Reply to Richard Essery

We thank Richard Essery for his positive comments and for taking the time to help us optimize our writing. Our responses are embedded in blue italics.

Domine et al. describe an important set of measurements of the thermal conductivity of snow in shrub tundra and give a useful discussion of how understanding of the processes that determine conductivity could be improved. My comments are restricted to some requests for clarifications and minor corrections
Clarifications:
page 1634, lines 17-21
These are important comments on the impacts of shrubs on snow, but none of them were directly "observed" in the work presented here.

We will change the wording to : "We conclude that shrubs have very important impacts…"

1635, 4
Insert the units of F and k_eff here.

Done

1636, 8
Note that Myers-Smith and Hik (2013, doi:10.1002/ece3.710) did not find evidence for increased nutrient cycling in shrub manipulation experiments, possibly because soil temperatures were decreased in summer due to increased shading by shrubs.

Indeed, this is a very interesting contribution to an active (and arguably controversial) research topic. Our paper is probably not the place to engage in this debate, so we prefer to remove any reference to nutrient cycling. We will just mention the thermal effect in winter and the modeling results of Gouttevin et al.(2012).

1640, 1
Was the sampler really a parallelepiped? A triangular prism is a more usual shape.

Yes indeed, it was a parallelepiped, not a wedge cutter. A more commonly used terminology seems to be a box cutter and we will use this term, and add the reference to Conger and McClung (2009).

1641
More information is required on the manual analyses. How was convection determined? – a subjective judgement of curvature in the profile before 100 s or a pre-set threshold? Was the distinction always clear-cut? How was the best time interval selected manually? For the profile in Figure 2a, it looks like a 20-50 s interval would have given the same conductivity, but Table 2 suggests that this was not always the case for profiles without convection. Showing an example of a non-convective profile that requires a delayed time interval would help the reader to see what is happening in these cases.

Convection was determined visually and in almost all cases, it was obvious, as a change in slope of 5% is readily detected visually. We will detail this in the revised version. The example of Figure 2b is well representative, although we do have plots where the curve is not as sharp. The impact of convection is important, as shown in Tables 1 and 2, so it is then critical to use the correct time interval: 20-50 s. When there is no convection, the ideal plot is linear after the initiation period,

and the difference in slope between the 20-50 s and 40-100 s time intervals are difficult to detect visually. This is reflected in Tables 1 and 2, where the differences between both time ranges is 2.7% in Table 1 and 4.6% in Table 2. In many cases, what is selected manually is a fairly large time interval. In some cases, however a slightly better fit was obtained by selevcting a time interval starting at 40 s rather than 20 s. Therefore, for an automatic procedure, this interval was chosen in the non-convective case. It is clear that in the non-convective cases, a careful selection of the best time interval is a second order improvement and is not critical. We nevertheless chose to reduce all sources of error, however small. This will be detailed in the revised version. This will also address comment 4 of reviewer 3.

1645, 21
There is no Fig. 8.

We meant Figure 6 and will change that.

The writing is excellent, but I have but I have a number of minor corrections to suggest.

Thank you for taking the time to correct the paper. The minor corrections will be made.