordingly.

4) Introducing the factor $1-\phi$ into Eq. (24) the authors go back to the length $\lambda 1$ in the second term by virtue of Eq. (13). This is worth to note. Also, with the factor $1-\phi$ in Eq. (23) the second term turns to μ_1. In the whole, it is worth to underline that λ_1 and μ_1 are always related with Eq. (13) and indeed μ_1 have the meaning of the optical size, being exactly $\mu 1 = 2d_{opt}/3$ independently of the snow density.

Reply: We agree that we should emphasize both, the μ_1 and λ_1 relation and its independency of the density.

Changes to the manuscript: We added a sentence in the theoretical section to emphasize the μ_1 and λ_1 relation and included the $(1-\varphi)$ term in the discussion.

5) Page 19, line 18-19. "The results in Malinka (2014) are mainly based on the Laplace transform of an exponential, $p(\alpha) = 1/(1+\mu_1\alpha)$, which only involves μ_1 (or the optical radius via Eq. 1)." This is not completely true, because the exponential law is considered only as an example, though very important one. I would just delete this sentence, because it doesn't carry important information.

Reply: We agree.

Changes to the manuscript: Deleted the sentence.

6) Page 19, line 20, table 1: "relative importance $\alpha\mu 2/2\mu 1$ of the second-order term compared to the first-order term in the expansion Eq. (12)." This value doesn't look very informative. I think that much more informative will be the value, proportional to the variance $\alpha\ (\mu_2-\mu_1^2)\ /2\mu_1$, because it will give the deviation from the exponential law.

Reply: We agree that the deviation of the exponential distribution would be illustrative here. If this deviation is defined by subtracting the two Taylor series up to the second order and normalizing by the first order term, we however end up with $\alpha(\mu_2/2-\mu_1^2)\ /2\mu_1$. Alternatively, the deviation from an exponential can be also characterized by the ratio $(\mu_2/2\mu_1^2)$, which is exactly unity for an exponential distribution. The values found here are considerably lower (this can be directly deduced from Fig. 8 of the present manuscript). Since this Figure will be replaced according to a comment from reviewer 2, the values of this ratio will be given in the Discussion. This also confirms what is already shown in Fig.5/Fig.8, namely that the chord length distribution of depth hoar is systematically closest to an exponential.

Changes to the manuscript: Table 1 is adjusted and the range of values for the ratio is given in the discussion section.

7) It would be nice to consider these relations taking into account the relationship between A(r) and p(l) in the general case of a dense medium, not restricted by the dilute one

Reply: We actually mentioned this point explicitly in the discussion. The work (Roberts and Torquato 1999) investigated this connection for Gaussian random fields, with good agreement over a broad range of volume fractions. This also indicates that the assumption of independence of successive chords (which underlies (Roberts and Torquato 1999) does not seem to be very restrictive. Their method however requires numerical Laplace inversion and the computation of another correlation function. For Gaussian random fields the latter is known analytically, but here it would require a considerable additional effort to introduce the relevant concepts and carry out the numerics, with almost no benefit for the established connections between the length scales.

Changes to the manuscript: The discussion has been rewritten and this point is made clearer now.

8) The point that was raised in the email discussion: you compare the expression A2 used by Libois et al., 2013 with the expression A3 from my paper (or eq. 23 in that numbering). But expression A2 (A1) is written for small absorption only, while eq. A3 is applicable to any absorption values. You can easily check this by the limit of strong absorption:
when $\alpha = \infty$ and $L(\alpha) = 0$, therefore $1-\omega = T_{out}(n)$ or $\omega = 1-T_{out}(n) = R_{out}(n)$, which means that all the light that goes into the particles is absorbed. This limit is not true for A2. For comparison you'd better take eq. 25 for small absorption instead of general eq. 23: $1\text{-w} = n^2\ \alpha\mu_1$ (in your notation) and easily find the B-factor $B = n^2 = 1.68$ at 1.3 um for ice. The deviations of B from this value demonstrate the difference between the models used by Libois et al. and the model of the random mixture.

Reply: We agree that the limiting case of α and to ∞ is not consistent in both expressions. However in practice we compare both expressions only in the limit of small α, for which both are supposed to be valid. This issue was brought up also by the second referee under point 2 and is further discussed there.

Changes to the manuscript: We clarified the underlying assumptions in the appendix and added necessary details to the discussion of the Figure in the discussion section.

References by the referee:

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