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Discussions

Interactive comment on "Seasonal evolution of snow permeability under equi-temperature and temperature-gradient conditions" *by* F. Domine et al.

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This paper presents an interesting contribution in the context of the emerging quantitative study of snow dynamics. It clearly shows that much experimental, theoretical, and modeling work needs to be done prior to get a convincing picture of the processes at work. This is well reflected in the conclusions.

Even though the results are not overwhelming, I recommend accepting the paper after the authors addressed the issues below and do some minor revisions as suggested.

A few points in details:

P2727,L11: "in the top few cm" or less according to Clifton et al., 2008 and Bartlett & Lehning, 2011

P2729,L9: "laterally homogeneous" formulation sounds strange (to me)

P2729,L17: "Equi-Temperature" the term may be quite correct in your case but it is still misleading in general. What about LTG (Low Temperature Gradient)?

P2731,L18ff: This looks somewhat contradictory to the description of the set up on page 2730, lines 5ff where you neglect the thermal inertia of the table?

P2732,L9ff: parameterization of snow viscosity: I'm surprised that your basic formulation should increase that much between -7 and right below -10 °C. Bounding the viscosity by its value at .7 °C may become a problem at much lower temperatures though (see Groot et al, doi:10.5194/tc-7-333-2013, 2013). Could you comment on that?

P2732,L289: What radiation measurements were used during the period of the experiment?

P2735,L20: You should use another symbol than K for the average permeability

P2736,L23ff: Even if it seems obvious, please state that these are modeled properties. The same applies to other parts in the text. Making it clear will help the reader.

P2737,L16: "individual layers were identified" what happens to this date when elements are merged?

P2737,L27: "..., the model results follows ..." you will need to carefully check the paper for such typos.

P2738,L25: I'm not really impressed by that result and ask myself whether we can be satisfied with it. It rather shows that there is still work to do (see page 2739, lines 27ff!)

P2739,L18: Could be termed "grain size" according to the ICSSG.

P2739,L20: "aerodynamic drag" I would call it "form drag" as in fluid dynamics

P2740,L25ff: Does this paragraph not rather belong to the introduction?

P2741,L4ff: Again, how deep wind pumping effects snow dynamics is still controversial. Are a few millimeters at most enough to draw that conclusion?

P2741,L15: Your measurements span only an order of magnitude in permeability an a limited density range (see Fig. 5). I wonder how well they really fit Eq. 3 over that range given the logarithmic scale of the ordinate?

Figures

Figs 1 & 2: I don't think you need to draw that many symbols here (see ICSSG, Fierz et al., 2009). Furthermore, why is there no layer boundary between DH and FC in the third layer of the 12 Feb 2004 profile in Fig. 1? All this may lead to over- and misinterpretations!

Fig 3: and others: "Height" would be better to label the ordinate. Furthermore, I wonder whether the dotted "guides to the eye" always work (see left panel)

Fig 7: Were negative temperature gradients never modeled?

Fig 7f: I only see RG appearing between periods of FC formation. Is this what you describe in the text?

Fig 8: logK would require a dimensionless quantity to be displayed.

Fig 9: Model results?

Figs 10 & 11: replace "profile measured" by "observed profiles". I wonder whether there are not too many different representations in that figure. Maybe the bars "with the resolution of the measurements" could be left out?