The Cryosphere Discuss., 6, C2528–C2531, 2012
www.the-cryosphere-discuss.net/6/C2528/2012/

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.

F. Domine (Referee)

florent.domine@gmail.com

Received and published: 19 December 2012

Objectives of the paper

The purpose of this paper is to obtain parameterizations of snow thermal conductivity, and ultimately of other properties, by taking into account snow microstructure. Such parameterizations are expected to be better that usual ones based solely on density. The authors use 2-point correlation functions, and a simplified treatment of those, to obtain expressions of the thermal conductivity as a function of density and of an anisotropy parameter, Q. They compare the values obtained with those simulated from a finite element code using the snow tomographic images. They show that the vertical conductivities calculated using Q compares well with the simulated values. The comparison is not as favorable for the horizontal conductivity. The authors conclude optimistically as to the general use of microstructure, and in particular of anisotropy, to predict a wide range of snow macroscopic properties.

General appreciation

The idea underlying this paper is doubtless very interesting and quite promising. Proposing a simple and efficient parameterization of snow physical properties that has much less scatter than density-based equations is somehow the dream of many snow scientists. The ideas here is original, at least for snow, and publication would therefore be beneficial to the community. However, in its present form, the paper makes many (too many?) approximations, most of them without justification, and many obvious difficulties appear to be swept under the rug so that in the end this paper leaves a sense of frustration with the feeling that not all the story has been told. I almost tend to think that publication is premature and that further careful work is required before a robust paper can be written. I therefore recommend major changes before publication, as detailed below.

Detailed comments

1- The authors consider the lower bound for the conductivity. There is no information on the upper bound, which supposedly diverges, of which I am not fully convinced. Page 4678, line 22 the authors state “this constitutes our explicit expression for the effective conductivity” while talking about the lower bound. Why would the lower bound represent the actual quantity ? Some justification is mandatory. In the end, the authors just ascribe a linear relationship betweeen the lower bound and the true bound, which appears like a too simple conclusion of all their efforts, especially since this relationship works only for the vertical conductivity, and clearly fails for the horizontal one. Here I get the feeling that their treatment should be more elaborate. It works in some cases, not in others, and the explanation stated: “as a remedy, we shall address the available

TCD

6, C2528–C2531, 2012

Interactive Comment

third order improvement in future work" (p. 4684, l. 15) does not quite convince me and probably does not suffice.

2- Another approximation is with the treatment of the correlation function. The authors mention that they have shown earlier that C(r) cannot be described by a single length scale. Yet, they do focus on a single length scale. What is the possible impact of this approximation?

3- The authors compare predictions of their equation to numerical evaluations of the thermal conductivity using a finite element code. Do they take into account just heat transport through the ice matrix or also through air? This is important, because even though ice is much more conductive than air, pathways that are dead ends for ice conduction turn out to transport heat through air that connect 2 nearby crystals. The impact of this is detailed in Calonne et al. (Table 1). Please give details of what the code actually does.

4- Figure 1. RMSE is greatly reduced if directional average is used. Should I deduce from that that needle probe measurements, which average directions, should have a lower RMSE ? This is not what Sturm et al. (1997) and Domine et al. (2011) obtained. I recommend using a full figure for the inset, and perhaps having the fits of the z and average components together on one of these figures to facilitate comparison.

5- Another aspect that may limit the application to natural snow is that the authors neglect latent heat processes, which may be important, and even predominant for DH. In the end, all these processes come into play in actual snow and this is what snow models are interested in. Any comment on that?

6- Figure 3. I am suspicious about the high frequency density variations. I would think this is just computing errors. After all, there are errors in the voxelisation of images so there are errors on density. The volume used for the calculations may also be a bit small so that these density variations could just show random variations in ice mass distribution. Can you evaluate those? I cannot think of a simple actual physical process

TCD
6, C2528–C2531, 2012

that would lead to such “noisy” density fluctuations, except at a very local scale.

7- Figure 5. The correlation between simulated k_e,xy and the bound is not very good. Please give a RMSE value. Again, can you comment on why the agreement is not as good here? Also, since equations deal with a low bound, we do not expect the bound to be significantly higher than the simulation, and this happens for all k_e values >0.17. Any reason? The few lines of comment given in the discussion are not fully satisfying.

8- Is Including Q and C(r) into snowpack models realistic ? We would have to treat 3-D microstructure. Please comment on that.

Minor comments

Title 4.1. Density is mentioned but in fact the variable used is volume fraction. Please check consistency throughout.

p. 4682, l. 8. “Second-order bounds are generally known to be not very tight”. What do you mean “very tight”? Close to the true value? Should not that be mentioned earlier, when the authors take the low bound as the true value?

p. 4682 l. 14. Can the non-monotonic variations in Q be related to the changes in depth hoar structure in taiga snowpacks detailed by Sturm and Benson (1997) ?

p. 4682 l. 20. Q evolves orthogonally to density. In one TG experiment (figure 4), density is constant, while it shows variations in the other. Were experimental configurations different ?

P. 4680 l. 17: RMSE=0.32. Is that not 0.032 ? Please check RMSEs throughout.

p.4681 l. 7, replace k_ice with k_air

Replace monotonous with monotonic throughout

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