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

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1 General comments

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.

We thank Charles Fierz for his positive appreciation of our work. We acknowledge that there is still work to do in terms of observations and modeling of snow metamorphism in natural conditions, and feel useful to provide a status report on how current tools perform in this regard.

2 Technical correction

2.1 Text

P2727,L11: "in the top few cm" or less according to Clifton et al., 2008 and Bartlett & Lehning, 2011
References to Clifton et al., 2008 and Bartlett & Lehning, 2011 will be added.
P2729,L9: "laterally homogeneous" formulation sounds strange (to me)
By "laterally homogeneous" we mean that the snowpack structure was reasonably one-dimensional (i.e., low horizontal variability of snow layer heights and characteristics).
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)?
The abbreviation ET is widespread in the literature and corresponds well to our observations, i.e. that metamrophism produces crystals similar to what would be obtained without gradient. So our metamorphism is essentially ET, based on observations. As far as we know, LTG is not an accepted abbreviation and we do not feel that this contribution is the appropriate vehicle to introduce new terminology.
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?
We agree that the current formulation is contradictory. However, even without any thermal inertia of the table material, the temperature at the base of the snowpack is not strictly equal to air temperature, but rather results from the energy balance of the

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interface between air below the table, the table material, and the snowpack above. Performing this calculation is clearly beyond reach for our study, in part because many of the needed variables are not known (wind speed below the table, incoming long-wave radiation below the table etc.). Instead, we use a fixed-thickness layer of 10 cm of ground material to compute heat diffusion between air temperature and the bottom of the snowpack. The text will be revised to better reflect this approach.

P2732,L9ff: parameterization of snow viscosity: I'm surprised that your basic formulation should increase that much between -7 and right below -10C. Bounding the viscosity by its value at -7C 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?

The text will be edited to simply reflect the fact that the maximum impact of temperature on viscosity is a factor 2 difference. Allowing a larger impact leads to unreallistically low compaction rates.

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

Incoming radiation data were all taken from the ERA-Interim meteorological reanalysis ; no field measurements of these variables were performed during the field experiment. The text will be edited to make it clearer (deletion of the reference to a "spin-up" period).

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

Equation (5) is introduced to explain how the average permeability is computed from a sample made of layers of various permeability values. However, the notation used in Equation (5) is specific to this Equation and we do not see ho useful would it be to introduce alternative notations only for this equation.

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.

The text has been edited in many occasions to clarify this point.

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P2737,L16: "individual layers were identified" what happens to this date when elements are merged?
This sentence will be reformulated because the previous formulation was to not reflecting the method used in a clear manner. The proposed modification reads "These three variables were binned using snow layer age since snowfall with a bin size of one day, and the average and standard deviation were computed within each bin". The attached plot shows that the snow layer age evolves smoothly with time during a given model run, so that the impact of layer merge/split has a negligible impact on the results displayed in Figure 9 of the manuscript.
P2737,L27: " ..., the model results follows ..." you will need to carefully check the paper for such typos.
Most of such typos will hopefully be fixed in the revised manuscript.
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!)
The term "satisfactory" will be replaced by "encouraging". This is the first trial of a direct calculation of the time evolution of permeability of seasonal snow, and even if the agreement is far from perfect it could have been anticipated that the discrepancies would have been much larger.
P2739,L18: Could be termed "grain size" according to the ICSSG
This term will be replaced by "visually determined radius" consistent with the remainder of the manuscript.
P2739,L20: "aerodynamic drag" I would call it "form drag" as in fluid dynamics
This suggestion will be taken into account in the revised manuscript.
P2740,L25ff: Does this paragraph not rather belong to the introduction?
This paragraph draws on results from the present study, so moving it to the introduction would make it seem more speculative than it actually is. This is why we prefer to keep it at its current location.
P2741,L4ff: Again, how deep wind pumping effects snow dynamics is still

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controversial. Are a few millimeters at most enough to draw that conclusion?
This part of the manuscript does not introduce definitive statements but rather a discussion on open topics. We will add the following sentence to clarify our views on the matter "Since wind pumping (Colbeck, 1989; Cunningham and Waddington, 1993) on rough surface can show effect down to depth of 1 m (Albert and Schultz, 2002), this effect may be worth further consideration."
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?
Yes, the fit is not as good as the data of Calonne et al. (2012). This is because these authors made calculations on small samples while we made measurements on large multilayer samples, and permeability, density and SSA values do not apply to exactly the same samples because of different sampling requirements for each of these variables. It is therefore hard to get data in close agreement with the expected behavior, especially on the tables where metamorphism does not have the same homogeneizing effect as on ground, where everything was transformed into depth hoar. It is however interesting to note that our scatter is somewhat similar to that of Arakawa, who also did measurements on similarly large samples.

2.2 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!
Previous studies have used that many symbols. There is no clear limit between layers because the transition was so progressive that no layer limit was clearly visible.
Fig 3: and others: "Height" would be better to label the ordinate.

"Snow height" will be replaced by "height" when appropriate.

Furthermore, I wonder whether the dotted "guides to the eye" always work (see left panel)

This will be clarified in the revised manuscript.

Fig 7: Were negative temperature gradients never modeled?

What is reported here is the absolute value (or modulus) of the temperature gradient. This will be clarified in the text and in figure captions.

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

The text will be revised for enhanced clarity. It is now simply stated that both RG and FC appear in the table case, while in the ground snowpack DH are found. The fact that layers seem to experience RG in-between periods of FC simply stems from threshold effects in the definition of snow types from the continuous variables used in Crocus to describe snow microstructure.

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

K will be replaced by K/K^0, where $K^0 = 1\text{m}^2$, where appropriate.

Fig 9: Model results?

The caption will be edited to clarify that model results are indeed shown here.

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?

Figures 10 and 11 will be improved for better readibility.

Interactive comment on The Cryosphere Discuss., 7, 2725, 2013.

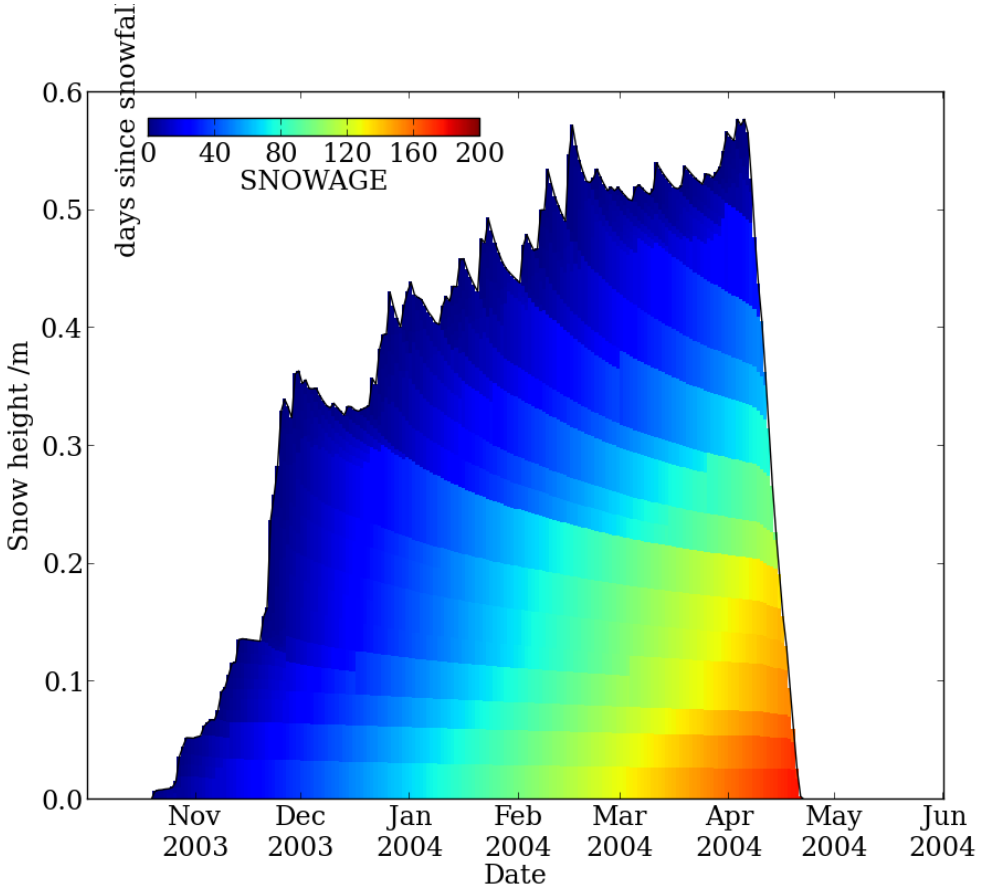

Fig. 1. Time evolution of the age (in days in snowfall) of numerical snow layers used to describe the vertical profile of the physical properties of snow by Crocus.

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