

Interactive comment on “Seasonal evolution of the effective thermal conductivity of the snow and the soil in high Arctic herb tundra at Bylot Island, Canada” by F. Domine et al.

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General comments

The is an interesting study, which shows in detail the enormous problems to measure, monitor and interpret thermal conductivity of snow under field conditions in the high arctic. The paper shows that large uncertainties exist in measurement and in the application of models, and that a continuous monitoring is difficult. In fact, the results suggest that simple density measurements and the now very well calibrated parameterizations, maybe a more feasible and precise way to observe the evolution of the snowpack. The interpretation of the measured thermal conductivities of the needle probe are in my view not always supported by other the other data presented in the

paper, as will be discussed below in detail.

The discussion on subnivean life is a bit out of focus in this paper, clearly an important aspect of the arctic snow cover, but in my view not the right place.

Specific questions:

The authors interpret thermal conductivities in snow around $0.02 \text{ W m}^{-1} \text{ K}^{-1}$ as snow conductivities (they mention that this is within errors the same value as in air). I believe there are two points not made clear: The snowpack, if the bottom layer would be air over an extended area, would immediately compact (in fact, avalanche formation mechanics gives an upper bound of about max. 1 m^2 air gap before an spontaneous collapse of the snowpack forms). The "close-to-air" values are therefore at least not spatially representative.

The inclusion of the soil in the interpretation is very useful, except that no detailed granulometric soil analysis seems to exist as this is a well investigated research site? More detailed data would clarify the observed behavior of the soil-freezing behavior. In fact, the observed curve indicates that the soil is not a silt, but a fine sand.

The authors put substantial weight on the effect of water vapor fluxes on the snow-cover. The explanation of the fragile depth hoar bottom layer, as well as the formation of indurated layers, is based on the interpretation of temperature and vapor pressure gradients. The calculation of the vapor flux is omitted with the argument that the diffusivity is not well known. Laboratory experiments and numerical simulations (Calonne, 2014; Pinzer, 2012) defined the diffusion coefficient precisely - in fact, due to the hand-to-hand process, the diffusivity in air is a very precise approximation. Approximate calculation for the season 2014-2015, with an average snow temperature of -30 deg C , temperature gradient 50 K m^{-1} , and a duration of 90 days, result in a mass flux of 0.24 kg m^{-2} . This flux seems to me too small to explain the observed processes.

Obviously, spatial variability of the thermal conductivity of snow can not be measured by

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permanent stations. However, as is obvious from the snow profile and the descriptions, spatial variability is an issue at the dm - m scale. As a suggestion, long snow profiles as done by Rutter et al (2014) in the arctic, or as demonstrated using a penetrometer by Proksch et al., would have contributed much to reduce the uncertainty in the measured values and their interpretation.

The use of needle probes as monitoring devices is strongly defended by the authors. However, a careful inspection of their Fig. 2 and Fig. 13 a) and calculating thermal conductivity based on the well accepted Calonne et al (2011) parameterization (or the Yen-parameterization) using the measured density, shows that the needle probes underestimate severely (for depth hoar a factor of about five) the effective thermal conductivity.

The numerical simulation using Crocus seems to have major problems with creating a realistic density profile. As no details are given, my conclusion is that severe deficiencies must exist in the model parameterization. I suggest that the model runs are checked by an expert, as they seem to me beyond any reasonable behavior, or this part of the manuscript should be deleted.

References Calonne, N., C. Geindreau, and F. Flin (2014), Macroscopic Modeling for Heat and Water Vapor Transfer in Dry Snow by Homogenization, *The Journal of Physical Chemistry B*, 118(47), 13393–13403, doi:10.1021/jp5052535.

Pinzer, B. R., M. Schneebeli, and T. U. Kaempfer (2012), Vapor flux and recrystallization during dry snow metamorphism under a steady temperature gradient as observed by time-lapse micro-tomography, *The Cryosphere*, 6(5), 1141–1155, doi:10.5194/tc-6-1141-2012.

Proksch, M., H. Löwe, and M. Schneebeli (2015), Density, specific surface area, and correlation length of snow measured by high-resolution penetrometry, *Journal of Geophysical Research: Earth Surface*, 120(2), 346–362, doi:10.1002/2014JF003266.

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Rutter, N., M. Sandells, C. Derksen, P. Toose, A. Royer, B. Montpetit, A. Langlois, J. Lemmetyinen, and J. Pulliainen (2014), Snow stratigraphic heterogeneity within ground-based passive microwave radiometer footprints: Implications for emission modeling, *Journal of Geophysical Research: Earth Surface*, 119(3), 550–565, doi:10.1002/2013JF003017.

Technical corrections

I 200 These values are questionable based on the authors density measurements. If there is no heat conducting matrix, there is no mechanical (compressive) strength.

I 230 Did the authors any calibration of the temperature and soil humidity sensors before or after the deployment?

I 283 "Rise" -> rise

I 290 The same limitations concerning vapor flux are valid also for convection (if there is any with the measured snow profile). In my view the speculation is out of place.

I 317 ff The fluxes are easy calculate, this section should be rewritten in view of the actual fluxes.

Almost all Figures: The time axis is lettered in French, not English

Fig. 1 The appearance of the vegetation in the photo seems to involve some vertical structure, completely flattened out during early winter? Not unimportant for the interpretation of the depth hoar formation.

Fig. 2 The symbol for melt-freeze indurated depth hoar is actually defined (Int. Class., p. 19, a lying "8" with depth hoar symbols inside)

Fig. 3 Snow depth or snow height. Caption, text and axis are not consistent (also Fig. 6)

Caption Fig 3: where there no easy measurements of snow depths around the stations

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to know spatial variability around?

Fig. 7 The measured thermal conductivity data are inconsistent with the density profile.
Give error bars.

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