

Response to Reviewer 3

We thank the reviewer for his useful comments and for giving us the opportunity to improve our paper. Our responses are embedded in blue italics.

General Comments

This paper describes a carefully designed and executed experiment which successfully acquired time series of needle probe snow effective thermal conductivity (Keff) measurements. A series of post-processing steps convincingly isolates the most accurate sampling periods for measurements with and without convection, which allows robust error analysis and a clear determination of needle probe Keff underestimation compared to other techniques. While relatively high uncertainty and bias no doubt exist in needle probe measurements compared to more advanced techniques, this study has shown a practical approach for use in remote areas, with the added benefit of providing time series measurements. This is a rigorous study, and the paper is clearly written. I suggest only editorial corrections and these clarifications:

1. Page 1637 line 21: I'm sure there is a good practical reason but why during the 2012/13 season were the needle probes not inserted into the vertical profile earlier in the season? Could the removal of the snow block to insert the probes be avoided simply by inserting the probes earlier?

What happened is simply that a manufacturing delay caused the probes not to be delivered in time for our summer campaign, when the instruments were deployed. We therefore had to insert them during our winter campaign. We will simply refer to "logistical difficulties in the revised version". These just happen when working in the Arctic, just like the temporary system failure the second year.

2. Page 1638 line 22: "We found that using the cooling curve added little value to our data, so our work focused on treating the heating curve." I suggest adding a short explanation to clarify this statement. Did the cooling curves add little value because the results were similar to the heating curves? Or was there some ambiguity in the interpretation of the cooling curves based on the procedure outlined in Figure 3?

Deriving the thermal conductivity from the cooling curve is less accurate than from the heating curve, as detailed in Morin et al. (2010). Adding a low accuracy measurement to a higher accuracy one therefore has little interest. This will be clarified.

3. Page 1639 line 28: briefly describe the measurement of specific surface area (SSA). I assume these were integrating sphere and laser measurements? Not clear what a "parallelepipedic" density cutter looks like. . .

Since we never use SSA data in this paper, we choose not to mention it in the revised version. We will replace "parallelepipedic metal sampler" with the more common term "box cutter".

4. Page 1641 line 5: I suggest adding further details on the manual analysis of the heating curves. Specifically, the separation of convection from non-convection cases is not clear. How was the "best time interval" selected?

A similar comment was made by R. Essery, confirming that we need to provide more details. In fact, a visual examination of the plot readily detected convection. The "best" time interval was determined visually by selecting the largest interval that remained linear. Obviously, there is some arbitrary character to any manual or visual treatment, but as in any field, experience also helps.

The fundamental argument is that a small change of slope is readily detected visually, and we will mention that in the revised version.

5. Page 1650 line 21: Discussion of meteorological differences between years (previous paragraph) and metamorphic conditions would be strengthened by more quantitative information on the snow microstructure profiles, especially at the needle probe locations through both seasons. Can SSA information be added to Figures 4, 5, and/or 6? Only three manual measurement periods were conducted through the 2 seasons, so some supposition is necessary but were the changes in K_{eff} through January to April 2013 (not observed in 2014) due to the changing vertical position of the needle probes relative to the total snowpack depth (and related metamorphic processes due to increased late season snow depth) which remained comparatively consistent in 2014?

SSA cannot today be measured automatically. It was only measured during field campaigns, and even then, our focus was on thermal conductivity and density so that we have fewer SSA than k_{eff} data during our campaigns. At the heights of the NPs, SSA was between 10 and 20 $m^2 kg^{-1}$, usually closer to 10, so that these small variations in fact yield little exploitable data on microstructure. Adding data to Figures 4 and 5 would add points to one date on Figure 4 and to 2 dates on Figure 5. Given that there are spatial variations on SSA and that of course SSA was not measured at the very NP site to avoid disturbing the snow, we do not feel able to draw any useful conclusions relevant to this paper from our SSA data. Likewise, we feel that adding SSA data to Figure 6 would add little value to our paper. Our intended use of our SSA data is for a subsequent modeling study, when we compare simulated and measured SSA values, in an attempt to detect the impact of vegetation on SSA. Thus, our SSA data will be available, but we feel that it will be best detailed in a subsequent paper. We prefer to keep our focus on thermal conductivity. Regarding possible changes in needle position, the positions were fixed. Figure 1 shows that they are inserted in a fixed manner in a vertical pole. As detailed in our paper, we feel that the changes were caused by the different meteorological (and therefore metamorphic) conditions.

Editorial Comments

Page 1635 line 5: change to “. . .that it is meant to. . .”

Done, thank you.

Page 1637 line 6: not clear what is meant by “forest tundra”. Are you referring to forest patches surrounded by tundra or the forest to tundra transition?

We are referring to forest patches on tundra, which is the published definition. We have added a reference to clarify that.

Page 1640 line 2: change to “. . .cleanly breaking. . .”

Sure

Page 1642 line 11: change to “. . .to ensure measurement quality. . .”

Sure

Page 1642 line 21: correlation coefficient = r ; coefficient of determination = R^2
We will change to “squared correlation coefficient”

Page 1645 line 21: incorrect reference to Figure 8.

We meant Figure 6, sorry.

Page 1645 line 25: instead of referring to depth hoar as ‘very soft’ I suggest using a term like ‘low density’. Instead of ‘hard’ depth hoar, would it be correct to describe it as “icy depth hoar” or depth hoar mixed with melt/refreeze clusters in 2013-14?

Indeed, this is the case and we will modify the text accordingly. However, we will also maintain the term soft and hard, and we feel these are useful descriptors.

Page 1647 line 15: suggest modifying ‘almost systematically’ to ‘systematically’.

This will be changed.

Page 1648 line 26: is the 29% additive from the terms in this and the previous sentence?
If so, there appears to be a rounding error.

*No, it is the square root of the sum of the squares of all errors, leading to a total error of 28.88%.
This will be specified in the revised version.*