Reply to Martin Schneebeli

The Reviewer's comments are in black, and our response is embedded in the text, in blue italics. Line numbers refer to those of the version in track changes mode.

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.

Thank you for this overall positive comment. We however do not agree with the suggestion that density measurement and the use of density-thermal conductivity correlations would be a good method to estimate thermal conductivity. We discuss this below with the relevant comment.

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.

This is a matter of view point. The reviewer is an Alpine snow scientist and some of his main interests include avalanches and snow microstructure. We focus on Arctic problems and subnivean life is critical. In fact, although the main motivation of this program is permafrost thermal regime, another important objective is subnivean life. Please remember that lemmings form the very base of the terrestrial food web (Gauthier et al., 2011) and understanding their life conditions is critical for any Arctic wildlife dynamics consideration. Most Arctic snow scientists always have subnivean life in the back of their mind, but on the other hand very few ever think about avalanches. We do intend to use snow thermal conductivity as a proxy for lemming-relevant snow properties, as explained in our paper (lines 63-64) and we therefore feel this discussion fully belongs to this paper. In fact, a very recent study by (Fauteux et al., in press) shows population variations between 2014 and 2015 that appear consistent with our snow observations and we have added a short paragraph in the discussion to mention this (lines 433-438).

———————————— **Specific questions:**

The authors interpret thermal conductivities in snow around 0.02 W m^{-1} 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 mˆ2 air gap before an spontaneous collapse of the snowpack forms). The "close-to-air" values are therefore at least not spatially representative.

The reviewer probably misunderstood our statements. We say clearly in the results section that these values are for depth hoar and that they are a bit low and close to that of air because of the negative artefact due to the use of the NP method. Our lines 227-229 read: "k_{snow} dropped to values around 0.02 W m^{-1} K^{-1} because of rapid depth hoar formation. These values may seem a bit low, especially considering that air has a thermal conductivity of 0.023 W m^{-1} K^{-1}, but the low value can be attributed to a negative systematic error of about 20% caused by the NP method, as described in (Riche and Schneebeli, 2013) and discussed above."

Regarding the presence of air at the bottom of the snowpack, the reviewer probably misunderstood our statements. We never say that air is continuously present over extended areas. Rather we clearly show the discontinuous nature of these air gaps in Figure 8. Furthermore, spontaneous collapse of the snowpack is predicted for Alpine conditions where snow is thick and the overburden significant. In the Arctic, very different conditions prevail with a thin snowpack (as clearly stressed in Figures 2, 3, 6 and 7) and hence a much lighter overburden. Finally, the values measured are for snow and definitely not for air. Measurements in air are almost always very easy to recognize: heating curves are erratic because of the complex and irregular convection that always takes place. This was alluded to in the methods section (lines 127-130) and we now further stress the point line 230 "It is fairly certain that this NP was not in an air gap, because measurements in air almost always produce erratic heating curves due to complex convection, and none of the heating curves were suspicious." We therefore believe that, even though there are clearly spatial variations, our values are representative and this is supported by the field data of Figures 2 and 7, with values similar to the automatic measurements.

The inclusion of the soil in the interpretation is very useful, except that no detailed granulometric soil analysis seems to exists 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.

This is an excellent point. Granulometric data was indeed lacking. We performed such an analysis using a Horiba partica LA-950V2 laser scattering particle size analyser. Data show a bimodal size distribution with modes centered at 17 and 59 µm. If the standard 50µm size limit between sand and silt is used, then our sample is 65% silt and 35% sand by mass. A subsection was added to our methods section to describe briefly the method (lines 171-174). The results are now mentioned lines 258-261.

The authors put substantial weight on the effect of water vapor fluxes on the snowcover. 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.

Thank you for this very useful comment and for taking the time to make the calculation. Using the data of Calonne et al., we have calculated water vapor fluxes and these are now shown in Figure 12. Our calculations lead to a total mass loss about 10 times greater than the Reviewer's estimate, because he used a temperature of -30°C while in the lower part of the snowpack, especially in early season, the temperature was around -5°C, so that the water vapor pressure was about 10 times greater. In any case, we agree that this is not quite sufficient to fully explain the total snow collapse. We therefore now suggest that diffusive fluxes alone cannot explain our observations, but convection and air advection (wind pumping) probably also took place and must have contributed to the mass loss, explaining the observation. The presence of convection cells is fully consistent with our observation of irregular collapse (Figure 8). This is now detailed in the discussion, lines 381-395.

Obviously, spatial variability of the thermal conductivity of snow cannot be measured by 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.

Indeed, more measurement would have been highly desirable, as always, but there is only so much we can do in a 10-day campaign with complex logistics, multiple objectives and occasionally uncooperative weather. We did however investigate the spatial variability of the density of the depth hoar layer as shown in Figure 2. Figure 8 a and b also illustrate this variability.

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.

Thank you for this interesting comment; however we have to disagree with the reviewer. First of all, we do not "strongly" defend the use of the heated needle probe. We are fully aware of its limitations and artefacts (thanks in part to the reviewer's publications, by the way). We only state that today it is the only technology suitable for the continuous monitoring of snow thermal conductivity, but we'll gladly consider any alternative, when available. Second, we do not think that the parameterizations of either Calonne et al. or Yen et al. are well accepted. The parameterization of Calonne is based on 30 values and that of Yen on less than 60. Calonne et al. only measured Alpine snows and did not measure a single sample of low-density depth hoar. Most or all of the snows used by Yen are not Arctic snow. Given the huge difference between Arctic and Alpine or *temperate snow, of which most snow scientists are not fully aware, we feel that it is not reasonable to attempt to determine the thermal conductivity of Arctic snow from very small and not representative data sets. Calonne et al., on their Figure 1, also report the 500+ values measured by (Sturm et al., 1997), all of them on Arctic and subarctic snow. These clearly show the huge difference between (sub)Arctic snow and Alpine snow, and demonstrates beyond doubt that parameterizations not developed for Arctic snow simply should not be used for Arctic snow. The large data set of (Sturm et al., 1997) also shows that for a given density, the range of thermal conductivities varies by a factor of 5, so that density correlations cannot predict accurately Arctic snow thermal conductivity. We admit that needle probes may underestimate the thermal conductivity of depth hoar, and we did mention that in our paper, but this effect is simply not sufficient to warrant the use of the parametrization of Calonne et al. or Yen to understand Arctic snow thermal conductivity. We have added a paragraph to our discussion (lines 467-476) to detail all this.*

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.

Full details of the model are given in (Domine et al., 2016), so we do not feel it is useful to repeat them here. The model runs were in fact performed for that earlier paper, of which Samuel Morin is a co-author, and he has an extensive record of publications using Crocus. The problem is not the model parameterizations or our use of the model, the problem is that the model has been developed for Alpine snow and does not take into account vertical water vapor fluxes. To fully convince the reviewer that this is the problem, we asked our colleagues Alexandre Langlois and Jean-Benoit Madore (University of Sherbrooke) to perform runs with SNOWPACK, the detailed snow physics model of the Reviewer's institution. They used NARR forcing data. Figure 1 below shows data similar to those of Figure 13 in our paper, which we also show here to facilitate comparison.

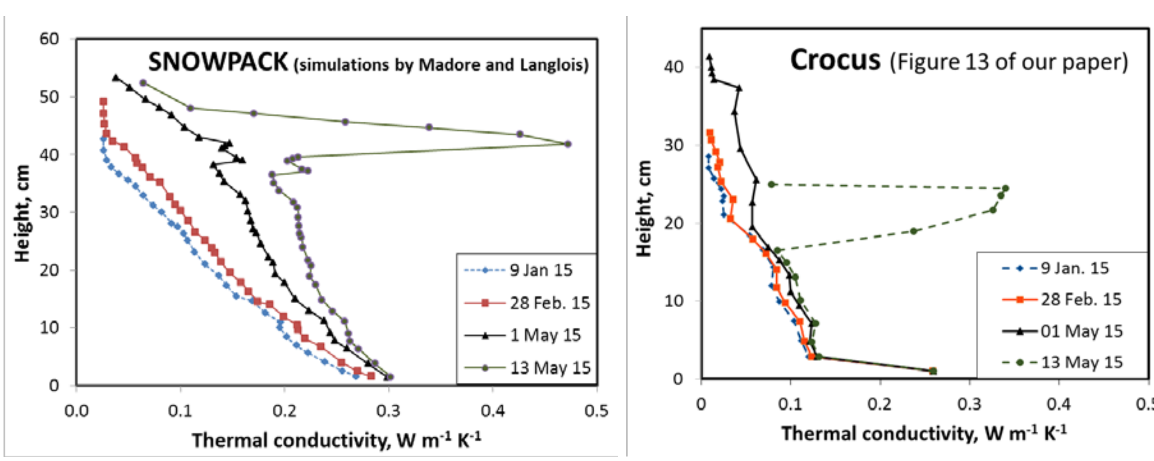

Figure 1. Simulations of snow thermal conductivity at Bylot Island using the SNOWPACK and Crocus models.

Neither Crocus nor Snowpack can reproduce the low thermal conductivity at the base of the Arctic snowpack and the high values at the top. We hope this will convince the Reviewer (and the Editor) that the problem is not our use of the model. The problem is that current detailed snow physics models (which include SNOWPACK and Crocus) miss what is arguably the most important process in Arctic snow: the upward water vapor transport due to the huge temperature gradient. Until this process is included in the models, sensible simulations of Arctic snow physics will not be possible. FD has been trying to get the message across for years, but most Alpine snow scientists (and that seems to be most snow scientists) do not seem to realize the importance of this process. We hope these results will help…

—————————— Technical corrections

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

We believe we have discussed this in depth above. Again, density-thermal conductivity measurements cannot give accurate estimates.

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

In the methods section, line 91-92, we have added that "Water content sensors used the manufacturer's calibration for mineral soils and were not recalibrated, which may produce an error of up to 3%."

l 283 "Rise" -> rise

Corrected, thank you.

l 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.

As discussed above, water vapor fluxes are 10 times greater than estimated by the reviewer. Convection effects are also probably greater.

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

Indeed. The fluxes have now been calculated and Figure 12 changed accordingly. This led to what we feel is a significant improvement. Thank you for your comments of this aspect.

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

Changed.

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.

We now mention (lines 78-79) that "Vegetation consists of sedges, graminoids and mosses". In the results section, we also mention "Vegetation was observed to be mostly flattened by snow, with some sedge or graminoids stems still upright, but they did not seem to have impacted snow structure." lines 188-9.

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)

We could not find this in the classification on p. 19 or elsewhere. The lying 8 on p.19 has circles inside. The word "indurated" is mentioned only once in the whole document, in the footnote of p. 17 and there is no associated symbol.

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

Thank you. For snow, we now use height throughout. We use depth for soil.

Caption Fig 3: where there no easy measurements of snow depths around the stations to know spatial variability around?

Sure, we apologize for failing to detail this. We have now added line 202 that "Measurements using an avalanche probe at 236 spots within 200 m of our site on 12 May 2015 showed a mean snow height of 25.3 cm, with a standard deviation of 13.1 cm". We make a similar statement on line 264 for the 2014 season.

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

We hope that the above discussion will convince the reviewer that they are perfectly consistent. This is Arctic snow, not Alpine snow. Errors on measurements are detailed in the text. For example, in line 227, we specify that "the low value can be attributed to a negative systematic error of about 20% caused by the NP method, as described in (Riche and Schneebeli, 2013) and discussed above". Adding error bars would be confusing, as these are normally used for statistical, not systematic errors.

Acknowledgement

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References

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