Reply to Matthew Sturm

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.

In this paper the authors report on the evolution of the thermal conductivity (k_{eff}) of the tundra snow cover of Bylot Island. It is an interesting paper, with a key finding being that the contrast in k_{eff} between depth hoar and wind slab is on the order of 1:10. While this finding is not new, it is a useful piece of information that is neither widely appreciated nor captured by snow models (as the authors show). Overall, I recommend the paper be published….but not until it has been shortened considerably. The number of readers who will want to wade through all the detail currently in the paper in order to glean the main points is limited, and as consequence, the impact of the paper will be reduced

Thank you for this overall positive comment. Time will tell what the impact of the paper will be. The paper does not just present snow data but also original soil data, which are currently very scarce in the literature. Furthermore existing monitoring data on soil thermal conductivity arguably suffer from artefacts. Here, by minimizing the heating power of our needle probes, we argued that we limited some artefacts, and we feel that this adds interest to our data. Regarding the length of our paper, we feel that establishing links between meteorological events and the evolution of the thermal conductivity of snow deserves detailed discussions. For example, a detailed understanding between wind speed and wind slab physical properties such as thermal conductivity is critically needed. Current parameterization of wind effects is based on limited knowledge (see e.g. (Vionnet et al., 2012)) and at least some modellers will doubtless appreciate these details: some want to move away from simple and inaccurate density-thermal conductivity correlations. Lastly, the reviewer, while recommending considerable condensation, also requests numerous very lengthy additions. Doing both does not seem easy...

One other key point: if the 1:10 contrast in k_{eff} is the big take-away message of the paper, then the authors have missed an opportunity to discuss a key aspect of how tundra snow functions, in a way that is convolved with this 1:10 contrast. As seen in the authors' Figures 2 and 7, the relative percentage of depth hoar vs. wind slab controls the bulk thermal conductivity of tundra snow covers to a large extent. The number of windstorms, the relief of the underlying tundra tussocks (inter-tussock depth hoar), and the sequence of snowfall events all combine to determine what these percentages will be, hence how conductive the snow cover will be. This is a sensitive balance, one that could easily change if the climate, wind regimes, and snow-up dates, change. Discussing this would be valuable and for those readers that this paper aims at possible new information.

Such a discussion would deserve a paper on its own, and in any case this is not compatible with the considerable text condensation recommended above. We nevertheless address this issue in section 4.1 of the discussion, and mention how *threshold effects will lead to the abrupt transition between depth hoar and wind slab (lines 375-379). Thresholds may be reached by a sudden snow accumulation episode, whether caused by precipitation or wind. Note that the Reviewer stresses the tussock-type topography, which is frequently encountered on the Alaska North slope where he has done extensive work. This does not exist here, although hummock microrelief exists but not at our very site, and we therefore cannot discuss that here.*

<u>Other specific comments follow:</u>

1. The authors have a tendency to cite new rather than seminal work, and philosophically, I find this distressing. It leads to the field "forgetting" facts and findings, then these have to be rediscovered. For example, that there are large amounts of organic matter in the permafrost was well known and reported on in the 1960s and 1970s: see Lachenbruch and Ferrians among other authors. A similar comment on references in the snow literature.

We agree with the Reviewer that we do not mention many seminal references. However, for the sake of concision, this was intentional and there are several reasons for this. For example, in the case of organic matter in the permafrost, we feel that this aspect, although important for many purposes, is not critical for our work, and we therefore do not wish to review it, even shortly. We therefore feel that a single recent (and therefore more complete) reference (Hugelius et al., 2014) is sufficient to inform the reader on the subject. Readers interested in more detail can look through the very many references of that paper. Regarding seminal snow references, we agree that there are many seminal references relevant to this work such as the work of Akitaya and in particular (Akitaya, 1975). However, again, our purpose is not to review depth hoar studies. In fact, Akitaya's work is not very relevant to Arctic depth hoar, as the Reviewer subsequently mentions, and we prefer to cite more relevant work, which by the way we find just as seminal, e.g. (Sturm and Benson, 2004; Sturm and Benson, 1997). And again, we are urged to remain concise and try to do so. Regarding soil thermal conductivity, we do cite seminal work, such as (Penner, 1970), line 138.

2. Line 117: Convection in snow. This was a lot of attention paid this topic in the 1980s and 1990s. The authors should see and cite:

Powers D, O'Neill K, Colbeck SC (1985) Theory of natural convection in snow. Journal of Geophysical Research 90:10641-10649.

Sturm M, Johnson JB (1991) Natural convection in the subarctic snow cover. Journal of Geophysical Research 96 (B7):11657-11671.

Sturm M (1991) The Role of Thermal Convection in Heat and Mass Transport in the Subarctic Snow Cover. USA-CRREL Report 91-19.

Indeed, we are well aware of these references. However, those papers deal with natural convection in the snow cover. Here, we only briefly discuss convection induced by the *heating of the needle, which in this case causes an artefact. We therefore believe that natural convection is not directly relevant to this discussion and again, to remain concise, we feel that it is not essential to discuss it here.*

In response to a comment by Reviewer 3, we invoked natural convection to explain the irregular collapse of depth hoar and cite (Sturm and Johnson, 1991), line 387.

3. Line 151: Alternate methods of measuring thermal conductivity: It is not clear that guarded hotplate methods would produce more accurate or appropriate values $k_{eff.}$ ---just different. While the dynamic method of the needle probe has some problems (pointed out by the authors) so too do steady-state methods. For example, real snow covers rarely, if ever, develop a steady-state non-varying temperature gradient. Also, when snow is subjected to uni-directional gradients, the thermal conductivity evolves through metamorphism, leading to a varying value of k_{eff} (or alternately, such mild gradients have to be imposed that the resultant tests are equivocal). I suggest altering the statement to indicate that the alternate methods are impractical, and *may not even be more accurate*.

This is currently a very controversial topic. Our understanding of this issue differs from that of the Reviewer. Clearly, it would be in our interest to be able to state that needle probes have no disadvantage relative to other methods. However, we feel that the recent work of (Calonne et al., 2011) and of (Riche and Schneebeli, 2013), which we cite in our paper, fairly convincingly point to a systematic error in needle probe measurements. We discuss this in detail in our earlier work (Domine et al., 2015). We briefly reminded the main important points of our earlier discussion lines 142 to 161. The Reviewer points to possible problems with steady state methods, and he is certainly correct: no current method to measure snow thermal conductivity is fully satisfactory, but we feel that current evidence does point to more problems with the needle probe method, which we must mention for full information of the reader. Fig. 6 of (Riche and Schneebeli, 2013) nicely sums up some aspects of the problem. By the way, we are not sure to understand the argument that natural snow covers do not develop a steady state temperature gradient. Perhaps the Reviewer is suggesting that steady state method may be used for thermal conductivity monitoring, but we probably all agree that this is not possible.

4. Line 156: Do you have to worry about pore water migration in measuring thermal conductivity of the soil using a needle probe?

This would be the equivalent of convection when measuring thermal conductivity in snow. We have never seen this aspect mentioned and do not expect it to be important given the very low permeability of soils. In any case, it is certainly much less important that problems due to melting, which we discuss in detail in section 4.4.

5. Lines 185 and 265-270: ***Indurated depth hoar***. I would like to see a more comprehensive discussion of this snow texture. I believe the lead author and I discussed this snow texture when working jointly on snowpits in Alaska about a decade ago. He is correct that no formal symbol for the material exists in the International Classification,

but that arises in part from the fact that I don't think the committee charged with revising the classification believed such a material existed. The symbol I have been using for almost 20 years for this type of snow is a combined symbol of wind slab (black circle with a slash) and depth hoar (chains of grains or just cups). This is not that different than the one introduced in the paper, and it would be good to mention both as many of my students continue to use our older symbol. In addition, Carl Benson and I described this texture in our 1993 paper the phenomenon, though it was a few years later I introduced the term "indurated":

Elsewhere wind slabs are adjacent, one on top of the other. As the winter progresses even dense wind slabs can begin to metamorphose into depth hoar. We have observed wind slabs as dense as 0.35 g cm metamorphose into depth hoar by the end of the season.

Before introducing that term I corresponded with Dr. Akitaya (arguably at the time *the* expert on depth hoar) regarding it. He had never seen this texture in Japan because the temperature gradients are too low to produce it.

Indeed, FD does remember well this March 2004 discussion with the Reviewer near Barrow, Alaska. Indurated depth hoar is frequent in the Arctic and its absence in the international classification is a very big problem. FD mentioned this to Charles Fierz when he circulated drafts of the classification, but that steered no interest in the committee. We therefore fully agree that many world snow experts are familiar with mid-latitude snow, but have seldom seen Arctic snow, even though it is more prevalent. There is therefore a need to fully describe indurated depth hoar. We have done that lines 189-201, quoting the most detailed description we found, from (Derksen et al., 2009) and (Domine et al., 2016). We also discuss that indurated depth hoar can form not only in wind slabs but also in refrozen layers, and propose 2 symbols to differentiate both types. Additional details are given lines 302-308. We also mention the works of (Hall et al., 1991) and of (Sturm et al., 2002), who mentioned a snow type that was presumably depth hoar, although not named specifically. We also mention (lines 303-309) the symbols used by those authors but we feel the symbols we propose are more logical and above all allow differentiating whether indurated depth hoar formed in wind slabs or melt-freeze layers

6. **Temperature Gradients**: (Lines 313-317) In order to turn wind slabs into indurated depth hoar, strong gradients are needed (see authors Figure 11). While the gradients a Bylot Island are super-critical (> 25°C/m) for much of the winter, they are less strong than I would have expected. I attach comparable data from Alaska's North Slope. Not only are the gradients stronger than those in the paper, but they start earlier in the winter. In a second graph, I have smoothed the data (48 hour running average), as the authors have done. The result is noticeably milder. I suggest the following changes/additions: a) don't smooth the gradients. In 48 hours a lot of metamorphism can take place. Smoothing makes the graph cleaner but less "physical" as far as metamorphism is concerned, b) show the critical gradient….whether that of Marbouty or Colbeck or Armstrong. It gives readers an appreciation of what drives the changes, c) consider computing something like an integrated metric called "time under a strong

gradient" or the like. Right now the paper mainly focuses on the agreement between the thermistor and NP values, but that misses the BIG point of what is actually driving the physics. And while Lines 325-330 provide the vapor gradient (and state that it can grow depth hoar), the values are really meaningless unless they are translated into something like a growth rate. Consider as an example computing how fast these vapor gradients could grow a thin sheet of ice…like the skirt on depth hoar cup. That would make the values mean something.

We have replaced the smoothed by the unsmoothed gradient in Figure 11. Values are a little bit greater, but not much: the highest value is 104 K m^{-1} vs. 87 for smoothed values. Gradient values are then just lower than in Alaska 25 years earlier. Higher values were probably reached earlier in the season but we need more than 12 cm of snow to measure it, since this is the level of our second NP. Our thermistor at 7 cm height would have given us the early season data, but as mentioned in the paper, the cable was chewed by a fox. This is detailed lines 355-361.

Following also the recommendation of Reviewer 3, we have calculated the water vapor flux, which is in fact what drives metamorphism, rather than the temperature gradient. We discuss threshold values and their relation to snow accumulation (lines 376-380). However, we do not wish to insist on "critical gradients" as we believe these have little physical meaning for Arctic snow. The real important physical variable is the water vapor flux, and we focus on that (Figure 12 and lines 366 ff.). The "critical gradient" was meaningful for Alpine and temperate snow, when the warmer temperature of the gradient was always close to 0°C. In Arctic snow, the warmer temperature can be as low as -35°C (Figure 3) so that the flux here, for a similar gradient, will be 15 times lower than if the warmer temperature were 0°C. We therefore think that reasoning on gradients for Arctic snow is misleading and we do not wish to follow that avenue.

8. **Soil to Snow Fluxes**: These have been measured during depth hoar metamorphism. See:

Trabant D, Benson CS (1972) Field experiments on the development of depth hoar. *Geological Society of America Memoir 135:*:309-322.

Santeford HS Snow Soil Interactions in Interior Alaska. in Modeling of Snow Cover Runoff, USACRREL, Hanover, NH, pp. 311-318.

Sturm M, Benson CS (1997) Vapor transport, grain growth and depth hoar development in the subarctic snow. Journal of Glaciology 43:42-59.

We mention (Sturm and Benson, 1997) line 409 and compare our soil water loss value to theirs line 413. The other 2 references could not be found.

9. Line 341: There is a large literature on depth hoar and subnivean animals. Possibly the best starting point is W. O. Pruitt's 1957 paper: Pruitt Jr, W. O. (1957). Observations on the bioclimate of some taiga mammals. *Arctic*, *10*(3), 130- 138.

Indeed there is, but further developing this aspect is not compatible with condensing the manuscript and with Reviewer 3's recommendation to even remove this section. We therefore limit ourselves to citing the work done at our research site, adding the recent results of (Fauteux et al., in press) which allow us to relate in a very preliminary manner our observed snow properties with lemming populations (lines 433-438).

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