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The Cryosphere Discussions

Interactive comment on "3-D image-based numerical computations of snow permeability: links to specific surface area, density, and microstructural anisotropy" *by* N. Calonne et al.

Anonymous Referee #1

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GENERAL COMMENTS

The paper by Calonne et al. is continuing the trend in determining physical properties of snow and firn by direct numerical simulation. They use a fair number of snow samples (35) digitized by micro-tomography, a commercial software for the simulations, and calculate a relation between density, specific surface area and (scaled) permeability (which makes much nicer plots). What is new in this paper is the "measurement" of small scale anisotropy, which is not easily accessible by direct methods. The paper is an interesting example of the application of direct numerical simulation, but gives relatively little new scientific insight beyond well known facts about permeability in porous

media (N.B. there a dozens of paper in the geophysical literature on direct numerical simulation of permeability, most notably by Adler, Arns and Knackstedt, which are not even mentioned). It remains also rather unclear what is the wider objective, beyond the numerical exercise. It remains especially unclear how the important issue of anisotropy can be determined except by direct numerical simulation. The descriptive-methodological part in this paper is dominating. In addition, there are no direct physical measurements done and directly compared to their numerical solutions, a point which can also be criticized in earlier papers, but which should not be repeated ad infinitum. It would have been interesting to see, finally (after Freitag, Courville and Zermatten) a direct comparison of these numerical techniques. They are all within the range of parameterizations proposed - but the gain in knowledge is rather small. The (log) scaled permeability shows a good correlation with density, and this is indeed a nice results, as SSA_v and density are sufficient to estimate permeability within about a factor of 3. I consider this paper as an interesting methodological development, but not a paper to be considered to appear in a very high-ranking cryosphere journal.

SPECIFIC COMMENTS: The introduction starts after a very non-specific sentence with theory. Introduce the reader first to the subject (I think you can assume for given that readers of The Cryosphere now what are the constituents of snow), explain your hypotheses and objectives. Present the theory in the "Methods". The discussion of the literature is not complete, especially there is no inclusion of the direct numerical simulation of permeability in the larger field of porous media (where the French researcher P.M. Adler plays a leading role). The discussion of permeability in snow and firn is incomplete. Bader (1939) did much more than a few measurements of permeability, he actually showed some deep insight in the process. Please read his original contribution. He realized dozens of precise measurements which would well compare to the data presented here. He was also the first to propose permeability as a classificatory variable, almost identically as later done by Arakawa et al. (2009) (which was not aware of the work by Bader). The pioneering work by M. Albert et al. is also discussed superficially, as they made many detailed and precise measurements with a technically improved method. Is direct numerical simulation of permeability really able to deal with spatial variability typical for this property? How about thin layers of a few mm thickness (or even thinner)?

The samples investigated are mostly from cold-room experiments: do they really cover typical natural conditions, and to which degree? In addition, only one climatological type of snow (maritime alpine) was sampled - missing all tundra, taiga, and polar snow types.

The explanation of the numerical technique is very short, and possibly not easy to follow for readers of TCD. How is the periodic BC handled in the code? Are the samples mirrored at the BC?

Results and discussions are mixed in one section: it would result in a better structured and readable manuscript if these were separated. It would then also become more clear what the hypothesis of this paper is.

Concerning the correlation of K*: while it is always possible to calculate Rˆ2, the interpretation of Rˆ2 for log-transformed data as a measure of quality is not as easy, as the original values are clearly not normally distributed, but belong to a log-normal distribution. A confidence interval would require a bootstrap analysis, and this should be done here.

I also missed an error analysis: As K* is scaled by SSA_v, both errors on SSA and density contribute to uncertainty. It would be interesting to read more about this issue.

The conclusion that simple cubic packing of (equivalent?) spheres is well suited to come close to the permeability of snow is surprising. How do you built in anisotropy? How did you deal with a volume density below 0.5?

Concerning anisotropy, Luciano and Albert measured much larger samples. Although they point out that their sample were "visually homogeneous", this is unlikely to be the case for Summit firn, as shown by Dadic, R. et al. (2008),JGR, 113(D14),

doi:10.1029/2007JD009562. So what they measured, is actually a "macroscale anisotropy" and not the microscale anisotropy discussed here.

Finally, I found two conclusions confusing. First, the major conclusion "... the the permeability of seasonal snow, if assumed isotropic, can be reasonably inferred ...": but you should in the entire paper that the permeability IS anisotropic. So what do you conclude really? That is can be approximately estimated (we know that), but you have no handle on anisotropy. Second, the authors suggest that permeability may by useful for classification (as suggested by Bader and Arakawa). But on the next sentence it is written "... the high coefficient of correlation between K, SSA and rho ...", so then SSA and rho are AS GOOD as K, and there is no advantage of K?

TECHNICAL CORRECTIONS: All figures have close to illegibly small numbering of axes and parts of the legends.

Fig. 4: what are the "3 mm" indicating? Size of the bar? What magnitude and unit has the velocity?

Interactive comment on The Cryosphere Discuss., 6, 1157, 2012.