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The Cryosphere Discussions

Interactive comment on "A general treatment of snow microstructure exemplified by an improved relation for the thermal conductivity" *by* H. Löwe et al.

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Dear Florent,

thanks for your comments and suggestions. We hope to convince you that nothing has been swept under the rug which apparently requires more elaborate explanations for various statements. Our point-by-point replies are given below.

1 Major comments

Comment 1

In general, the strong contrast treatment (Sen and Torquato, 1989) provides an upper and a lower bound. If the phase contrast α becomes very small or very large, one of the bounds becomes degenerate and a priori less interesting. In our case $\alpha \approx 100$ and for $\alpha \to \infty$ the upper bound becomes infinite which has been discussed in (Torquato and Sen, 1990) which we cited on p. 4678, l.1 for that statement. Additional explanations will be added.

The confusion about "expression" vs "bound" seems to be a semantic issue. A bound always constitutes an approximate expression for a quantity. A prominent example is the equation $k_e = \phi_i k_i + (1 - \phi_i)k_a$ for the thermal conductivity which is an exact expression for a simple laminate. In fact, this expression is also a first order upper bound (Torquato 2002, Ch. 21.1). We will replace "expression" by "approximation" to avoid confusion.

The conclusion that the bound correlates linearly with the true conductivity is, from our perspective, a non-trivial result which indeed justifies some effort. To illustrate this: Predicting the conductivity in terms of microstructure is more or less a two step process: first, some relevant parameters have to be identified and second, these parameters have to be combined in a potentially non-linear way to yield an expression for the conductivity. This is exactly what the bound does: The bound suggests Q as a relevant parameter and it dictates how Q must be combined with the volume fraction ϕ to yield something reasonable. The linear relation found between the bound and the true conductivity implies that the *functional dependence* of the bound on the two parameters fairly well characterizes the nonlinear interplay between density and anisotropy.

This result is in fact surprising since low order density correlations do not capture long range connectivity properties ($\approx$ tortuosity) of the structure, which are generally believed to be crucial for transport properties. Loosely speaking, second order density correlations only characterize structural units and their neighbors. In contrast, third order density correlations include neighboring units and also next-nearest neighbor units. Eventually, only the entire hierarchy of correlation functions describes the structure completely. The question, how much correlations must be included depends on the true complexity of the microstructure and cannot be guessed in the first place.

The fact that there is an improvement by taking anisotropic two-point correlations into account and that this improvement is obtained only for one coordinate direction is first and foremost a finding of the present analysis. A look into literature however reveals that the difference in predictive power of a bound between different coordinate directions is not unexpected. It has been shown in (Torquato and Sen, 1990) that the sharpness of the bounds depends on coordinate direction. In particular Fig. 4 in (Torquato and Sen, 1990) reveals that in some cases the vertical conductivity is better characterized by the bound as the horizontal conductivity.

A more elaborate treatment would require to improve the analysis and calculate third order bounds which involve the three point correlation function S_3 of the snow samples (Eq. 6 in Torquato and Sen, 1990). Computing S_3 and evaluating the corresponding bounds is definitely a project on its own and beyond the scope of the present analysis.

Comment 2

The single length scale approximation (in terms of an exponential or Gaussian) for the correlation function in one coordinate direction is certainly the simplest approximation and should thus always be considered first. For snow this approximation, even neglecting anisotropy, has now been used for over 3 decades in remote sensing since (Vallese and Kong 1981). We have however shown in (Löwe etal 2011) that such a single-length

scale form is not valid, at least for isothermal conditions. We wanted to emphasize that we are aware of this fact, otherwise the present work would not appear consistently next to our previous results. Going beyond this approximation would require to find a physical model for the correlation function which fits isothermal and TG conditions likewise, which does not exist yet. We would however expect that the improvement caused by a better model for the two-point correlation function is minor compared to the improvement caused by the inclusion of higher order density correlations in the bound.

Comment 3

It was only implicitly stated that conduction through air space is included, otherwise the phase contrast parameter α defined as the ratio between ice and air conductivity would not make sense. Accordingly, the FE code computes conduction through ice and air with the usual continuity condition for the temperature and the normal heat flux at the ice-air interface as internal boundary conditions. We will give these details explicitly in the revised manuscript.

Comment 4

A needle probe measures the *geometric* mean of the vertical and the horizontal conductivity (Grubbe etal. "Determination of the vertical component of thermal conductivity by line source methods", Zbl. Geol. Palaont. Teil, 1, 49-56, 1983). This has to be contrasted to the *arithmetic* mean which is shown in the inset in Fig 1. Both cannot be compared directly. In addition, neither paper (Sturm etal. 1997, Domine 2011) provides a comparison of the scatter of the geometrically averaged conductivity and the vertical conductivity as a function of volume fraction. From our results we would however deduce that a (somehow) orientationally averaged conductivity of snow will show less scatter as a function of volume fraction than the vertical conductivity alone. A separate plot of the arithmetic mean of the conductivities will be given in the revised manuscript.

Comment 5

As pointed out in the introduction we focus solely on conductive transport and neglect latent heat which is certainly a limitation. However this limitation was likewise accepted in many previous studies (Calonne 2011; Kaempfer 2005; Flin and Brzoska, Ann. Glac. 49,17 2008). Addressing the impact of microstructure in the absence of latent heat provides an important reference because a general, theoretical analysis of the relevance of latent heat is not available yet. However, we would expect latent heat to gain importance only for low density snow where the conductive contribution of heat transport becomes small. We do not understand why latent heat should become predominant in depth hoar.

Comment 6

You are absolutely right, the volume of the sample is probably too small to completely suppress volume fraction fluctuations in the TGM experiment (no numerical issues!). However, the fluctuations are present in the simulations and likewise captured by the bound in the model (cf. Fig 3). The magnitude of volume fraction fluctuations can be read off the inset of Fig. 3 which is thus in the order of 10 %. We will point out the origin of the fluctuations in the revised manuscript.

Comment 7

The RMSE value will be given. Concerning the limitations for the horizontal conductivity we refer to our reply to Comment 1. But note, that in Fig. 2,5 we have not compared the simulation to the bare value of the bound. This is explained on p. 4681,l.5-6: The figures show a comparison of the linearly *corrected* bound (the final model, Eq. 5) with the simulated values. If simulations were compared with the bound directly there would be no crossing of the 1:1 line in Fig. 5. The figure caption “prediction of the bound” is probably misleading, will be reworded.

Comment 8

Note that it does *not* require to include the entire 3d microstructure, only the ratio of horizontal and vertical correlation lengths must be predicted. The present model for the correlation function is equivalent to a random arrangement of identical (overlapping or non-overlapping) spheroids (cf. Torquato and Lado 1991). To be consistent with this picture in a metamorphism model, it is probably sufficient to aim at a description of non-spherical “ice grains” in their nearest-neighbor environment, which seems feasible at least at a mean field level. These explanations will be included. The comment is however considered as an outlook, details are obviously not clear yet.

2 Minor comments

Title 4.1 Will be consistently changed to volume fraction.

p.4682,l.8 “Very tight” is not a very quantitative statement. But the discussion of bounds in (e.g. Torquato 2002 p.604) suggest that a significant improvement can be expected by using bounds with order $n \geq 3$. In other words, second order bounds are not "very tight". Will be rephrased.

p.4682,l.14 It is hardly possible to identify a descriptive classification as given in (Sturm and Benson 1997) with the evolution of Q.

p.4682,l.20 There were differences in the conditions, the temperature gradient for TGM-17 was 50 K/m and 100 K/m for TGM-2 which will be explicitly stated. Since initial volume fractions differ significantly, TGM-2 was probably subject to densification by settling.

p.4680,l.17 Ooops, we forgot to include the factor $1/\sqrt{N} \approx 0.08$ for all RMSE values. Thanks. Will be corrected.

p.4681,l.7 Will be corrected.

Interactive comment on The Cryosphere Discuss., 6, 4673, 2012.